THE ANNALS

OF

ELECTRICITY,

MAGNETISM. AND CHEMISTRY;

AND

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AND ASSISTED BY GENTLEMEN EMINENT IN THESE DEPARTMENTS OF
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VOLUME V.

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v.

THE ANNALS

OF

ELECTRICITY, MAGNETISM,

AND CHEMISTRY;

AND

Guardian of Experimental Science.

On the Identity of the Ordinary and Voltaic Electricity. By
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1. The views of electricians which have been so long enter-
tained with regard to the identity of the electricities, the gal-
vanic, frictional, and magnetic, appear to have commanded and
obtained almost universal assent. The experiments, however,
upon record, which I have hitherto perused, such as those of
Dr. Wollaston, and others, and the more recent ones of Dr.
Faraday, although manifesting in a high degree the analogy of
galvanic and ordinary electricity, are still not perfectly decisive,
and for reasons which will be hereafter adduced, have im-
presed me with the necessity of still further carrying on the
inquiry. I doubt not but to many minds the experiments of
the illustrious individuals above named may appear finally con-
clusive and satisfactory ; but if there still remain those who are
discontented, unless the two fluids so tested be submitted to
exactly similar circumstances, be tested by indeed the same
apparatus, effect chemical and other changes in precisely the
same quality of materials, evince the government of the same
laws in its conduction, insulation, polarity, &c., (see 41. 42. 44.
45.) and be indeed moulded into a perfectly identical form of
fluid ; if in the substance of the ensuing paper experiments be
found which can add satisfaction to, and set the matter finally
at rest in the minds of the latter class of individuals, this, and
this alone, will fulfil the objects of the following investigation.

2. On first surveying the distinctive characters of the two fluids, we find a wide unbounded contrast, and an unlimited diversity of properties.   On the one hand our attention is directed to the roaring and tempestuous burst of fluids from the most extensive, immense, and immeasurable distances, polarizing, as it were, heaven and earth at the same moment, and traversing with. fleetest strides the remote boundaries of the terestrial and celestial empire ? devastation, destruction, and ruin, marking frequently the footsteps of this subtile agent ; whilst the other is silently, slowly, and almost imperceptibly executing the most beautiful, useful, and interesting productions of the material universe. , It appeared that to attempt the establishment of identity between fluids of such opposite characters, it would become necessary to endeavour to mould the one under investigation, as nearly as possible, to the characters and properties of the opposing kind.

3. The subject which appeared least satisfactory, and which has induced me more particularly to commence a further inquiry, is, the decomposition of water by ordinary electricity ; an experiment which it is well known has so frequently been accomplished by the galvanic fluid.   For although this decomposition had been effected by many individuals, I am not aware that the gases were ever collected in a completely separate state, and submitted to a perfect analysis, and if we contemplate the manner in which the experiment has generally been performed, it has been without any reference to the character of the fluid imitated, and, as may be gathered from some of the following experiments, with mostly unsatisfactory results. (38. 45.) I doubt not, therefore, I shall be excused the boldness of my attempt, in *endeavouring* (although the subject has failed in the hands of the eminent) to establish the analogy of these fluids by experiments which have at all times been looked upon as possessing the most decisive character.

4. Finding, on the onset of my enquiry, the *tension* of ordinary electricity, in comparison with that of the most powerful voltaic battery, to be very much superior, and that this property may be instantly diminished (as will be shortly exhibited) (see 14) by charging a Leyden jar, I determined upon at once drawing out a statement of the properties of the galvanic and frictional electricities, to direct and guide me in all my future experiments.

5. Galvanic electricity, for instance, as existing in a common battery, or single pair of plates, usually presents the following properties or chafacters.:—1. It is produced in a *continuous stream* of one or two fluids of opposite kinds, one positive, the other negative.—2. The two streams or currents of positive and *negative* electricities have a constant and mutual attraction for each other, there being a continual tendency of the *positive*

fluid *to pass to the negative* body, and *to no other.*—3. The galvanic fluid is subject to the influence of induction, or polarization of the most *contiguous* kind, being produced by the assumed oppositely electrical conditions of the atoms of matter when in *absolute contact*, and hence the *tension* of this fluid or its capability of passing through a non-conducting medium, is *exceedingly limited*, and may also be termed *atomic.*—4. It is *conducted* badly by metals, and scarcely at all by water.—5. Its *attractive and repulsive* powers are also of the *feeblest* kind, for without a considerable number of alternations of plates, gold leaves cannot be diverged, nor any effect produced upon the pith ball or other electrometers.—6. Its physiological effects are very feeble in proportion to the quantity of the fluid, nor are they experienced at all unless contact of the poles be made and broken.—7. The poles of this fluid do not require insulation, a consequence of No. 2 and 3.—8. Its *quantity* is exceedingly great, and far surpassing the amount of fluid generated by an ordinary electrical machine.—9. It is incapable of being accumulated in the shape of charge; one instantaneous contact with the coating of a Leyden jar being productive of as great an amount of electric fluid as the continuation of the same for any length of time, its quantity being alone obtainable in a continuous current.—10. By increasing the polarizing influence by a given number of alternations, *tension* becomes apparent, is rendered evident by the passing of a spark and other effects, and by still further augmenting this influence, a greater degree of tension is obtained, altogether, however, much inferior to the effects of this kind in the ordinary electricity.

6. If we now proceed to contrast these peculiarities of the galvanic fluids with the modified forms of fluid under which frictional and aerial electricity appear, we find that the fluid termed lightening, and that which passes in sparks from the machine, differ most widely from the properties herein described. Now it is singular that these very forms of electricity should have been almost the only kinds made use of in the investigations of philosophers in imitating the galvanic action, and it is also more singular, that in all the experiments (except the first) no negative pole was ever made use of which had a mutual decomposing dependence with the positive, and, consequently, bearing not the slightest resemblance to the properties of the galvanic fluid, especially as described (No. 5. 2.)

7. Dr. Wollaston's experiments were performed chiefly by *sparks* from the machine of various dimensions, and in one instance by *current alone*, but at the same time with only *one* pole, and, says he, " the *appearance* of two currents of air may also be imitated by occasioning the electricity to pass by fine points of communication on both sides of the water, but, in fact, the resemblance is not complete, for in every way in

which I tried it, I observed that each wire gave both oxygen and hydrogen gas, instead of their being formed separately, as by the electric pile." Perhaps it is unnecessary to state that here no mention is made of connecting more than one pole to any electrical body whatever.

8. In 1807 Sir H. Davy immersed a guarded platina point connected with the machine in distilled water, and dissipated the electricity from the water into the air by moistened filaments of cotton. In this way it is stated that he obtained oxygen and hydrogen separately from each other.

In 1832, the late Mr. Barry communicated a paper to the Royal Society, stating to have decomposed water by electricity, and to have obtained separate oxygen and hydrogen, from a kite-string in communication with one wire or pole, whilst the other wire was merely connected with the ground.

10. In the experiments of Dr. Faraday, (I believe our latest authority upon this subject) as stated in his series of experimental researches in electricity in 1833, in the decomposition of water, it is stated, (329) "if with a constant pair of points the electricity be passed from the machine in sparks, a certain portion of gas is evolved, but if the sparks be rendered shorter, less gas is evolved, and if no sparks be passed, there is scarcely a sensible portion of gas set free. On substituting solution of sulphate of soda for water, scarcely a sensible quantity of gas could be procured, even with powerful sparks, and nearly none with the mere current. (330.) "When what I consider the true effect only was obtained, the quantity of gas given off was so small that I could not ascertain whether it was as it ought to be, oxygen at one wire and hydrogen at the other."

11. As the other decompositions performed by this celebrated philosopher appear to have been accomplished by attaching the positive pole to the prime conductor, and the negative to the "discharging train," (321) it is not improbable that the decomposition of water was attemped by him in the same manner, but the fact is not stated. With regard to the experiment of Sir H. Davy, Dr. Faraday adds in a note to (471) series 5, that "it does not remove any of the objections I have made to the use of *Wollaston's apparatus as a test* of true chemical action;" of the one quoted by Barry, he remarks, (339) "nor have any of the numerous philosophers who have employed such an apparatus obtained any such decomposition either of water or of a neutral salt by the use of the machine;" also, (342) "Mr. Barry's experiment is a very important one to repeat and verify. If confirmed, it will be, as far as I am aware, *the first* recorded case of *true* electro-chemical *decomposition* of *water by common electricity*." A very similar remark is also made by him. (359.)

12. It is apparent from what is recorded of these experiments

that no attention was ever paid to the properties of galvanic
electricity described (No. 5. 2.) viz., of providing poles depen-
dent one upon the other for decomposing power and action.
The positive pole was at all times connected to a positively
charged body, but the negative pole was attached to matter,
either in the natural state of electrization (speaking of it as a
mass) as in the case of the "discharging train" used by Dr.
Faraday, the "ground" by the late Mr. Barry, the "air" by Sir
H. Davy, &c., or in a negative state to which the fluid from the
positive pole had no special tendency to pass *the negative
conductor*, (see now 45.) of which, nevertheless, we have, I
believe no positive statement that it was ever used at all. Dr.
Faraday also remarks, concerning the poles used by Wollaston,
"that the poles, or rather points, have no mutual decomposing
dependence may be shewn by substituting a wire, or the finger
for one of them, a change which does not at all interfere with
the other, though it stops all action at the charged pole."

13. On examining the modifications of the electric fluid de-
rived from the machine, or lightning, we find it existing in two
very opposite conditions, determined by the distance of polariz-
ing bodies, or the thickness of the insulating medium through
which the polarization is effected. Thus lightning and the
sparks from the machine (the kinds generally employed by
electricians in imitating galvanic decomposition) are subject
only to *very distant* polarization, (see No. 5. 3.) and may be de-
nominated *free electricity*, from their capability of passing to
any body in their vicinity, (see No. 2,) having no tendency to
pass to one conductor more than another, except from conti-
guity, being in an *interrupted* stream or streams (see No. 1,)
possessing the most *unbounded tension*, (see No. 3,) powerful,
attractive, and repulsive powers, (see No. 5,) conducted excel-
lently by metals and with great facility by water, (see No. 4,)
Physiological effects only when the current is broken or inten-
sity very great, (see No. 6.) Insulation essential to its very
existence, (No. 7.) Its quantity is exceedingly small, (No. 8.)
but can be accumulated to any amount. (No. 9.)

14. The *tension* obtained by the influence of this polariza-
tion gives to the fluid *considerable mechanical force* and *power-
ful momentum*, and by the expansive character thus obtained,
the pressure of the atmosphere, and of water (when passing
through that fluid) is more forcibly resisted and sustained. By
this means an exceedingly *minute* quantity of electricity may be
rendered visible by expansion (17. b) and may appear to the
observer as fluid of great quantity, and "high intensity" (17. a)
The influence by which this modification of electricity is go-
verned, I have named *remote polarization*, and when this kind
of electricity is submitted to the action of *polarizing* influence
of the proximate kind within its immediate neighbourhood,

as in the *coatings* of a *Leyden jar* or other thin insulator, we find that the spark from the prime conductor which was capable of passing one inch, under these circumstances, will now only pass one-twentieth of that distance. The fact is instantly shewn by first taking a spark from the prime conductor, and observing the passing distance and the height of an electrometer in connection with the machine. If now a Leyden jar of any kind be connected to the conductor, the electrometer instantly falls, and the passing distance of a spark will be reduced to, at least, one-twentieth of its former magnitude. I find, also, that with the interposition of electrics of various thicknesses very different effects are produced. Thus with a passing distance of one-tenth of an inch and a very thin laminæ of mica, *three* discharges only passed in one turn of the machine, the electrometer being at 11 of an arbitrary scale. With glass one-thirteenth of an inch thick—and the same passing distance—31 discharges occurred in one turn, the electrometer being at 16 degrees. (See also 23. 24. 25.)

15. The *tension*, therefore, is by this means *considerably diminished.* 1. The production of fluid under this influence in a continued stream (5. 1.) presented many difficulties, but will be described further on in the paper. 2. The mutual attraction of one pole for the other, when attached to these polarized coatings or surfaces, and the tendency of the fluid of the positive to pass to the negative and no other, (as in 5. 2.) are very manifest. 3. The attractive and repulsive powers are also diminished. 4. It is much less easily conducted by water, and even metals, than any quantity of electricity under remote polarization. 6. The physiological effects are feeble where only a current exists, but manifested by making and breaking contact. 7. The poles do not require insulation, in consequence of No. 2. 8. The quantity may be rendered greater than that from the machine alone, (of which hereafter.) 10. By increasing the polarizing influence by a given number of alternations of jars, tension is considerably augmented.

16. From the knowledge of the extensive modification of the ordinary electricity thus effected by inductive influence, I have been induced to divide the *polarization* of electricity into three kinds: 1, The contiguous, or atomic; 2, The proximate; and 3, The remote.

17. In passing, it would be well to define the meaning of the terms tension and intensity, which I have adopted in this paper, since it appears that very different constructions are commonly appended to these important terms. (a) I observe Dr. Faraday speaking of tension and intensity (series 12—1370) as of *one* and the *same property*, (see index tension 1370, intensity 1370,) also series 4, (392) ordinary electricity termed "electricity of *exalted intensity*," and (419) electricity of "*high tension*." I

find in another publication, Henley's electrometer described as for the purpose of measuring "the intensity of the electricity." I also meet with in another work, that one of the principal circumstances in which *voltaic* differs from *ordinary electricity* is " *the very low state of intensity* in which it exists in the former when compared with the latter." Now the terms appear to me to denote two electrical conditions very different from each other.(b) By the term tension (or any meaning I can attach to it) appears to be understood polarizing power, transferring force, capability of passing, a forcible stretching or expansion *from the centre towards the circumference ;* and this appears to me to apply admirably to the nature of ordinary electricity, as obtained from the prime conductor, "electricity of high tension." (c) But by the term intensity I should understand condensation, or concentration *from the circumference towards the centre,* that of large quantity reduced to a very small compass, thus a burning lens condenses the rays of the sun, so as to bring them to a focal centre, where the highest degree of intensity of heat is experienced. This property is also exhibited by the *quantity* voltaic battery, where a fine platina wire is interposed in the current, it becomes immediately red hot, and is retained at a red heat so long as the battery continues in action. The same may be said of the passage of an ordinary electrical battery charge, through such a wire, a large quantity of fluid is in this instance also compressed into the narrow limits of a small conductor, and exhibits its intensity by rendering the wire momentarily red hot. If the quantity of fluid be augmented, or wire diminished, a greater intensity of effect will be the result, as shown frequently by fusion of the conductor.

18. From this view of the subject, therefore, it would appear that intensity has nothing to do with the properties of a spark from an ordinary electrical machine. With a given quantity of electrical charge, the tension (or capability of passing) from the polarizing to the polarized body or surface, will be in ratio with the distance of the two polarized bodies. (a) Thus the tension (or passing distance) of contiguous polarization is atomic ; of a Leyden jar, under proximate polarization, the distance of the thickness of the glass ; of the fluid from the prime conductor, under remote polarization, the distance of a body in which it is capable of inducing a given polarity ; of the fluid from a cloud (also under remote polarization) to the earth, &c. (b) Now the tension of all these electrical forces under similar circumstances, will be continually the same, according to the source whence each is derived. (c) Thus, if electricity be excited by a small machine, a spark of given dimensions will be the result in a given time, and it will be impossible to obtain from the same machine, under the same circumstances, a spark of greater magnitude. If a large machine also produce a given spark, a similar one may *cæteris paribus* at any time be obtained.

(d) Charge a jar by means of the spark so obtained, which will now become its source, and the result will be a spark as just exhibited diminished at least to 1-20th of its original dimensions, and it is impossible, by any means, to obtain afterwards the fluid so polarized, in dimensions greater than that to which it is now reduced by polarization. (e) I doubt not, but fluid submitted to the influence of atomic polarization would, under the same circumstances, give *atomic distances*, which afterwards could not *cæteris paribus* be increased. (f.) Combine a number of electrical machines, and an increase in the *quantity*, and to some extent in the tension will be the result. (g) But the method by which the passing distance or tension of ordinary electicity is most considerably increased, *after* proximate polarization of that fluid, is by the combined influence of a number of polarized surfaces. If six insulated jars, connected by conductors, so that the outside coating of the first shall communicate with the inside of the second, outer of second with inside of third, and so on throughout the series, be charged, by connecting the first jar with the prime conductor, the outer coating of the last being in connexion with the negative conductors, an explosion from the inner coating of the first to the outer lining of the last will take place about *six times as far* as from any two surfaces of a single jar by the ordinary method of discharging. Tension, therefore, is diminished in ratio with the *degree* of contiguity of polarization, increased by quantity, and greatly increased by reciprocal polarization.

19. In pursuing my experiments, I obtained permission to make use of the large plate machine in the Manchester Royal Victoria Gallery; a plate whose diameter is about five feet, having four rubbers, and considered a first rate machine; sparks of ten inches in length may be readily obtained from the prime conductor when in good order. On attempting to decompose water by the current alone (as hereafter) with this machine, I was exceedingly surprized to find little, if any, more gas eliminated from the poles than could be obtained by my own machine, the cylinder being only fourteen inches in diameter. I have since been led to the conclusion, *that probably the thickness of the glass* in the electrical machine, determines the passing distance, or tension, momentive force, and size of spark, (see 14.)

One surface of the glass receiving, by friction, an encreased quantity of electric matter not naturally belonging to it, induces polarization of the "molecules" (Dr. Faraday) throughout the whole substance of the glass, and causes the developement of free, undisguised fluid on the opposite surface. If the electricity be now collected from the latter surface, the electric would be left in the condition of a charged Leyden jar; one surface positive and one negative.

But if the fluid be collected from the charged surface alone, (as is usual,) the *disturbed fluid* at the opposite surface, together with the *forced condition of the molecules*, would conjointly tend to induce the removal of the fluid from the charged surface with *a force in ratio with the* amount of polarized molecules, or *thickness of the electric* which they compose. (see 14. 19. g.)

20. Apply these ideas and facts to voltaic electricity. It is produced and stil continues under the influence of atomic polarization. Take one pair of plates, for instance, the passing distance is atomic. By increasing the size of the plates, as in the quantity battery, or adding a number of plates in which there shall be a connection of all the zinc together, to form one pole, and of all the copper plates to form the opposite pole, the quantity *shall be much increased*, and the tension or passing distance *slightly so*. (17. f.) But if (as is well known) a number of alternations of plates be so arranged, that the first positive plate (the copper one) be in conducting communication with the second negative one, (zinc) a non-conducting medium be interposed between the two, the second plate with the third negative, and so on through the series, by which the various alternations are placed in the most advantageous position for aiding and assisting each other, (reciprocal polarization,) a considerable increase of tension, or passing distance, will be the result ; a few alternations will readily manifest the electric spark, and a very large number exhibit electric attraction and repulsion, and produce a brilliant spark, attended with the most splendid exhibition of electric light in existence, and with a tension equal to about 1-30th of an inch in cold, and three or four inches in heated air. (17. g.) With these views I proceeded to the practical application of the subject.

On the Analogy of Ordinary and Voltaic Electricity.

21. Finding that ordinary electricity, under the influence of proximate polarization, approaches nearer in character to the voltaic than any other modification, I proceeded to decompose water by means of an apparatus in which the principle of the Leyden jar formed the most prominent feature. I at first made use of uncoated wires, as in the experiment by M. M. Paets, Van Troostwyk, and Deiman, and although bubbles of gas did make their appearance upon the platina wires, there is every reason to believe them to have consisted entirely of carbonic acid gas, since the water made use of at that time was pump water, and the gas ceased to make its appearance after a very short period. Very shortly afterwards I coated fine platina wires (reduced to a moderately fine point) with glass, by inserting them in tubes, and melting the glass around, and grinding whilst the point appeared, &c., as in Wollaston's experiment. After several trials, I found that one Leyden jar insulated, its

B

internal being in communication with the positive, and its external with the negative conductor, produced the greatest quantity of gas, one pole being connected to the outer coating, and a wire from the other placed at such a distance from the knob of the jar, that the electricity would pass in small and very rapid shocks. By this means I obtained from one pole, on the •14th of July, in about an hour, 2-3rds of an inch of gas in a small tube receiver; the quantity in the tube situated over the other pole did not at the close of the experiment contain near so much gas, but in testing the former by passing an electrical spark through it, it exploded, and left about one-third its original bulk, so that there would be probably (supposing the remainder to be hydrogen) two parts oxygen to seven hydrogen in the whole amount, i. e. (considering the whole to be nine parts) three remained and four hydrogen exploded, with two of oxygen.

22. About the same period I completed an increasing battery upon the principle of the six jars already described, and by a little contrivance of two rotatory rods, A. B., from which projecting wires alternately came in contact with and discharged the whole series, through the two guarded poles by shocks. (1, 2, 3, 4, 5, 6.) By this means a very rapid decomposition of water was effected—two-thirds of an inch of gas in the hydrogen tube could now be produced in from twenty minutes to half an hour, but by passing a spark, the *instantaneous disappearance of nearly the whole amount of gas* in the tube *was the result*, shewing clearly that the decomposition was of mixed gases at each pole. I found, also, that the poles in this experiment had " no mutual decomposing dependence," as stated by Dr. Faraday concerning the decomposition by Dr. Wollaston, and that any other conductor in connection with the outer coating of the jar did not interfere with the formation of gas at the opposite pole; therefore each pole is of itself by shocks capable of decomposing. (See now 43.)

23. In pursuing the subject of proximate polarization, according to the rule of distance already laid down, I was in search for a material of nonconducting properties which would allow the two polarizing surfaces to approach as nearly as possible, and by this means I was in hopes that effects might be produced very much resembling those of the galvanic electricity. Indeed I was inclined at one period to imagine that no decomposition of water identical with galvanic could be obtained, unless we could subject the electric fluid to the atomic polarization. The material which I fixed upon was the thinnest lamina of mica which it was possible to produce by slitting, as is usual in microscopic experiments. I placed a small bit of tinfoil on its upper and under surface and charged it. On testing its physiological powers, I found that a quarter or half a turn of my machine

produced by this means a charge which shook the elbows vio-
lently. The power, indeed, of the machine appeared consider-
ably heightened in contrast with the effects of the thinnest
charged phial I could meet with. The very smallest distance
of break in the circuit (apparently contact) at which a ball
could be placed from the positively charged coating, gave con-
stant shocks and sparks between the balls, so that here we have
an excellent illustration of one of the propositions where it is
stated (14.) that proximate polarization diminishes the tension of
the machine, one-twentieth or more. The prime conductor from
which this was charged will at any period (when in moderate
order) give sparks from two to three inches; but by being in
contact with two almost contiguous polarizing surfaces, the
spark will not pass the 1-100th of an inch until the mica be
charged, and it is only the distance now which the so charged
mica has tension enough to pass which regulates the period of
discharging.

24. By the polarized mica and guarded platina poles, I suc-
ceeded in decomposing water apparently much more rapidly
than by any other means. By an experiment with a given pass-
ing distance of two balls (3-10ths inch apart) the charge of a
jar exploded by one turn of the machine, and with the same
distance (the jar being removed) twenty sparks passed in one
turn with the same apparatus, but a very fine lamina of mica
being interposed instead of a jar, the discharge would not pass
at all, and in six turns the lamina was pierced by explsion and
destroyed. With the mica I obtained *eight* distinct shocks, felt
in one finger of each hand up to the wrist, during one turn—
the passing distance being about 1-50th of an inch.

25. With the Leyden jar instead of the mica, (the jar being
1-20th inch thick) I placed the balls at a distance apparently
imperceptible, perhaps 1-80th inch, and obtained distinct
shocks, and this afterwards by contact, the lacquer of the balls
alone intervening. In this last experiment we learn that polar-
ization of glass can be effected to a much greater distance than
discharge through air; the fluid will more readily polarize to
the distance of 1-20th inch than pass 1-80th, or even the dis-
tance of the lacquer covering the balls.

26. On the 21st of July, I passed 3-4ths inch *sparks* through
the decomposing apparatus, and obtained a decomposition of
water, but on contrasting it with the quantity obtained through
the medium of a small Leyden phial, the discharges occurring
as frequently as the sparks for a given number of turns of the
machine, I obtained about double-sized globules of gas, from
the discharges of the phial, to what were obtained by the sim-
ple spark alone. When the spark was made use of, no *star* ap-
peared upon the apex of the pole, as is always the case when a
Leyden discharge is made use of. Upon attempting to decom-

pose by the *current alone* without any break in the circuit, and with the *poles as yet made use of, no gas* made its appearance at either pole.   It may be well here to remark that through a fine platina point, in which the electricity is forcibly confined to the limits of the most minute conductor, if an attempt be made to pass a shock of any magnitude, the constricting portion of glass is instantly and forcibly removed, and the platina laid bare for a considerable portion of its length, so that the pole by this means becomes utterly useless.

. 27. From the results of these experiments the idea suggested itself that a twofold condition of each pole, one during, and the other instantly after, the passage of the electric discharge of *spark or shock* might occur, and by this means be the reason why oxygen and hydrogen were found products from each pole.   This idea was also somewhat strengthened by find-ing that Dr. Faraday discovered a two-fold condition of an electro-magnetic wire, and opposite effects from the same, upon making or breaking contact see series 1st—(11, 14, 15, &c.)   " The slight deflection (says he) of the needle occurring at the moment of completing the connection, was always in one direction, and the equally slight deflection, produced when the contact was broken, was in the other direction."   To prove the same by experiment I first charged a jar, insulated as usual, and connected to the positive and negative conductors, having first appended a wire pole, to each coating, terminating in a ball, and about three-quarters of an inch apart, and to and at the end of the negative, a slender wire terminating in a pith ball.   The ball was repelled from its perpendicular by excited wax until the discharge passed, when it was instantly attracted and re-tained (still out of its perpendicular) by the wax for a short period, thus manifesting a negative state of the negative pole, during the charge, and a positive or natural state of the same during, or instantly after, the passage of the contents of the jar. I next suspended a pith ball by a glass thread in the vicinity of the negative pole of the jar ; during the charging of the jar the ball adhered firmly to the termination of the pole, but instantly on discharging the jar, the pith ball forcibly separated, and was repelled to a distance, and found to be attracted by excited sealing wax.   By this experiment also, a two-fold condition, first negative and then positive, of the pole in contact with the outer coating of the jar, is pretty fairly exhibited.   Finally, I procured an electroscope, (a pith ball terminating the extremity of a fine glass filament, which filament was supported by a needle point, insulated, and thus allowing the rotation of the filament upon its axis), this I arranged between the two termi-nating balls of the Leyden jar—the ball of the electroscope be-came positively electrified and pointed constantly to the nega-tive terminating ball during the charging of the jar—but in-stantly on the explosion taking place, the pith ball was repelled

towards and attracted by the positive terminating ball, some-
times as near as possible, but contact was purposely prevented;
This was invariably the case during a dozen or more repetitions
of the experiment. Upon applying excited sealing wax the pith
ball was attracted strongly, shewing the same to be positively
electrified. In all these experiments the ball was first attracted
to the positive pole—it then receded a little until the residuum
was discharged, after which a permanent attraction was the
result. The most rational method of accounting for the alter-
nately opposite electrical condition of each pole is by supposing
the charge to pass in a globular volume. Thus :—

Fig. 1, p p.   p   n   n p

Fig. 2, p p.   n   p   n p

By the Franklinian hypothesis.

During the passage of the charge at the termination of the
positive pole p. the opposing end of the negative pole n. would
be negatived by the polarizing influence of p. But the moment
the charge arrives at n (represented by p. fig. 2) the termina-
tion of the positive pole would be negatived by the inductive
influence of the charge at p. fig. 2.

2S. If, then, the poles of a Leyden jar be alternately positive
and negative, or negative and positive, which has been already
seen, there is no difficulty in explaining why oxygen and hydrogen
gases are formed at each pole, and independently of each other.
If it is found that the negative pole of a voltaic battery invari-
ably sets at liberty hydrogen gas—the positive pole oxygen, and
each pole of the Leyden jar be alternately negative and positive,
or vice versa, the decomposition by the latter with mixed
hydrogen and oxygen gases at each pole, is identical with the
principles of decomposition exhibited by the voltaic apparatus ;
for if from each pole a single gas should be alone obtained, the
pole itself being continually changed, the effect would be indeed
" different from that of the galvanic phenomena." It appears,
therefore, that by the use of sparks or shocks, or indeed any
break in the electrical current, we do not produce a decom-
posing effect truly identical with that of the voltaic battery,
there being mixed gases eliminated by each pole.

(To be continued.)

*An Investigation of the Lateral Explosition, and of the Elec-
tricity communicated to the Electrical Circuit, in a Dis-
charge.* By JOSEPH PRIESTLEY, LL.D., F.R.S. From the
Phil. Trans. for the year 1770.

Several years before Dr. P. made any experiments in elec-
tricity, except with a view to amuse himself and his friends, he
had observed, that in discharging jars, and particularly such as
were filled with water, without any coating on the outside, he
felt a slight shock ; though it was plain that the hand in which
he held the discharging rod made no part of the circuit. Mr.
Wilson, also, in his first experiments on the Leyden phial, ob-
served, that bodies placed without the electric circuit would be
affected with the shock, if they were only in contact with any
part of it, or very near it. Analogous to this was his observa-
tion, that if the circuit was not made of metals, or other very
good conductors, the person who laid hold of them, in order to
perform the experiment, felt a considerable shock in that arm
which was in contact with the circuit. See History of Elec-
tricity, p. 95. Lastly, in the course of his experiments with
large electrical batteries, he found the force of this lateral ex-
plosion, as he calls it, to be very considerable : for he several
times observed, that a chain which communicated with the
outside of the battery, but which made no part of the circuit,
made a black stain on a piece of white paper, on which it acci-
dentally lay, almost as deep as the chain that formed the cir-
cuit. (History, p. 644.) And when, in order to judge, by his
feeling, of the lateral force of electrical explosions, he made it
pass over a part of his naked arm, the hairs of the skin were all
singed, and the papillæ pyramidales raised, not only along the
path of the explosion, but also wherever any part of the chain
had touched it, though not in the circuit. Ib. p. 686.

It was to ascertain the nature and effects of this lateral ex-
plosion, that the following experiments were made. Not having
the least doubt, but that if any electric spark passed between a
body that was insulated, and another, the insulated body would
appear, either to have received or to have lost electricity ; he
imagined that nothing more was to be done than to insulate
bodies placed within the influence of the electric circuit, with
pith balls hanging from them ; and on their diverging with the
electric spark, immediately to observe of what kind the electri-
city they had contracted was ; and previous to the experiment
he conjectured it would be negative ; supposing that the dis-
charge from the inside coating, in an interrupted circuit, was
not able to supply the outside fast enough. And since the
larger the insulated body was, the greater quantity of the elec-
tric fluid it was capable of receiving, or parting with, and con-
sequently the more sensible the effort would be ; he began with

suspending on silken strings, a pasteboard tube, covered with tinfoil, 7 feet long and 4 inches thick, with large knots at each end; and a brass ball (at the end of an iron rod, which communicated with the outside of the jar) was placed within about a quarter of an inch of it; while the discharge was made through an insulated interrupted circuit, no part of which was less than two feet from the insulated tube. On making the explosion, the spark appeared as he expected; but to his great surprise, he could not find that either positive or negative electricity was communicated to the insulated tube. Neither the pith-balls, nor the finest threads diverged, or moved in the least, at or after the discharge; though, every thing else remaining in the same state, the least sensible electricity communicated to this tube (a quantity so small as hardly to be visible, in the form of a spark, at the time of the communication) made the balls and the threads separate to a great distance, and would have kept them in a state of divergency more than an hour. Lest a small degree of motion or divergency should escape notice while he was intent on making the discharge, he had an assistant along with him, whose eye was on the threads all the time the Dr. was making the experiment.

The experiment, as will easily be imagined, shook his whole hypothesis, and confounded all his ideas. He could not question the fact, having repeated the experiment, with precisely the same event, above fifty times, on account of having been hardly able to believe his own senses, or those of others There was an evident electric spark, sometimes near half an inch in length, between the bodies composing the electric circuit and the insulated tube, in such a state of the air as he knew, by frequent trials, would have kept it electrified a long time, and yet there was no communication of electricity. He did not remember that he was ever more puzzled with any appearance in nature than he was with this; and in the night following these experiments endless were the schemes that occurred to him of accounting for them, and the methods with which he proposed to diversify them the next morning, in order to find out the cause of this strange phenomenon. Accordingly, he was no sooner at liberty to attend to this experiment, but repeating it with some difference in the disposition of the apparatus, he observed that on every discharge a slight motion was given to the threads that hung from the insulated tube. On this the impossibility of an electric spark, neither giving nor taking anything from an insulated body, contrary to his most attentive observation, and that of his assistants, he concluded that some motion must have been given to the threads the day before; especially when he found that in these later experiments the communicated electricity was always positive, the same with that of the inside of the jar; but the quantity of it was so small, that the most

exquisite contrivance was necessary to ascertain the nature of
it; for though on this occasion the lateral spark was near a
quarter of an inch in length, the threads on the insulated tube
could only be made, by the explosion, to change their position,
from leaning a little one way, to leaning as much the other, in
the neighbourhood of an insulated brass rod, loaded with a
small quantity of positive or negative electricity.

Dr. P. could not help, however, being surprised that so large
a spark should give ho more electricity to the insulated tube
than it appeared to have done; when, in other circumstances,
a spark ten times less than this would have made a great and
permanent alteration; yet improbable as these circumstances
were, he entertained no doubt at that time, but that these insu-
lated bodies were electrified, either positively or negatively, ac-
cording as the inside of the jar was positive or negative, by this
lateral explosion, though the degree was exceedingly small, and
he continued in this persuasion the longer, as it happened to be
a considerable time before he got another spark that communi-
cated no sensible electricity. Dr. P. cannot help taking notice,
that if it had happened, that in his first experiment the insu-
lated tube had always acquired or lost the least sensible elec-
tricity, (and as he afterwards found there were many chances
against the first result,) he should have formed, and have ac-
quiesced in, some sort of hypothesis, to account for the giving
or receiving of electricity in those circumstances, and there the
business would have ended, but the seeming contrariety of these
appearances obliged him to pursue them further.

Not being able completely to satisfy himself with his last con-
clusion, attended with the difficulties above mentioned, he kept
diversifying the experiments, and introducing every circum-
stance that he could imagine might possibly affect the result of
them, and among the rest, he made the following experiments
which quite unhinged him again, and left him as much at a loss
as ever he had been before. Having suspended a fine thread
on an insulated brass rod placed about 1-8th of an inch from
another rod, which was likewise insulated, and one end of which
was in contact with the coating of the jar, and, having electri-
fied the rod that supported the pith-balls, and placed a rod
loaded with the same electricity near them, he observed that
on every discharge, the balls, which before were repelled, were
instantly attracted by the electrical rod, and that the result was
invariably the same whether they and the rod were loaded with
positive or negative electricity, and also whether the jar was
charged positively or negatively. He repeated the experiment
for several hours without the least variation in the event, which
clearly proved that in these circumstances the electricity of the
rod that received the lateral explosion was discharged by it.

Afterwards, Dr. P. repeated this experiment with some little

variety, and found the electricity of the rod only lessened by
the lateral explosion. These experiments, however, by no
means favoured the supposition of the uniform communication
of electricity, either that of the inside or that of the outside of
the jar: and together with the former experiments, convinced
him that this lateral spark by no means produced the effect that
might have been expected in communicating electricity. But
with the next set of experiments the difficulty began a little to
clear up, and continued to do so gradually, till he gained all the
satisfaction he could wish for, with respect to this puzzling
phenomenon. The first time that he was able to vary the elec-
tricity of the insulated body placed near the electric circuit, or
of the bodies that formed the circuit, (which he now began to
attend to,) by any different adjustment of the apparatus, was
on the following occasion.

Near to an iron rod, that touched the bottom of a jar charged
positively, he placed another insulated rod, with a pair of pith
balls hanging to it; and observed, that when he attempted to
make the discharge through an imperfectly conducting circuit,
(bringing for instance, part of the table into it), a strong spark
passed between the insulated rod and the other that touched the
jar, and immediately the balls separated as far as they possibly
could; and, continuing in a repulsive state, appeared to be
electrified negatively. But immediately completing the circuit
with good conductors, and making the remainder of the explo-
sion in full spark; another spark passed between the two rods,
and immediately the balls fell close together again; and some-
times would separate with the opposite, or positive, electricity.

Dr. P. could not, on this occasion, make the lateral spark, in
the full explosion, so great as in the imperfect discharge. He
also observed, that the more interrupted the circuit was, the
farther would the lateral explosion reach; and that the electri-
city, which the full explosion communicated, was always posi-
tive when the jar was charged positively, and negative when it
was charged negatively. The result of the imperfect discharge
was always the reverse. Insulating several bodies, and the jar
too, charged positively, they all equally contracted positive
electricity by the discharge.

In this state of the experiments, he had no idea of the possi-
bility of the lateral spark not communicating electricity to the
insulated body; but he imagined that the kind of electricity
communicated depended on some circumstance in the disposi-
tion of the apparatus, that he was not sufficiently aware of. At
length recollecting, that this last experiment resembled, in some
respects, that curious one of Professor Richman, mentioned in
the History of Electricity, p. 272, in which it appeared, that
when the coating of either side of a plate of glass communi-
cated with the ground, the opposite electricity of the other side

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was more vigorous ; he was satisfied that the negative electricity
of the bodies that formed the circuit in the imperfect discharge,
was produced by the greater difficulty with which the outside
of the jar was supplied, than the inside was discharged ; so that
the outside was comparatively in a state of insulation, and
" therefore would communicate negative electricity to all bodies
within its reach. And from this he was led to conclude, that,
provided the jar was insulated, and the inside was made to part
with its electricity with more difficulty than the outside re-
ceived it, the bodies that formed the circuit would contract
positive electricity ; and the result answered exactly his expec-
tations. He also concluded, that, making the interruption in
the middle of the circuit, since, in this case, the inside would
give, and the outside receive, with equal difficulty, the bodies
in the circuit, places between the place of interruption and the
inside of the jar, would be charged positively ; and those placed
between the place of interruption and the outside, would be
charged negatively ; and this also was verified by experiment.

In this state of things, Dr. P. found that he could give the
insulated circuit what kind of electricity he pleased, provided
there was any kind of interruption in some part of the cir-
cuit ; and conjecturing that the electricity of bodies placed near
the circuit would be the same with that of the bodies that com-
posed it, he sometimes placed the rod that supported the pith
balls near the circuit, and sometimes introduced it into the
circuit ; and found, that, in both cases, it contracted the same
electricity. This tended to confirm him in the supposition, that
the lateral explosion was always attended with a giving or re-
ceiving of electricity, according to the nature of the circuit,
and the place where it was situated , and he again overlooked
the disproportion between the cause and the effect.

Presently after this, it occurred to him, that what may be
redundant electricity of the outside or inside of the jar, sepa-
rates from that which is in the glass, and constitutes the charge,
must have some concern in this event ; and the supposition was
verified by fact. For insulating a jar, charged positively, he
observed, that when he touched the outside coating last (as is
commonly done in setting it down) and made the discharge
through good conductors, they were all electrified positively ;
and bodies placed near the circuit were the same. On the con-
trary, if, after placing the jar on the stand, he touched the
knob of the wire, communicating with the inside, so as to take
off all its redundant electricity ; both the circuit and the neigh-
bouring bodies contracted negative electricity.

Dr. P. had at this time quite forgot that Æpinus had made
the same observation, on discharging a plate of air, mentioned
in the History of Electricity, p. 272 ; but considering what he
says on that subject, he found he was mistaken with respect to

the reason of this experiment not succeeding with Dr. Franklin and others, who had always asserted, that the electric circuit contracts no electricity at all by a discharge. For he says, that the surfaces with which the doctor tried the experiment, were not large enough to make the effect sensible; and that the distance of the metal plates was likewise too small, as, he says, it necessarily must be in the charging of glass: whereas he could give the insulated circuit to sensible electricity with a common jar, as he could with his plate of air; and much more depends on the height of the charge, which must have been inconsiderable in the plate of air, than the quantity of surface; which, however, may be increased at pleasure, by multiplying jars in batteries.

He found, however, afterwards, that much depended on the quantity of surface in the coating, and the bodies connected with them, as containing more of that redundant electricity, the effect of which was seen in the last mentioned experiment. For when he discharged the jar, standing in contact with the prime conductor, the tendency to the communication of positive electricity was so great, that, in that situation, the insulated circuit contracted strong positive electricity, when, every thing else remaining the same, except removing it from the conductor, and then making the discharge, it contracted no electricity at all.

Being now perfectly master of the electricity of the circuit in electrical explosions, and being able, in two different methods, to give which of the two electricities he pleased; he imagined that, if he could so balance them, as to communicate neither, there would be no lateral spark, as in the abovementioned experiments; but in this he was absolutely mistaken. For, in the first place, when, after setting the charged jar upon the stand, he took off, as near as he could guess, one-half of the redundant electricity of the inside, and left both sides equally electrified (as appeared by the equal attraction of the pith balls to them both), the discharge of the jar through a circuit of good conductors did not, indeed, communicate the least sensible electricity to the circuit, but the lateral explosion was almost as manifest as before. The pith balls, hung upon the rod that received it, never separated. In the next place, he repeated this experiment by balancing the two different methods of communicating electricity to the circuit, one against the other. For, not insulating the jar, but, setting it on the table, which gave the circuit and the bodies contiguous to it an advantage for contracting positive electricity by the discharge; but, at the same time, making an interruption in the circuit (by introducing part of the table into it, which tended to give them negative electricity); he could easily manage it so, that the circuit contracted neither the one nor the other; and yet, as in

the former case, the lateral explosion was as considerable as
ever. The balls never separated:

To vary this experiment, he placed an insulated brass ball, two
inches in diameter, round and smooth, so as not easily to part
with any electricity it had got, in the place of the rod that
supported the pith balls; and having found a situation in which
no electricity was communicated to the circuit, he observed that
none was communicated to it, though, to all appearance, it re-
ceived a spark of about ½ of inch in length. At least, if it had
contracted any, it was so little, as to make it very problemati-
cal, whether a pith ball, or a fine thread, was moved by it, or
not: whereas, when he gave it the smallest sensible spark in
any other manner, it would attract those light bodies for a long
time together. The interruption of the circuit made use of in
this experiment, was not by means of any part of the table, but
only about a yard of brass chain introduced into it, and dis-
posed between the inside of the jar and that part of the circuit
near which the insulated ball was placed. N.B. The ball must
not be placed near the jar itself; for, in that situation, he found
that, though it was very smooth, and perfectly spherical, yet it
could not be placed very near the outside of the jar standing on
the table, without contracting negative electricity, in a very
small space of time.

These experiments threw him back into his former state of
perplexity, with respect to the lateral spark; since, when the
two electricities of the circuit were exactly balanced, it was very
little diminished, and yet the body that received it was not in
the least sensibly electrified. But, on reflection, he concluded,
this lateral spark must be of the nature of an explosion, and
consequently, that an electric spark must enter, and pass out
again, within so short a space of time, as not to be distinguished,
and leave no sensible effect whatever: for though, in this case,
part of the electric matter natural to the body must be repelled,
to make room for the foreign electricity, its restoration to its
natural state was so quick, that no other motion could corres-
pond to it. This hypothesis is favoured by the observation, that
it is the very same thing, whether a body be introduced into a
circuit, or placed near it, with respect to contracting electricity;
that is, whether the electric charge enter the body at one place,
and go out at another, or whether it be received and emitted at
the same place. This lateral explosion is an effect similar to a
partial circuit, in which, part of the electric matter that forms
the charge in an explosion, goes one way, while the rest of the
charge goes another; the only difference is, that this detached
part of the charge leaves the common track, and returns to it
again, in the very same place.

Several remarkable partial circuits occurred in the course of
his experiments before, particularly one, mentioned in the His-

tory of Electricity, p. 692, in which, part only of the explosion passed in the shortest way, while another part of it took a circuit, consisting of the same materials, 30 times as long; and another, mentioned, p. 691, where one circuit was made through a thick rod of metal, and another, at the same time, through the open air.

That there is an admission and an explosion of the electric matter, in this lateral explosion, seems evident, from this circumstance, that it is far more considerable when the body that receives it is large, than when it is small. In the former case, there is room for the electric matter, natural to the body, to retire, on the admission of the foreign electricity belonging to the charge; whereas, in the latter case, there is not room for it. When he placed a small brass ball, of about a quarter of an inch in diameter, near the circuit, he could not perceive that it was at all affected by any lateral explosion; and the spark was very inconsiderable, when he placed a needle, about two inches in length, to receive it; but when he connected the large tube above mentioned, by means of a pretty thick iron wire, or any body whatever, placed in the neighbourhood of the circuit, he had (with a jar of only half a square foot of coating glass) made the lateral explosion, an inch or more in length, consisting of a very full and bright spark of electric fire.   Insulated bodies, of about eight or nine feet in length, seem to admit as large a lateral explosion as any body whatever is capable of: for, connecting them with the ground, by means of the best conductors (which gave the electric matter in the bodies, the freest recess possible), he could never make this explosion much more considerable, using the same jar, and all other circumstances the same.

It is a manifest advantage in these experiments, that the lateral explosion be not taken from the coating of the jar itself, or from any part of the circuit, very near to it.   He found that, cæteris paribus, it is the most considerable when taken at the extremity of a brass rod, of one foot, or a foot and a half long, the other end of which is contiguous to the jar.   It is analogous to this, that the longest spark is taken, not from the body of the prime conductor itself, but at the extremity of a long rod inserted into it.   The electric matter seems to acquire a kind of impetus by the length of the medium through which it passes.   But he found that the maximum, in this case, did not exceed, or rather, that it did not quite reach, three feet; for, making use of a thick iron rod, eight or nine feet long, the lateral explosion, taken at the extremity of it, was about the same, as when it was taken at the end of a rod four inches from the jar; and not half so considerable as when taken at the extremity of a rod one foot long.   This, he imagined, might be owing to the obstruction which the electric fluid meets with

in passing even through metals ; which appears. by his former experiments, to be much more considerable than was generally imagined.

On the whole, this remarkable experiment seems to be made to the most advantage in the following circumstances. Let the jar stand upon the table ; let a thick brass rod, insulated, stand contiguous to the coating ; and, near the extremity of this rod, place the body that is to receive the explosion. This body must be 6 or 7 feet in length, and, perhaps, some inches in thickness, or be connected with a body of those dimensions. Lastly, let the explosion be made with the discharging rod resting on the table, close to a chain, the extremity of which reaches within about an inch and a half of the coating of the jar. In this case, the operator will hardly fail of getting a lateral explosion of an inch in length ; which shall enter and leave the insulated body, without making any sensible alteration in the electricity natural to it.

With large jars, containing 3 or 4 square feet of coated glass, bearing a very high charge, Dr. P. makes no doubt but that this experiment might be made to much more advantage ; but at the time he was engaged in this investigation, he happened not to have any such jar, and therefore only used one that contained half a square foot of coated glass. If the interruption in the circuit, which is almost necessary in these experiments, be made by introducing a length of chain into it, rather than by making part of the explosion pass along the tube, there is a medium in the length of chain, that answers better than either a longer or a shorter circuit. In a long interrupted circuit, the electric matter seems to lose the impetus which it discovers in a short one.   In all these cases, the electric charge seems to remain for a moment in the parts of the interrupted circuit ; and therefore instantaneously rushes, in all directions as well towards bodies that are not placed along its passage to the jar, as those that are ; but, when the same charge occupies a larger circuit, it has more room to expand itself, and is not so strongly impelled to desert it. He found, however, by repeated trials, that when he made use of three yards of brass chain in the circuit, there was a distance to which the lateral explosion would not reach.   The same distance it also would not reach, when the circuit consisted of only one brass rod ; but it reached it with great ease, when only half a yard of chain was used ; even without any other interruption in the circuit. But it reached to a much greater distance, when the chain was very short, and the interruption was greater in other respects.

Dr. P. had imagined, that, since the body which had received the lateral explosion, contained, for a moment, more than its natural quantity ; that if it were acutely pointed, some would escape, and that on the return of the explosion, the body would

be exhausted; but he found no such effect, though he affixed
fine needles to the bodies he made use of. The lightest pith
balls placed near the extremities of these needles, were not in
the least affected by the explosion. When he placed a number
of brass balls, one behind another, the lateral explosion passed
through them all, being visible in the intervals between each of
them, and returned the same way; leaving them all in the same
state in which it found them; and a great number of lateral
explosions might be taken at the same time, in different parts
of the circuit, some of them very near one another. It made no
difference, whether the lateral explosion was received on a flat
smooth surface, or the points of fine needles. In both cases the
spark was equally long and vivid.

Dr. P. had no sooner completed these experiments on the
lateral explosion, but he had a curiosity to see what kind of an
appearance it would make in vacuo; since no other phenomenon
in electricity resembles it. In all other cases, the electric matter
rushes in one single direction; whereas, in this, it goes and
returns in the same path, and, as far as can be distinguished, at
the same instant of time; so that all the difference of the two
electricities, which are so conspicuous in vacuo, must here be
confounded. Accordingly he found, though the pump was not
in good order, that he could perceive this explosion in vacuo,
at the end of rods, placed several inches asunder; and when
they were brought within about two inches, they seemed to be
joined by a thin blue or purple light, quite uniform in its ap-
pearance. As these rods were made to approach, this light
grew denser; but still exhibited no such variety, as is observed
between the bodies that give and receive electricity, in the
common experiments in vacuo.

Dr. P. was pretty soon convinced, that uncoated jars could
not be used to any more advantage in these experiments, than
those that were coated; since the want of coating only operated
as an interruption in the circuit, occasioning a difficulty in the
admission of the charge on the outside of the jar. And, in all
cases, the greater this difficulty of passage was made, provided
the discharge was made at once, the more considerable was the
lateral explosion, and the greater shock was given to the hand
that held the discharging rod; which shock was nothing more
than one of these lateral explosions, issuing from the rod as
part of the circuit. He concludes the account of these experi-
ments with observing, that they may possibly be of some use in
measuring the conducting power of different substances; since,
the greater is the interruption in the electric circuit, occasioned
by the badness of its conducting power, the more considerable,
cæteris paribus, is the lateral explosion.

On the Appearance of Lightning on a Conductor fixed from the Summit of the Mainmast of a Ship down to the Water. By
Capt. J. L. WINN. From the Phil. Trans. for the year 1770.

Capt. Winn says he was never without a conductor in his
ship. He had them of various constructions: that which he
last used was a chain of copper wire, down by the outside of the
ship into the water. That such a chain, so disposed, may con-
duct the lightning, and prevent a stroke that might destroy a
ship, has often been demonstrated; but a circumstance that
occurred in his last voyage, may perhaps have greater weight
with some seamen, than all the reasonings of the electricians.
If it should be a means of persuading them to make use of
conductors, his intention will be answered.

In April last, as they approached the coast of America, they
met with strong south-westerly gales; they had continued
several days, when exceedingly dark heavy clouds arose in the
opposite quarter, forced against the wind till they had covered
all the north-eastern half of the hemisphere : the struggle then
between the two winds was very extraordinary; sometimes one
prevailing, sometimes the other. Captain W. was apprehensive
they should have much lightning, and got his conductor in
order ; when, in hauling up the mainsail, the sheet block struck
violently against the back-stays, to which the chain was fastened,
and, as he found afterwards, broke the latter, which occasioned
the phenomenon described below. It was near midnight, and
very dark, when he first observed a pale bluish light a few feet
above the quarter rail : at first he thought it proceeded from the
light in the binnacle ; but finding that it frequently disappeared
and returned again precisely in the same place, and that it some-
times emitted sparks not unlike those of a small squib, he began
to suspect that it proceeded from the conductor. To be cer-
tain, he ordered all the lights to be put out below, and that no
rays of light might issue from the binnacle, he covered it en-
tirely with his cloak. He was presently confirmed in his con-
jecture, that the light and sparks proceeded from the chain ;
for, placing himself near it, during the space of two hours
and a half, he saw it frequently emit continued streams of
rays or sparks ; sometimes single drops as it were slowly suc-
ceeding to each other, and sometimes only a pale feeble light.
On examining next morning, he found the chain broken a little
above the ship's gunwale, half the eye of each link being quite
gone, and the points of the remaining halves about three-fourths
of an inch asunder : luckily the chain was fastened to a smaller
rope above and below the eye of each link, which prevented
that part of the chain below from falling into the water, or of
being separated from the part above, beyond the striking or
attracting distance.

On the Method of obtaining True Flat Metallic Surfaces, independently of the usual Operation of Grinding. BY JOSEPH WHITWORTH, ESQ.

Before proceeding to the particular consideration of our present subject, it will be proper to premise a few general remarks, for the benefit of those to whom it may be now for the first time presented.

It is necessary to form a distinct idea of what is meant by a true surface. Strictly speaking, it is a surface of which all the points are in the same plane—that is, on the same level—and which is, therefore, in no degree either round or hollow, but perfectly flat. Supposing two such surfaces applied together, they would touch at every point throughout. Now, it will be easily understood, that in various operations it becomes a matter of the greatest importance to obtain a surface of this kind. If it is required to fix anything perfectly horizontal or perpendicular, or at any inclination with great nicety, this cannot be done without the aid of a flat surface to serve as a guide, nor without the means of forming such surface on the materials employed. Again, in the case of metallic surfaces working together—want of truth is attended with various evils. The motion, instead of being in a straight line, undulates with the outline of the surface, and this will often entirely defeat the object proposed. The spaces between the parts in contact open throughout the slides a free communication, which it is in many cases essentially necessary to prevent. Thus, in the valves of steam engines, want of truth leaves the communication open between the two sides of the piston, and there is consequently a loss of power proportioned to the amount of error. Another and an universal consequence of error is, that the friction of the two surfaces, instead of being diffused throughout their whole extent, is collected at particular points, and hence they are apt to gall, and become unfit for use. Nor is this all ; the friction is not only unequally distributed, but also materially increased. The inequalities of the opposed surfaces create mutual obstruction, and become clogged with particles of foreign matter.

These considerations will suffice to show, that a true surface is an object of the greatest practical importance. But it will be at once perceived, that absolute truth is unattainable, and that we can only make such approximation as the means within our power will allow. Hence it becomes necessary to know where the practical limit lies, and what is the degree of truth which it may be desirable or possible to attain. There are a great

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* Read at the Conversazione, held at the Royal Victoria Gallery of Practical Science, Manchester, on the evening of the 8th December, 1840. Joseph Radford, Esq., in the Chair.

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variety of tests applicable to surfaces, each of which will tell to a certain degree of nicety. There is, for example, the plumb line, the spirit level, the winding strips, and the straight edge. But the degree of truth which it is possible and in many cases necessary to attain, is far beyond the limits of these instruments.
• When applied to surfaces, they do not distinguish between the points in actual contact, and those which are extremely near—and hence a surface may seem to be perfect, though in fact it is full of inaccuracies. But if we substitute a true surface, of the same extent as that we wish to try, we obtain the means of testing it to the utmost degree of nicety. The method hitherto adopted, and still practised in preparing what are called true surfaces, is, after filing, to grind them with emery by rubbing one on the other.

My present object is to show, that this process is not at all calculated to produce a high degree of truth, and that a method entirely different ought to be adopted in its stead.

I have here two cast iron surface plates, which have been got up in the manner I shall presently describe. They possess a degree of truth far beyond what is generally attained. So rare, indeed, is it, that they have occasioned no little surprise to some of the most distinguished engineers and mechanicians of the present day. The effects, which, as I shall presently show, result from the truth of these surfaces, are till now almost unknown, and would a short time ago have been pronounced impossible. If one of them be carefully slided on the other to exclude the air, the two plates will adhere together with considerable force by the pressure of the atmosphere.

The surfaces should be well rubbed previously with a dry cloth, till they are perfectly free from moisture, in order that the experiment may afford a fair test of accuracy. If any moisture be present, it will act like glue, and would cause adhesion to take place, supposing the surfaces to be much inferior. But, if they be perfectly dry, adhesion proves a high degree of truth rarely attained.

The experiment may be varied by letting one surface descend slowly on the other, and thus allowing a stratum of air to form between them. Before they come into contact, the upper plate will become buoyant, and will float on the air without support from the hand. This remarkable effect would seem to depend on the close approximation of the two surfaces at all points, without contact in any—a condition which could not be obtained without extreme accuracy in both. The escape of the remaining portion of air is retarded by friction against the surfaces, the force of which nearly balances the pressure of the upper plate.

These surfaces were got up by means of filing and scrap-

ing alone, without being afterwards ground. The operation of scraping is but partially known, and has been adopted only as preparatory by grinding. In reference to both processes, a great degree of misconception prevails, the effect of which is materially to retard the progress of improvement.

It will be evident, from a little consideration, that a true surface cannot be obtained by grinding. And, first, with regard to general outline, how is the original error to be got rid of? Let it be supposed, that one of the surfaces is concave, and the other a true plane. The tendency of grinding, no doubt, will be to reduce the error of the former ; but the opposite error will, at the same time, be created in the true surface. It will further appear, that, if the original error be inconsiderable, the surfaces must receive positive injury from grinding. Certain parts are acted upon for a longer time than others. They are consequently more worn, and the surfaces are made hollow. If grinding be not adapted to form a true general outline, neither is it to produce accuracy in the minuter detail. There can be little chance of a multitude of points being equally distributed under a process from which all particular management is excluded. To obtain any such result, it is necessary to possess the means of operating independently on each point, as occasion may require ; whereas grinding affects all simultaneously. If a ground surface be examined, the bearing points will be found lying together in irregular masses, with extensive cavities intervening. An appearance, indeed, of beautiful regularity is produced ; and hence, no doubt, the universal prejudice so long established in favour of the process. But this appearance, so far from being any evidence of truth, serves only to conceal error. Under this disguise surfaces pass without examination, which, if unground, would be at once rejected.

Another evil of grinding is, that it takes from the mechanic all responsibility and all spirit of emulation, while it deludes him with the idea that the surface will be ultimately ground true. The natural consequence is, that he slurs it over, trusting to the effect of grinding, and well knowing that it will efface all evidence either of care or neglect on his part.

It thus appears, that the practice of grinding has altogether impeded the progress of improvement. A true surface, instead of being, as it ought, in common use, is almost unknown : few mechanics have any distinct knowledge of the method to be pursued for obtaining it. Nor do practical men sufficiently advert, either to the immense importance, or to the comparative facility, of the acquisition.

Due latitude must be allowed the expression, 'true surface.' Absolute truth is confessedly unattainable. Moreover, it would

be possible to aim at a degree of perfection beyond the necessity of the case, the difficulty of which would more than counterbalance the advantage. But it is certain, that the progress hitherto made falls far short of this practical limit, and that considerations of economy alone would carry improvement many degrees higher. The want of it, in various departments of the arts and manufactures, is already sensible. The valves of steam engines, for example,—the tables of printing presses,—stereotype plates,—surface plates,—slides of all kinds,—require a degree of truth much superior to that they generally possess. In these, and a multitude of other instances, the want of truth is attended with serious evils. The injury occasioned in consequence to the valves of locomotive engines renders them liable to constant derangement, and causes an enormous waste of steam power, under circumstances more especially requiring its most advantageous application. In stereotype printing, inaccuracy of the plates renders packing necessary to obtain an uniform impression. A vast amount of time and labour is thus sacrificed; and the end is, after all, but imperfectly attained.

There is perhaps no description of machinery which would not afford an illustration of the importance belonging to truth of surface, and at the same time of the present necessity for material improvement; nor is there any subject connected with mechanics, the bearings of which on the public interests, whether manufacturing or scientific, are more varied or more extensive.

The improvement so much to be desired will speedily follow upon the discontinuance of grinding. Recourse must then be had to the natural process. The surface plate and the scraping tool will come into constant use, affording the certain means of attaining any degree of truth which may be required. A standard of excellence will thus be established, and a new field will be opened to the mechanic, in which he will find ample scope for the exercise of skill, both manual and mental."

Mr. Whitworth then proceeded to describe particularly the mode of obtaining a true surface such as those exhibited. He noticed that there were two cases for consideration—the one where a true surface plate is already provided as a model for the work in hand, and the other where an original surface is to be prepared.

"The former case, which is that which will generally occur in practice, is simple, requiring care rather than skill. Colouring matter, such as red ochre and oil, is spread over the surface plate as equally as possible. The work in hand is then applied thereto, and moved slightly to fix the colour; which, adhering to the parts in contact, afterwards shows the prominences to be reduced by the scraping tool. This operation is frequently repeated, and at each repetition a smaller quantity

of colouring matter is used, till, at last, a few particles spread out by the finger suffice for the purpose, forming a thin film over the brightness of the plate.

" The latter case is more complicated, and requires considerable skill in the mechanic. Three plates are got up together, and serve mutually to correct their own errors."

Thanks having been voted to Mr. Whitworth for his paper, the chairman (Mr. Radford) asked if the process of scraping had originated with Mr. Whitworth ; and that gentleman said, certainly not. He had mentioned in the paper that it was in use before, but not generally, and merely as preparatory to grinding. It was a much shorter process, that which he proposed, than grinding.

The chairman asked if the highly finished scraped surface would resist the action of the file more than before. He believed that in grinding (whether from the introduction of emery or not he could not say) the surface was found to be more tenacious of the file's tooth.

Mr. Whitworth said, he had not found that scraping made the metal harder ; of course, the emery used in grinding would destroy the file.

Mr. Fothergill said, it appeared to him there was nothing new in the process, except the introduction of scraping, instead of filing and grinding. In reference to the valves of steam engines and other matters where flat surfaces were required, Mr. Robert's invention of the planing machine should not be overlooked. What was the time required for scraping, as compared with the use of the planing machine ? It was well known, that this machine had come into extensive use in planing the valves of steam engines, and the surfaces upon which they had to operate ; and considerable facility and saving of time, and consequent expenses, were obtained over the old smooth filing and grinding.

Mr. Whitworth said, where a planing machine would do the work sufficiently accurate, his plan of scraping was not necessary.

Mr. Fothergill asked if the valves of steam engines could not be made sufficiently accurate by the planing machine to be steam-tight.

The Chairman said, he understood Mr. Whitworth to say, that his process of scraping was to remove the rough superficies left by planing.

Mr. Fothergill then objected to the ribs on the back of the plates, that, though they might give a stiffness to the plates, a change of temperature might take place from the action of the hand or other cause, and the expansion and contraction of the metal would differ at different parts of its surface, and consequently the plates would not retain at all times the same amount of accuracy.

Mr. Whitworth said there was no doubt a difficulty, however the surface was obtained to keep it perfectly flat; but he apprehended, if the plate were not ribbed, even if it were a much thicker plate, the surface would be still further off the truth.

' Mr. Fothergill thought a solid plate would not yield so soon to a change of temperature.

The Chairman thought, that the change of temperature in the metal, effected by a workman's hand, would be so trifling as to be inappreciable. '

Mr. Whitworth wished to ask Mr. Fothergill if he considered the slides of valves for locomotives were got sufficiently correct by the planing machine.

Mr. Fothergill had seen many instances where the valves had been tested, and there was not the slightest escape of steam. If the planing were properly accomplished, it would answer every practical purpose, with great saving of time (which was now every thing in point of economy) over the ordinary method. He wished to know whether Mr. Whitworth found it or scraping to require less time.

Mr. Whitworth repeated, that the scraping process was subsequent to the use of the planing machine. It did not supersede the planing machine in any case whatever.

Mr. Fothergill thought that the valves of steam engines could not be conveniently got at to scrape. No doubt, in planing them, great care was required in fixing the valves to the table of the planing machine, or else they might be sprung, and consequently would either acquire concavity or convexity, or cross-siding.

The Chairman said, he apprehended Mr. Whitworth was not instituting a comparison of cost, but seeking to obtain an exquisite degree of accuracy and flatness of surface. There might be machinery requiring a more exquisite degree of flatness between the parts than the steam engine, and in such cases his plan would supply the desideratum.

Mr. Whitworth asked when a surface had been taken out of the planing machine, and was found to be untrue, what steps would be taken to get it true.

Mr. Fothergill said this could only arise from a person not being properly acquainted with his business; but in such case he should recommend it to be properly bedded a second time, and the planing machine to go over it again; and he thought it would sooner be brought to a surface than by Mr. Whitworth's

Mr. Whitworth asked, when the slides were taken of the planing machine, how was it ascertained whether they were true or not.

Mr. Fothergill said, by the very method Mr. Whitwoth had pointed out, by trying different surfaces together, and sometimes even the planing machine itself might become inaccurate. They had an original surface to which they could refer at any time.

The chairman asked if Mr. Fothergill thought that as exquisitely true a surface could be produced by the planing machine as by scraping.

Mr. Fothergill had never tried how far it was practicable on the planing machine to produce an exquisitely true surface.

Mr. Whitworth said, they had made several planing machines, and got every part up by hand, and made the surface as correct as they could ; but they had never been able to get so good a surface from the machine as from scraping : they always needed correction afterwards.

The chairman thought it clear that a surface could not be produced so accurate by the planing machine : there was the wearing of the tool, and the action of the surface, which must resist the tool more or less in different parts of the superficies ; and those inequalities would be corrected by the process which Mr. Whitworth had suggested.

In reply to a question whether, if a valve came untrue out of the planing machine, Mr. Whitworth would plane it over again, —that gentleman said he should not, unless it was very considerably out of truth. He should generally apply it to the original surface plate, rub a little colouring matter on, and rub the surface over it, and then, with file and scraping tool, go over the bearing points. His opinion was, that a surface could not be got sufficiently accurate for the slides of a steam engine from the planing machine.

The chairman thought it was plain, that the surfaces produced from the planing machine were only approximations to the truth, even in the most practised hands.

A gentleman asked, if, when Mr. Fothergill tested the valves and found them steam-tight, oil or any other fluid was interposed.

Mr. Fothergill said, there was nothing but a little oil, which was always applied, when first put on, to prevent galling.

The chairman did not think the valves of steam engines to afford a fair criterion of the value of this process. The table of a steam printing-press required more truth of surface.

Mr. Whitworth said, he might mention that he had had a conversation with the engineer of the Liverpool and Manchester Railway last Monday (having communicated the scraping process to him nine months ago) ; and the engineer then told him, that the week before he had taken to pieces the stationary

engine at the end of the tunnel at the Liverpool end of the
line—having six weeks ago scraped up the surfaces of the
slides as he (Mr. Whitworth) had shown him—and last week
he had them out, and let on the steam at 50lb. pressure, when
not one particle escaped; the surfaces were most beautifully
polished, and did not seem one bit worse. He also stated,
that he had found very considerable advantage in scraping up
the slides for locomotive engines.

Mr. William Read said, he supposed Mr. Whitworth would
always begin with three surfaces.

Mr. Whitworth said he certainly should; for of two, one
might be a little concave and the other a little convex, and still
they might fit each other; but, if a third were introduced, of
course any little inaccuracy would be detected.

The Chairman said, in grinding it was usual to have three
surfaces.

Mr. Whitworth said, that the time expended in endeavouring
to get true surfaces by grinding was much greater than by
scraping; and, after all, a true surface could never be got by
the former process.

The Chairman said, it might be interesting to show the ab-
sence of sound, when one of these perfectly flat plates was
allowed to fall on another.

Mr. Whitworth showed this experiment, when a dull, dead
sound was produced totally different from the clanging noise
of two metallic substances striking together, which Mr. Whit-
worth said was owing to the air preventing the plates from
coming directly into contact.

Mr. Sturgeon, the lecturer at the Gallery, said, he would
offer another application of plane surfaces. in a case where they
could not be made too accurate,—magnets. It was a matter of
no consequence to the magnetist whether they were ground,
planed, or scraped; but it was important that their surfaces
should be true. In a piece of iron for an electro-magnet, a
great deal of accuracy was required in the surfaces which had
to come into contact. If the magnet were applied to the keeper,
or cross-piece, then, as both these surfaces should be as flat as
possible, of course an absolutely flat surface was a desideratum,
certainly not till now supplied. It had long been a recognised
principle in magnetism, that the nearer the whole of the surfaces,
and not merely little spots or points here and there, approached
each other, the better is the magnet; the more it would carry
with the same exciting force. In all cases of making or bring-
ing electro-magnets into a state of activity, the means employed
was a voltaic battery of the description of the one before him,
and a series of wires were wrapped round the iron in the same
manner as shown. Until within the last two months, electro-

magnets were in the same shape and form as usual, the horse-shoe or syphon-shape. Mr. Joule, of Salford, a very scientific young gentleman, first made them of the form now exhibited, which only weighed a quarter of the large magnet in the gallery, yet it would lift twice as much, in consequence of a peculiarity of form and arrangement, which Mr. Joule had given to the iron, and the peculiar manner of employing the connecting wires. He had, at first, a stout copper wire for his conductor, which went a few times round the iron. Under these circumstances, the magnet he now exhibited, would carry about a ton. . He, Mr. S., had seen 19 cwt. suspended to the keeper without breaking the contact.* But, by employing a bundle of wires instead of an individual wire of the same thickness, Mr. Joule had now made this magnet to carry 2,775lb, before the one surface separated from the other, when the voltaic battery was in action upon it. But, if they could obtain a better adaptation of the two faces to each other, the same magnet would probably carry not less than a quarter of a ton more. Mr. Joule had made this magnet in application to a theory of his own; and, in working out that theory, he had produced another magnet, still far more extraordinary than the former. [This was a long narrow cylinder, resembling, with its keeper, two long pigs of lead, placed face to face.]† Its weight was about 6lb.; it would at present carry 1,556lb., by the mere magnetic force exercised between the two pieces of metal, by the use of the same galvanic power. In all these cases Mr. Joule had employed a very formidable iron battery, such as was first made in this gallery; having sixteen square troughs of cast iron, in each of which was placed, properly insulated, a sheet of zinc, well amalgamated; then, with these sixteen troughs well charged with a solution of sulphuric acid, and formed into a series of four, or placed in a range of four times four, he obtained this powerful electro-magnetic action. He, Mr. S., had now to show them a still more modern and more extraordinary magnet, this being its first appearance in public, and an experiment performed with it that evening was the first by this singular magnet. It was not a horse-shoe, nor a long bar or cylinder of iron, but a disc, something like a quoit; and that which in the ordinary magnet was usually called the cross-piece could scarcely bear that name; but, from its shape, might be named the lid of the magnet. In this case, they had an involute channel for the reception of a single strand of very thick copper wire. The strand was covered with silk, for the purpose of preventing contact between the wire and the iron. It passed through the centre of the disc, came out of the face, and filled up, to a certain extent, the convoluted groove. Now, with this magnetic and a voltaic battery, such as that in the gallery (

* This magnet is described by Mr. Joule, at page 187, plate iv. vol. v. of the Annals.

† Ibid page 470, plate v ii.

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series of four pairs), and another of precisely the same size, still
making merely a double battery (consisting of eight cast-iron
jars, with their respective cylinder of zinc,—eight jars in a series
of four), he had that evening seen that magnet carry a ton
(2,240lb.)* This magnet had been first proposed to be made in
that room six weeks ago, by their worthy chairman, Mr. J. Rad-
ford, and it had been completed this evening; and he (Mr. S.)
had now very great pleasure in stating the result of its first ex-
periment. Here again they would see the importance of a flat
surface, in reference to these magnets. He had no doubt, from
the inequality of its surface, which Mr. Whitworth had been
kind enough to point out to him that evening, that they had not
arrived at any thing like the maximum of its magnetic power ;
and that having levelled its surface to a closer approximation
to absolute truth, he thought it very likely, that, before the
next conversazione, this magnet would be capable of carrying a
ton and a half. He was aware, that other extraordinary magnets
were now being constructed in Manchester ; and, by and bye,
the institution would have another magnet, as extraordinary as
any of them, added to its already unparalleled stock.—(Ap-
plause.)

On the Therapeutic Effects of Electricity.   By S .Lillie
Thornton, Esq., Surgeon.

The reputation of electricity as a medical agent, is very dubious
and fluctuating : owing to the exaggerations, the mistakes, the
misrepresentations, and the prejudices, and particularly the interest
of those who have administered it.   Hence it requires attentive
observation and quick discernment, as well as a mind not to be
biased by prejudices, to be able to learn where it has been useful
and where not.

The different accounts of its medical powers, in apparently
similar cases, are so discordant, that we are at a loss to know how
to determine. The method of administering it has probably
brought it into disrepute; the administration of it is frequently
committed to persons totally, or in a great measure, ignorant of
the science of electricity, or of the use of the machine, and who are
little interested in the result. Nor is this all ; the operator not
having patience to proceed for a considerable length of time in a
gentle exhibition of electricity, thinks that by increasing its force
he can shorten the time ; and hence, from the exhibition of strong
shocks, the patient grows worse, or at last, if it does not aggravate
the disorder, he grows tired of the painful and disagreeable opera-
tion. The medical man seeing this, often condemns or neglects
the use of electricity ; a remedy which, if properly administered,
is one of the most powerful we are acquainted with, and in many
cases one of the most useful.

* Mr. Radford has, since, substituted a bundle of wires, for the copper rod conductor :
and the magnet now carries about 22½ cwt.

That we may know in what diseases to have recourse to this remedy, we must first understand its nature, or mode of acting, which being determined we can safely say that it will be of use in all diseases whose nature is opposite to the effect of this remedy. This is one of the most certain principles we have in medicine ; viz., that when a remedy relieves or cures the disease, the operation of that remedy is contrary, or tends to produce a state of the body directly opposite to that produced by the cause of the disease ; and of these two, the nature, or modus operandi of the remedy, o the nature of the disease, either being given, we can determine the other *e. g.* if we know the disease, we can by trial, ascertain the operation of the remedy ; and if the operation of a remedy be known, we can, from its utility or disservice, determine the nature of the disease in which it is employed.

Now we know that electricity acts as a strong stimulant ; that its effects are, to increase the strength of the pulse, to promote all the secretions, and in short, to increase all the animal functions ; and we find it does this in a greater degree than most other stimulants, and therefore it may be looked upon as one of the most powerful stimulants with which we are acquainted. We shall only, therefore, expect to find it useful in those cases where the action of the body is weakened, or debilitated either in the whole body, or in a part only. In nervous affections we find this remedy has been of singular service ; on the contrary, in all those cases attended with, or depending on, an increased excitement of the system, its use will be pernicious, and we find this doctrine confirmed by experience. But before we proceed to treat of the diseases in which this remedy ought to be administered, we shall make a few observations concerning its use ; and first, where we can use other remedies along with it, we ought never to neglect them, for, in many cases, this remedy does little more than render the system more favourable to the action of other remedies ; its stimulus is so diffusible and quick, that it often favours the action of the more durable stimulants, but seldom of itself produces any permanent effect.

At first the electricity ought to be administered in the most gentle manner, particularly if the patient has not been accustomed to its use before. We ought always to begin with drawing small sparks, which may be gradually increased, if it can be done without inconvenience to the patient ; afterwards slight shocks may be used. In many cases, the patient can scarcely bear sparks, the electric fluid in this case may be drawn off in a stream by means of a wooden or metallic point, or small sparks may be drawn through flannel. When we administer electricity in diseases of the eye, as amouroses, we ought only to draw the fluid off by means of a point. Strong shocks should seldom, if ever, be administered ; for much more is to be expected from continuing them moderately for a considerable time, than from a violent application.

There is another method of administering electricity, which has
been practised, and with considerable success, viz., what has been
called the electrical bath.   The person is to be placed in an insu-
lated chair, and one part of him is to be connected by means of a
chain with the prime conductor, and another with the rubber ; by
this means we may make the electric fluid flow through any part
we please, without any pain, and frequently with the greatest
benefit : this method will not answer, unless the machine is very
powerful.

If there be any humidity, particularly of an oily nature, on the
part from which we wish to draw sparks, or pass shocks, we shall
frequently be disappointed ; in this case, the part ought to be well
washed with warm water, and afterwards wiped perfectly dry ;
otherwise our endeavours to draw sparks will be to no purpose.
We have seen several cases where the smallest spark could not be
drawn.   In those diseases where the whole body is affected, the
application of electricity ought to be as general as possible.   On
the contrary, where a part of the body only is affected, we ought
to confine the action of this remedy to that part.

We shall now just mention a few diseases in which this remedy
is useful : chronic rheumatism is frequently relieved, and sometimes
cured, by the administration of electricity.   In paralysis, it has
been generally used in those cases which admit of a cure ; it is the
most likely remedy.   We have seen it administered with the great-
est advantage, in those cases where the patient is not far advanced
in years, where we have no reason to expect lesion of the nerves,
or a depraved state of the parts, when the disease has been brought
on by cold, and is not universal, we may hope for success.   On
the contrary, when the affection is universal, and the patient ad-
vanced in years, when there is reason to believe that there is a lesion
of the nervous system, particularly in its origin, we have little to
hope for.   Though, in these cases, it is always a physician's duty
to give it a trial, and to endeavour to afford any relief, though never
so small, to his distressed patient.   There are, however, instances
upon record where paralytic affections, after many years continu-
ance, and in very old people, have been cured by the use of this
remedy.   It has been found useful in the following diseases :—
contractions, if they depend on the affection of a nerve—rigidity
has frequently been relieved after continuing the application for
some time—sprains, relaxation, if applied after the inflammation
has subsided—indolent tumours, it has been said, by Carpue in his
Introduction to Electricity and Galvanism, to be useful—chilblains
as a preventive, and some cases are recorded where they have been
cured by electric sparks—Deafness, the application of electricity to
the mastoid process generally, affords relief, and about one in five
have been cured—opacity of the cornea, a current thrown about
ten or sixteen minutes a day, on the eye from a wooden point, has
frequently cured this disease—gutta serena, it has been found very
successful in this disorder.   It is very useful in many spasmodic

diseases, particularly tetanus, chorea, or St. Vitus's dance,—but for further account of these diseases and the use of electricity, the reader is referred to a valuable paper on the subject, by Dr. Addison, in the second volume of Guy's Hospital Reports.

We shall next month give any account of several experiments, both on man, animals, and plants, with some MSS. notes on the same subject, by our learned friend J. Conley, Esq., L.L.D.

On the Electricity of Effluent Steam. By W. G. Arm- strong, Esq.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

My letters to ProfessorFaraday on the remarkable develop- ment of electricity which has recently been discovered in a jet of steam issuing from a steam-engine boiler in this neighbour- hood, having already appeared in your publication, it is, of course, unnecessary for me here to repeat the circumstances detailed in those letters. I shall therefore take up the narrative of my proceedings, relative to this curious subject, at the point at which the second of those letters concludes.

Having found electricity in all the three boilers I had ex- amined in which water from the neighbouring colliery was used, and not having discovered any indications of it in the boilers which was supplied with rain-water, I was naturally led to believe that the effects I have described were attributable to the peculiar nature of the water from which the electrical steam was produced ; and, under this impression, I lost no time in visiting some other high-pressure boilers in the same district, which were also supplied with colliery water, strongly impreg- nated with lime and other mineral matter. The steam from the safety-valves of these boilers also proved to be electrical, but not to such an extent as I had reason to anticipate from the similar- ity of the circumstances to those, under which electricity was developed in such an extraordinary degree at Seghill. I then proceeded to try a number of boilers in this town and neigh- bourhood, in which steam was propagated under different pres- sures, and from waters of various descriptions ; and by insula- ting myself and holding a conducting rod in the steam discharged from the safety-valves, I succeeded in every instance in obtaining electrical sparks, which varied in the different cases from about one-fourth to about half an inch in length.

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* Mr. Armstrong's letter here referred to, will be found in the last No. of the Annals, vol. 3, page 452.—Edit.

In company with Mr. Robert Nicholson, the engineer of the
Newcastle and North Shields railway, I next tried the boilers
of the locomotive engines used on that railway, and finding
electricity in great abundance in the ejected steam from these
boilers, I determined, with Mr. Nicholson's permission and
assistance, to institute a set of experiments upon one of them,
with a view to a fuller investigation of the subject.

I shall now briefly describe such of these experiments as
have been the most marked in their results, and shall divide
them into two classes, first taking those which were chiefly
intended to exhibit the extent to which electricity existed in
the issuing steam, and then proceeding with the experiments
which were undertaken to ascertain the cause of the electric
development. Nearly all the experiments were made at night,
under cover of the engine-shed, and the atmosphere was gene-
rally humid ; but when it happened to be otherwise, the quan-
tity of electricity derived from the jet was greatly increased.

Upon trying the steam in the first instance by the method
adopted in the previous cases, that is to say, by standing on
an insulated stool and holding with one hand a light iron rod
immediately above the safety-valve, while the steam was freely
escaping, and then advancing the other hand towards any con-
ducting body, sparks of about an inch in length were obtained :
but it was soon observed, that by elevating the rod in the
steam the electricity was gradually increased, and that the
maximum effect was not attained until the end of the rod was
raised five or six feet above the valve, at which point the length
of the sparks occasionally reached two inches. Small sparks
were even obtained when the rod was wholly removed from the
steam and held in the atmosphere at the distance of two or three
feet from the jet, and the electricity thus drawn from the air was
positive, like that of the steam. When the rod was extended
into the cloud of vapour which accumulated in the upper part
of the shed, electricity was drawn down as by a lightning-
conductor from a thunder-cloud. I endeavoured to ascertain whe-
ther any precipitation of moisture, analogous to the formation
of rain, accompanied the abstraction of electricity from the
steam, and a sprinkling of wet was undoubtedly felt on the face
and hands by the person holding the rod, so long as he remained
insulated, but the effect ceased as soon as the insulation was
destroyed.

After fully trying the steam with a simple iron rod, as a
conductor, recourse was had to other conductors which pre-
sented a larger surface to the steam, but the effect was not
materially increased until a bunch of pointed wires of different
lengths was attached to an iron rod and held in the issuing steam,
with the points presented downwards. The iron rod terminated
in a round knob at the end next the hand, and from this knob

sparks of the *measured length of four inches* were actually drawn, almost as rapidly as they could be counted, while a stream of electricity was at the same time passing off from the rod, at the part which most nearly approached the chimney of the engine. Very perceptible sparks were also obtained when the points were held in a clear atmosphere, at the distance of at least eight feet from the nearest part of the jet.

In all the preceding experiments, the effect appeared to be proportionate to the quantity of steam discharged from the valve, when other things remained the same ; and the electricity became quite imperceptible when the escape was very inconsiderable.

By abruptly raising the valve when the engine-shed was dark, the edges of the lever and margin of the brass cup which surrounded the valve, were rendered distinctly luminous with rays of positive electricity which were strongest the instant the valve was lifted, and then quickly subsided, becoming very faint after the lapse of a second.

In proceeding to investigate the cause of this extraordinary development of electricty, the first question which I proposed for inquiry was Whether does the steam first become electrical, that is to say, is it electrical in the boiler, or if not, does it become so in passing through the orifice, or not till it escapes into the air? In order to determine which of these three suppositions was correct, the apparatus represented in the annexed figure, and of which the following is a description, was employed.

A is a glass tube passing into the steam chamber through the cock B, which was screwed into a hole in the top of the boiler, and was furnished with a stuffing -box to prevent escape between the outside of the tube and inner surface of the cock ; C is a stop cock affixed to the upper end of the glass tube, and upon which cock is screwed a second glass tube D terminating in another stop-cock E.

The application of this apparatus will be easily understood. If the steam were in the same state of electricity in the boiler as when it issued into the air, it would necessarily communicate positive electricity to the insulated cock C, in passing through the tube. Or, if the steam acquired its electricity by friction, or otherwise, in the channel through which it was discharged, it could only,in the present instance, do so *at the expense* of the
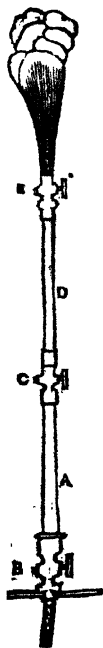
cock C, which, being insulated, would in that case indicate ne-
gative electricity.   Or, lastly, if the electricity were developed
by condensation, expansion, or any other cause which came
into operation after the steam escaped into the air, then the
cock C would have neither positive nor negative electricity.

Previously to inserting the lower glass tube in the boiler, the
steam was allowed to blow off through the large cock B, and
the jet which issued proved, to the surprise of every one pre-
sent, almost destitute of electricity.   This result completely
vitiated the inference I had drawn from the circumstance of
not finding electricity in the steam from the rain-water boiler
before alluded to, in which case, as I have already stated
in my second letter to Professor Faraday, the jet was obtained
from the gauge cock.   •

The lower glass tube, without the upper one attached to it,
was then passed into the boiler, and a highly electrical jet was
obtained from it, which communicated positive electricity to
the stop-cock C, from which the steam was discharged.   The
upper tube was accidentally broken in screwing it on to the
lower one, leaving only about three inches of glass above the
cock C.   Under these circumstances the cock C still continued
highly charged with positive electricity, and a pale lambent
light flashed at short intervals down the inside of the tube
from the cock towards the boiler.

Having replaced the broken glass tube with a new one, the
experiment was tried again on a subsequent evening, and the
jet being now removed to a much greater distance than
before from the cock C, no electricity whatever could be
detected in that cock, while the one above it indicated posi-
tive electricity in a very high degree.   It therefore became
pretty evident that the electricity was not developed until
the steam issued into the atmosphere, and that the upper stop-
cock derived its electricity from its contiguity to the jet.   One
circumstance alone seemed in some degree to militate against
this supposition, namely, that the electricity of the cock E was
greatly increased when the cock C was partially closed, as if
the expansion which in that case took place in the upper tube
rendered the steam electrical previously to its reaching the cock
from which the jet was discharged.   No negative electricity,
however, could be discerned in any part of the apparatus, and
without a developement of negative electricity I cannot see
how positive electricity can possibly arise from expansion.   The
more probable explanation of the effect appeared to be, that
the partial closing of the middle cock shortened the transparent
or non-conducting part of the jet, and thereby caused the elec-
tricity to be more readily communicated from the opake part
of the jet.

In consequence, no doubt, of increased accumulation of electricity which was thus occasioned in the highest cock, together with the unavoidable dampness of the surrounding medium, the upper glass tube, and the cock above it, became illuminated in the most singular and beautiful manner. Flashes of wavering light flickered round the exterior surface of the glass, and darted from it to the distance of three or four inches, while strong rays of electrical light streamed from the angular parts of the cock, and the flashes from the glass were accompanied by a snapping noise which was distinctly audible amidst the hissing of the steam when the ear was advanced within a short distance from the tube.

The upper glass tube was then removed, and as an additional test of the non-existence of free electricity in the interior of the boiler, a pointed wire was thrust down through the cock C and tube A into the steam, and effectual means were used to prevent the escape which would otherwise take place at the cock C, in consequence of the tap remaining open to admit the wire. Now this wire being insulated by the glass tube and communicating with the insulated cock C, must have rendered that cock electrical, if the steam were electrical in the boiler; but not the slightest indication of electricity could, under these circumstances, be found in the cock.

Having withdrawn the pointed wire from the tube, another glass tube, of which the sectional area was about ten times greater than that of the one inserted in the boiler, was then attached to the cock C, in the manner as the tube D had been before. The comparatively large bore of this tube allowed the steam to expand in a very great degree before it issued into the air, and caused it to be discharged in the state of low-pressure steam; but no diminution of electricity could be perceived in the jet, when thus attenuated; so that the electrical development does not appear to depend upon the degree of violence with which the steam comes in contact with the atmosphere.

The entire absence of negative electricity seemed to preclude the possibility of the phenomena arising from expansion, and the only remaining supposition appeared to be, that the condensation which took place in the jet, set free the electricity which the steam had absorbed in the process of evaporation. This supposition had been previously rendered probable, when it was discovered that the upper and most opake part of the jet yielded the most electricity, although I was at first inclined to attribute that circumstance to the dampness of the steam, in that part of the jet, rendering it a better conductor, and causing it to part more readily with its electricity. Experiments were next, therefore, commenced to ascertain the effect of insulating the boiler, and wholly condensing the steam; but these require repetition before they can be much relied upon. The great

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difficulty is to effect insulation amidst so much moisture, but I
have no doubt that with a little perseverance this object will be
accomplished, and I trust I shall be able to furnish, in time for
insertion in the next Number of the Philosophical Magazine,
such further results as will set the question at rest.

                                    I am, yours, &c.,
                                        WM. GEO. ARMSTRONG.
Newcastle-upon-Tyne, November 19, 1840.

(From the Philosophical Magazine)

Further Experiments on the Electricity of Steam. By H. L. PATTINSON, Esq., F.G.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Since my last letter to you, dated the 19th ult. (see page 456
vol. v. of the Annals,) relative to the electricity of steam issuing
from two boilers at Cramlington Colliery, the subject has been
further pursued both by myself and others, and sparks have
been obtained from the steam of various boilers, in every direc-
tion.   The mode of operating has generally been that described
in my letter, viz. suffering the steam to escape from the safety-
valve of the boiler tried, and testing its electricity by holding
in it a shovel or an iron rod, the operator standing upon an
insulating stool.   Sometimes the indications have been very
slight, and sometimes there has hardly been any appearance f
electricity in the steam ; but in such cases the trials have been
generally made under unfavourable circumstances, and from all
that has yet been done, the presumption is certainly that steam
is always more or less electrical.   It is not, however, always
electrical to the same extent under the same pressure, as I shall
presently show.

Mr. Armstrong was the first to experiment with a locomotive
engine-boiler (one used on the Newcastle and North Shields
railway), from which he obtained very striking results.   The
directors of the Newcastle and Carlisle railway, through their
secretary, Mr. Adamson, gave me permission to experiment
upon the boilers of the locomotive engines on that line, and I
now beg to lay before you the results I have obtained.   In
preparing for, and performing these experiments, I have, as
before, been assisted and accompanied by Mr. Henry Smith,
and I have received the most willing and efficient aid from
Mr. Anthony Hall of Blagdon, the mechanical engineer on the
railway.

1. A copper rod, half an inch in diameter, and five feet long, was provided, made hollow for lightness; this was terminated at one end by a two inch ball, and at the other (which was bent at a right angle) by ten or twelve sharp-pointed wires, spread out in every direction to collect the electricity more perfectly from the steam.

2. The Wellington locomotive engine, immediately after coming to the station with passengers, was first tried. At this time the steam was blowing forcibly out of the safety-valve, at a pressure of fifty-two pounds per inch. On holding the pointed conductor in this current of steam, with its point downwards, the individual holding it standing at the time on an insulating stool, sparks three to four inches long were given off from his person to the boiler. The sparks were largest when the valve was held down a minute or two and then suddenly lifted, so as to suffer a large volume of steam to escape with great rapidity. By this management the sparks were frequently four inches long, and occasioned considerable pain to the person on the stool, even when given from a brass ball held in his hand. The sparks were largest when the points of the conductor were held in the steam about two feet above the valve; but larger sparks were obtained when it was held much higher; and indeed sparks were obtained by holding the conductor entirely out of the cloud of steam, and at a distance from it, for the air in the wooden shed in which we operated became speedily electrical throughout. The electricity was positive.

3. The steam in the boiler was now gradually run down to see how the electrical condition would vary with the pressure. At forty pounds per inch the sparks became much less, the largest not reaching three inches. At thirty pounds the largest spark did not reach two inches; at twenty pounds it became barely an inch; at ten pounds not more than from one-fourth to one-half of an inch; and at five pounds per inch pressure the spark was hardly perceptible. But if at any pressure the valve was held down a few minutes so as to suffer the steam to accumulate and then suddenly opened, there was always a great increase, for an instant, of the electrical effects.

4. Another boiler, that of the Lightning engine, which had also just come in from a trip, and had its steam blowing off forcibly, at a pressure of fifty pounds per inch, was now tried in exactly the same way as the Wellington. On holding the pointed conductor in the steam, whether regularly blowing off at the valve or escaping with great rapidity from the sudden lifting of the valve, it did not yield a spark more than one-fourth of an inch long. We then blew a quantity of water out of the boiler of the Lightning until it barely covered the tubes inside, and on afterwards testing its steam blowing off at fifty pounds per inch, the spark was found increased to nearly two

inches in length   The steam of the Lightning was, however,
much less electrical than the steam of the Wellington at the
same pressure, under all the circumstances of our experiments.

5. The strong current of steam and water issuing from the
boiler of the lightning when the water was blown out of it as
just stated, was tested for electricity, but no indications could
be perceived whatever.

6. A very large conductor had been provided, made of zinc
two-inch tubing, in this way,—three rings were made of this
tubing, respectively three feet, two feet, and one foot diameter.
These rings were attached to each other a foot and a half apart
by side pieces, so as to form a hollow frustum of a cone, three
feet high, with ends three feet and one foot diameter respec-
tively.   The inside of this cone was laced across with copper
wire, and the whole bristled with pointed wires in every direc-
tion.   By means of a long iron bar, placed upright in a cask of
rosin (both to insulate it and to serve as a foot), and a hori-
zontal arm projecting from it, made to slide up and down on
the vertical bar, the large conductor could be placed in any
part of the cloud or steam issuing from the valve, and the elec-
tricity given off could be conveyed from it in any direction.
Care was taken to round off all parts of this conductor, so as to
avoid sharp points and angles as much as possible.   On trying
this large conductor in the current of steam from the Welling-
ton, we were disappointed to find that it did not yield a longer
spark than the small pointed copper rod with which we had
previously experimented.   The spark was larger in volume,
but it did not possess greater intensity.   It never struck through
more than three inches of space, but its effect upon the person
when taken was very violent and painful.   Our intention was
to have ascertained the rate at which large jars could be charged
from the steam, in order to form some idea of the quantity of
electricity given off; but the evening had become very damp,
and the air was so moist, that we could not procure sufficient
insulation, and we were obliged to relinquish the attempt.

7. When the large conductor was held in the cloud of steam
with its lower part or apex about two feet above the valve, it
gave off numerous and powerful sparks ; but if at this time the
points of the small conductor were placed by a person connected
with the ground in the steam below the large conductor a foot
above the valve, the electricity given off by the large conductor
was materially diminished.

8. By means of screws, the entire engine (the Wellington)
was raised off the rails and placed upon blocks of baked wood,
so as to insulate it entirely.   The steam being now blown off at
the valve, the boiler and engine became strongly electrical with
negative electricity ; points placed upon any part of the engine
exhibiting the peculiar star of the negative element, and threads

suspended from the engine being repelled by excited sealing-wax. The steam was at the same time strongly positive, and when a point connected with the conductor held in the steam was brought near a point attached to the insulated boiler, the pencil upon the former and star upon the latter were beautifully decisive as to the electrical states of each.

9. I repeated Volta's experiment by placing a hot cinder upon the cap of a gold-leaf electrometer, and projecting a few drops of water upon it, when the leaves diverged strongly with negative electricity. I observed, that when the cinder was very hot, and the production of the steam consequently very rapid, the electricity given out was always most powerful.

10. I then insulated an iron pan, twelve inches diameter and two inches deep, and attached to it a pith-ball electrometer, with balls three-eighths of an inch diameter, and threads five inches long, and also attached to the pan a metallic wire, the pointed extremity of which was placed about one-twentieth of an inch distant from the point of another wire connected with the ground. The iron pan was then filled with cinders, very hot, from a wind-furnace, and on projecting upon them a few ounces of water, steam was evolved with great rapidity, and at the same moment the pith balls diverged to the distance of an inch, and sparks passed between the metallic wires. This was several times repeated.

These experiments enable us, I conceive, to give a clear explanation of the electrical phenomena presented by steam. There is no doubt, whatever, as Dr. Faraday conjectures in his note to Mr. Armstrong's paper in your last Number, "that this evolution of electricity by vaporization is the same as that already known to philosophers on a much smaller scale." The electricity appears to originate at the instant of vaporization, and the steam as it collects within the boiler is electrified with positive electricity, the water and metallic boiler being at the same time negative. In this condition the electricity of both is latent, like the electricity of the two plates of an excited electrophorus; but the instant steam is suffered to escape, its positive electricity being carried off along with it, and out of the influence of the equivalent quantity of negative electricity in the boiler, becomes free, and hence the steam is electrical with positive electricity. The same thing takes place with the boiler, in which negative electricity is set at liberty as the steam escapes, and which becomes evident on insulating the boiler.

When steam much mixed with water, or what engine-men call "wet steam," escapes from a boiler, it evidently cannot be very highly electrical, for the negative water will tend to neutralise the positive steam, and this may perhaps in some measure account for the increased effect in the Lightning on lowering the water within its boiler, and for the increase of intensity

in every boiler, observed when the valve has been forcibly held down and is suddenly opened; but it does not seem sufficient to account *entirely* for these variations of intensity, nor for the difference of intensity in different boilers ât the same pressure. It is therefore probable that chemical action between the metal of the boiler and the water has something to do with exalting the electrical condition of the steam at the moment it is generated; but this part of the 'subject certainly requires further investigation. By far the most powerful effects up to this time have been obtained from locomotive engines, in which water is heated in contact with brass tubes. How far this may influence the production of electricity, further experiments must determine. It is certainly somewhat curious to consider the splendid locomotive engines we see daily in the light of enormous electrical machines; but this they undoubtedly are.; the steam is analagous to the glass plate of an ordinary machine, the boiler to the rubbers; and a conductor properly exposed to the escaping steam gives out torrents of electricity.

<div style="text-align:center">

I am, Gentlemen,

Your obedient servant,

H. L. PATTISON.

</div>

Bentham-Grove, Gateshead,
November 21, 1840.   *Ibid.*

<div style="text-align:center">

On Terrestrial Magnetism.   By JOHN LOCKE, M.D., of Cincinnati, Ohio.*

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Messrs. Editors—For three or four years past, most of the scientific men of our country have felt an interest in the examination of the elements of terrestrial magnetism on this continent. The increasing attention to this subject in Europe has operated sympathetically, and has excited a few of us to action. Professors Bache, Courtenay, Loomis, Jackson, and myself, have each devoted a portion of our time to actual surveys; the results, so far as they have been communicated and published, are to be found mostly in the papers of the American Philosophical Society. But few of mine have yet been made public. The field is a very broad one, and the amount of labour yet to be performed is very great. As these elements are subject to a constant and progressive change, besides an annual and diurnal fluctuation, the exact laws of which are as yet unknown, time and a multiplicity of observations at each separate station are required before any generalizations can be well established.

I have this year commenced making monthly, and sometimes even hourly, observations at this place. In our money making

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* *From Silliman's American Journal.*

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country, I can procure little or no assistance in so unprofitable a
business, and my hourly observations are almost too laborious to
be continued.   My friend and correspondent, Prof. Loomis, has
collected together such observations as have been made, and
has published them in tables and in the form of a chart in
your Journal,* but so few have been the observations, and in
them generally no attention paid to the annual diurnal changes,
that such a chart must necessarily be only an approximation to
the truth, except at the few points which have been particu-
larly examined.   In the present volume of this Journal, pp.
49, 56, † Professor Loomis has, upon rather hypothetical
grounds, marked his own observations, mine, and Professor
Courtenay's, with " apparent errors," to a considerable amount.
Now most readers will understand by this, that the results
of the observations are absolutely out of truth, or disagree
with nature to the amount noted.   A careful examination of the
article shows that this was not his meaning, for the standard by
which these " errors" are made to appear, is more questionable
than the observations themselves.   Prof. Loomis, from a com-
parison of the most ancient with the most recent observations in
our country, supposes that he has obtained the average annual
decrease of the magnetical dip in the United States.   He then
applies this quantity as a correction to previous observations up
to the present year, projects lines of " equal dip" in the direction
indicated by two or more points thus determined, and by so
much as late observations disagree with these calculations, he
has noted them in " error."   The only objection which I offer
to this mode of expressing *difference*, is that it will not generally
be understood.   There is certainly no better authority for the
dip at any particular time and place, than careful observations
made with the best instruments.   When artificial lines do not
agree with observations, it is evident that those lines should be
marked " *in error.*"   The lines of equal dip, as obtained by
actual survey, are not great circles nor uniform curves ; they
undulate irregularly, converging in some places and diverging
in others, and sometimes, I believe, one line of equal dip will
divide into two which will afterwards reunite.   It is perhaps
customary with those who make magnetical charts, to endeavour
to equalize these natural irregularities, as the engineer after a
survey for a road equalizes the hills and hollows to obtain a less
devious but more artificial survey.   Such a line, although it is
easily projected, and looks well in the chart, has no existence in
nature, and is only an artificial approximation to truth ; so far
as it departs from the results of actual observations, the line
itself should be marked in error, not the observations.   This, I
say, would be philosophical ; a conventional mode of expressing
the same relation in different terms may obtain a preference,
and would be objectionable provided it should be properly
understood.

* Vide Silliman's Journal, Vol. xxxiv, p. 290, Vol. xxxix, p. 41.
† *Ibid.*

The largest number of my magnetical experiments were made in Iowa and Wisconsin Territories during last autumn, and the general results, including both the dip and intensity, have been communicated both to Congress and to the American Philosophical Society. But I am reminded by the above suggestions that I ought to lay before the public a specimen of the details at large, that a proper judgment may be formed of the degree of credit to which they are entitled. I shall confine myself at present to that part of my observations for determining the dip. My dipping compass was made by Robinson, of London, in 1827. As a check upon errors, I make at each station, by means of two separate needles, a double suite of observations. In each suite all the usual reversals are made, including the face of the instrument, the face of the needle, and the polarity of the needle by retouching. The dip is therefore determined by eight distinct readings of each needle, the results almost always agreeing within one or two minutes of a degree. The mean of the whole of the sixteen readings is finally taken. The following are examples:—

No. 1. Davenport, Iowa Territory, September 15, 1839. Lat. 41° 30' N., Lon. 90° 18' W.

Needle No. 1.	B. North.		Needle No. 2.	B. North.	
Face of instrument.	Face of needle.	Dip indicated;	Face of instrument.	Face of Needle.	Dip indicated.
E.	E.	72°05'	E.	E.	71°58'
W.	W.	70 47	W.	W.	72 02
W.	E.	72 47	W.	E.	71 54
E.	W.	71 06	E.	W.	72 03
	A North.			A North.	
E.	E.	70 59	E.	E.	72 16
W.	W.	72 52	W.	W.	71 26
W.	E.	70 53	W.	E.	72 12.5
E.	W.	72 57	E.	W.	71 28.5
		8)574 d6			8)575 20
		Mean, 71 48.25			Mean, 71 55

No. 2. Davenport, Iowa Territory, September 18, 1839.

Needle No. 1.	B. North.		Needle No. 2.	A. North.	
Face of instrument.	Face of needle.	Dip indicated.	Face of instrument.	Face of needle.	Dip indicated.
E.	E.	73°05'	E.	E.	72°18'
W.	W.	70 43	W.	W.	71 26
W.	E.	72 51	W.	E.	72 14
E.	W.	70 40	E.	W.	71 25
	A North.			B North.	
E.	E.	70 57	E.	E.	71 41
W.	W.	73 14	W.	W.	72 19
W.	E.	70 50	W.	E.	71 30
E.	W.	73 04	E.	W.	72 27
		8)575 24			8)575 20
		Mean, 71 55.5			Mean, 71 55

No. 3. *Lost. Grove,* (*Iowa,*) *Sept.* 23, 1839. *Lat.* 41° 39' *N.,* *Lon.* 90° 09' *W.*

Needle No. 1.	A North.		Needle No. 2.	B North.	
Face of instrument.	Face of needle.	Dip indicated.	Face of instrument.	Face of needle	Dip indicated.
E	E	71°07'	E	E	71°43'
W	W	73 16	W	W	72 36
W	E	70 56	W	E	71 45
E	W	73 19	E	W	72 39
	B North.			A North.	
E	E	72 59	E	E	72 11
W	W	70 50	W	W	71 34
W	E	73 01	W	E	72 05
E	W	70 54	E	W	71 44
		8)576 22			8)576 17
	Mean,	72 02•75		Mean,	72 02.125

No. 4. *Wapsipinnicon River, Sept.* 25, 1839. *Lat.* 41° 45' *N.,* *Lon.* 90° 23' *W*

Needle No. 1.	B North.		Needle No. 2.	A North.	
Face of instrument.	Face of needle.	Dip indicated.	Face of instrument.	Face of needle.	Dip indicated.
E	E	73°16'	E	E	72°23'
W	W	71 07	W	W	71 48
W	E	73 17	W	E	72 18
E	W	71 10	E	W	71 57
	A North.			B North.	
E	E	71 15	E	E	72 15
W	W	73 20	W	W	72 27
W	E	71 10	W	E	72 12
E	W	73 27	E	W	72 37
		8)578 02			8)577 57
	Mean,	72 15.25		Mean,	72 14.625

No. 5. *Brown's Settlement, Iowa, Sept.* 20, 1839. *Lat.* 42°04' *N.,* *Lon.* 91° 02' *W*

Needle No. 1.	B North.		Needle No. 2.	A North.	
Face of instrument.	Face of needle.	Dip indicated.	Face of instrument.	Face of needle	Dip indicated
E	E	73°21'	E	E	72°25'
W	W	71 18	W	W	71 53
W	E	73 14	W	E	72 25
E	W	71 26	E	W	72 10
	A North.			B North.	
E	E	71 26	E	E	72 33
W	W	73 22	W	W	72 25
W	E	71 19	W	E	72 28
E	W	73 32	E	W	72 26
		8)578 58			8)578 45
	Mean,	72 22.25		Mean,	72 20.625

No 6 ☿ Augusta River, (Iowa,) October 1, 1839. Lat 42° 14' N, Lon 90° 57' W

Needle No. 1.	A North.		Needle No. 2.	B North.	
Face of instrument	Face of needle	Dip indicated	Face of instrument	Face of Needle	Dip indicated.
E.	E.	71°42'	E	E.	72°48'
W.	W.	73 47	W	W	72 46
W.	E.	71 40	W	E	72 45
E.	W.	74 00	E	W	72 51
	B North.			A North.	
E.	E.	73 35	E	E	72 52
W.	W.	71 44	W	W	72 25
W.	E.	73 39	W	E	72 44
E.	W.	71 45	E	W	72 35
		8)581 52			8)581 46
	Mean, 72 44			Mean, 72 43.25	

No. 7. Farmer's Creek, (Iowa,) October 3, 1839. Lat. 42° 13' N., Lon. 90° 23'

Needle No. 1.	B. North.		Needle No. 2.	A. North.	
Face of instrument.	Face of needle.	Dip indicated	Face of instrument.	Face of Needle.	Dip indicated.
E	E	71°27'	E	E	72°15'
W	W	73 24	W	W	72 36
W	E	71 26	W	E	72 16
E.	W	73 22	E	W	72 36
	A North.			B North.	
E	E	73 44	E	E	72 43
W	W	71 42	W	W	72 37
W	E	73 51	W	E	72 47
E	W	71 30	E	W	72 33
		8)580 26			8)580 23
	Mean, 72 33.25			Mean, 72 32.875	

No. 8. Prairie du Chien, W. T, Oct 24, 1839. Lat 43° 68' N, Lon 90° 52' W.

Needle No. 1.	A. North.		Needle No. 2.	B North.	
Face of instrument.	Face of needle.	Dip indicated.	Face of instrument.	Face of needle.	Dip indicated,
E	E	72°25'	E	E	73°25'
W	W	74 17	W	W	73 22
W	E	72 05	W	E	73 21
E	W	74 32	E	W	73 25
	B North.			A North.	
E	E	74 33	E	E	73 26.5
W	W	71 53	W	W	73 02
W	E	74 24	W	E	73 14.5
E	W	72 02	E	W	72 58.5
		8)586 11			8)586 14.5
	Mean, 73 16.375			Mean, 73 16.875	

No 9 *Blue Mounds*, (*W T*) October 30, 1839 *Lat 43° 01' N, Lon 89° 36' W*

Face of instrument	Face of Needle	Dip indicated	Face of instrument	Face of needle	Dip indicated
Needle No. 1.	**B North.**		**Needle No. 2.**	**A North.**	
e	e	74°59'	e	e	73°43'
w	w	72 14	w	w	73 22.5
w	e	74 47.5	w	e	73 42
e	w	72 21	e	w	73 31.5
A North.			**B North.**		
e	e	73 08.5	e	e	73 48
w	w	74 21.5	w	w	73 46
w	e	73 03	w	e	73 40
e	w	74 35	e	w	73 52.5
		8)589 29.5			8)589 25.5
		Mean, 73 41.2			Mean, 73 40.6875

No. 10. *Madison*, (*W. T.*) *Nov.* 2, 1839. *Lat.* 43° 05' *N.*, *Lon.* 89° 06' *W*

Face of instrument	Face of Needle	Dip indicated	Face of instrument	Face of needle	Dip indicated
Needle No. 1.	**A North**		**Needle No. 2.**	**B North.**	
e	e	73°28'	e	e	74°11'.5
w	w	74 49	w	w	74 11
w	e	73 24	w	e	74 01.5
e	w	74 56	e	w	74 16.5
B North.			**A North.**		
e	e	74 59	e	e	74 12
w	w	73 00	w	w	73 47
w	e	75 02.5	w	e	74 02
e	w	72 47	e	w	73 49.5
		8)592 25.5			8)592 31
		Mean, 74 03.1275			Mean, 74 03.875

No. 11. *Mineral Point*, (*W. T.*) *Nov.* 1839. *Lat.* 42° 50' *N.*, *Lon.* 89°54' *W*

Face of instrument	Face of Needle	Dip indicated	Face of instrument	Face of Needle	Dip indicated
Needle No. 1.	**B North.**		**Needle No. 2.**	**A North.**	
e	e	74°30'	e	e	73°26'.5
w	w	72 10.5	w	w	73 09
w	e	74 22	w	e	73 18.5
e	w	72 22.5	e	w	73 12.5
A North.			**B North.**		
e	e	72 31.5	e	e	73 33.5
w	w	74 10	w	w	73 15
w	e	72 12.5	w	e	73 27.5
e	w	74 26.5	e	w	73 21.5
		8)586 45.5			8)586 44
		Mean, 73 20.6875			Mean, 73 20.5

The above specimens of my field notes are not selected, but are taken in course from one to seven. Those which follow are also in course, and have been selected because they differ from what would be expected from the projections of isoclinal lines. The evidence of the accuracy of the observations rests chiefly on the very close agreement of the independent results obtained by the two separate needles. It will be seen that out of the eleven cases above quoted, there is but one in which the difference is over a fraction of one minute of a degree, and that is the fifth, which shows a difference of one minute and five eighths of a minute. If there were a certain latitude of error, say five minutes, it is evident, by the calculation of chances, that such error between the two needles, would as often be doubled by being, the one plus and the other minus, as it would be merged by both being plus or both minus; and hence half of the greatest difference by the two needles may be taken as the limit of instrumental errors, which in the above observations would be only 0'.8625 of a minute; a quantity much smaller than I should have anticipated before making the examination. The instrument is evidently a very perfect one, yet at certain points, when the dip arrives at a particular quantity, probably from a want of perfect roundness of the pivots, one needle will read constantly and uniformly more than the other. Thus at Dubuque, where the dip is 73° 04', needle No. 1, read in the mean, constantly 2½' more than No. 2. Even this error would ordinarily be considered very small. The French have a saying that "the dipping compass is one very ungrateful instrument." But with this fine piece of workmanship of Robinson's, I have repeatedly admired the beautiful manner in which the reversals correct all of the errors, and the two needles, none of whose individual readings are alike, will ultimately in the mean, give almost identical results.

In my surveys, I did not expect the dip to be so little at Prairie du Chien, nor so great at Madison, Wisconsin Territory, as I found it. I went through with four suits of observations at the former place, before I was satisfied of their correctness. But all of the observations between Prairie du Chien and the four lakes, agree in determining that the lines of equal dip along the Wisconsin river, in advancing westward, incline rapidly to the north. From a point about five miles south of Mineral Point, the line of dip for 73° 16' passes to Prairie du Chien, in the direction of west 22° 10' north. The curvature between Madison and Blue Mounds, is probably still more to the north. At Dubuque, however, there was no evidence of such a northern inclination of the lines of equal dip. At Columbus, Ohio, the dip appeared to be so much less than I expected, that after determining it twice at our station, suspecting local attraction, I removed to another a mile distant, but the

result was still the same. At the very points marked by Prof. Loomis, I anticipated the departure of the magnetical quantities from their general direction, and was especially cautious in my examinations, but finally was compelled to record their results in obedience to the authority of nature. I believe there are so many anomalies in the elements of terrestrial magnetism, that the only safe way in proceeding with our surveys, is to observe industriously, and put down carefully, the results of experiments, without any reference to artificial lines, until we have dotted the map pretty thickly over with our records, and then see into what forms they will arrange themselves.

I have thus laid before your readers so much of my field notes as will enable them to understand in general my mode of operating, and have presented to them the evidence which convinces me that the results, at the time and place given, were accurate within at least two or three minutes of a degree. By this means I hope to inspire that confidence which alone gives interest to such researches. The papers of Prof. Loomis are well calculated to draw popular attention to this very interesting subject, and we hope that a science which has been considered of sufficient importance in foreign countries to induce their governments to erect observatories, supply them with instruments and observers, and even to fit out naval expeditions to explore distant regions for the advancement of its interests, will not soon be neglected by its few votaries in this country, or be so far overlooked by the great body of our community, that all encouragement to its cultivation will be withheld.

Here I had intended to bring my remarks to a close; but on reviewing them I perceive there is a possibility that some of them may be understood as a censure upon my friend Prof. Loomis. Nothing of this kind is intended. There is no difference of opinion between him and myself as to the facts. It is merely the manner of representing a fact which has elicited the remarks. A difference exists between an artificial line and a quantity determined in fact; call the one A and the other B. The question is then merely, is it more expedient to assume A to be the standard and mark B in error, or to assume B as the standard and mark A in error? I object to the choice which Prof. Loomis has made, because it will give to most of your readers, such as are not magneticians, the impression that both Prof. Loomis, Prof. Courtenay, Capt. Sabine, myself and others, are unable to determine the dip within a very great latitude of error. But Prof. Loomis did not originate this mode of expressing the difference of the two quantities; he had the precedent established by the most able foreign magneticians. It may be that conventional authority in this case, as in numerous others, ought to prevail, and that Prof. Loomis is right in conforming to that authority. Still we hope the discussion of the subject

is not without its use to a large class of readers, by making
them more familiar with some of the minutiæ of an interesting
and in many respects a new science, and by inspiring them
with greater confidence in the degree of perfection to which
magneticians have arrived in their observations.   They will, I
hope, feel more interest, in the observations of Prof. Loomis
himself, who is furnished with the best of instruments, not only
in magnetism, but in meteorology and astronomy, and is in all
of these departments an industrious and accurate observer.

Could the necessary labor be performed, such charts as would
exhibit the lines of equal dip, equal variation, and equal inten-
sity in all of their various windings, including all of the so
called local influences, minutely true and faithful to nature, I
believe some new generalizations would be obtained.   Possibly
it might appear that particular geological formations are asso-
ciated with some peculiarities of magnetism.   There was an
indication of this kind in the survey of Iowa, to the Report of
which the reader is referred.   But to establish a generalization
requires the concurrence of numerous instances of the same
kind; the change of magnetism with a change of minerals
might in a single instance be accidental.

Medical College of Ohio, at Cincinnati, July 30, 1840.   *Silliman's Journal*

**On the identity of Edwardsite with Monazite (Mengite,) and
on the Composition of the Missouri Meteorite.** By CHARLES
UPHAM SHEPARD, Professor of Chemistry in the Medical
College of South Carolina.

The Journal of the Franklin Institute for May, 1840, contains
the following translation from Poggendorff's *Annalen der Physik
und Chemie,* No. I, 1840, of an article by Gustavus Rose, on the
identity of Edwardsite and Monazite,

"The royal collection at Berlin received a fragment of gneiss
from Norwich, in Connecticut, containing a part of a crystal of
Edwardsite, which although fractured on either termination, had a
sufficient number of planes remaining to determine its angles,
Shepard (American Journal of Science and Arts, xxxii, 162)
correctly referred this mineral to the oblique-rhombic system, and
added that the prism was terminated by a four-sided pyramid.
He observed that the cleavage was sometimes perfect, but generally
uneven parallel to the oblique terminal plane, but very perfect
parallel to the longer diagonal.   He further remarked that
it bore the closest resemblance to zircon, which the Munazite was
supposed to be by *Menge,* who first found it in the Ilmen branch
of the Uralian chain. The few measurements of Edwardsite nearly
correspond with those of Monazite, excepting the inclination of
an oblique terminal plane to the plane replacing the obtuse lateral
edge, which, with Monazite, give an angle of 100° 3', with Ed-
wardsite 103° 58', but the calculation of the former was grounded

on imperfect measurements. In regard to form, therefore, the two minerals correspond.

" They also resemble each other in relation to other properties. Colour, hyacinth red to reddish brown, the lustre of Edwardsite somewhat stronger; hardness = 5 (apatite). Specific gravity of Edwardsite is rated too low by Shepard = 4.2 to 4.6,—that of Monazite, according to Breithaupt = 4.992 to 5.079. In behaviour before the blowpipe alone, or with fluxes, both alike, both infusible. Shepard observes that the former fuses with great difficulty on the edges, but no such fusion was observable on the specimen in the royal cabinet. There are some differences in their behaviour with acids, the former, according to Shepard, being slightly affected by aqua regia, the Monazite, according to Kersten being decomposed by chlorohydric acid with the evolution of chlorine.

" The apparent differences of their chemical composition may be reconciled. The Monazite was analyzed by Kersten, the Edwardsite by Shepard.

Monazite.		Edwardsite.	
Peroxide of cerium,	26 00	Peroxide of cerium,	56.53
Oxide of lanthanium,	23.40	Zirconia.	7.77
Thorina,	17.95	Alumina,	4.44
Peroxide of tin,	2 10	Silica.	3.23
Protoxide of manganese,	1.86	Protoxide of iron, ⎫	
Lime,	1.68	Glucina, ⎬	traces.
Titanic acid, ⎫	traces.	Magnesia, ⎭	
Potassa, ⎭		Phosphoric acid,	26.66
Phosphoric acid,	28.50	•	
	98.49*		98.73

" The chief differences then are that Monazite contains both oxide of cerium and lanthanium, the Edwardsite only peroxide of cerium, (Shepard gives protoxide,) that the former contains thorina, the latter zirconia. Lanthanium is probably contained in Edwardsite, as it generally accompanies cerium, having been first discovered during the past year by Mosander, (was unknown to Shepard.) In regard to thorina and zirconia, it can hardly be assumed that the given quantities are correct, since we have no accurate method of separating them from oxide of cerium; it is nevertheless worthy of notice that 7.77 zirconia are a nearly full equivalent for 17.95 thorina, for the former contains 2.04, the thorina 2.12 oxygen. It might, therefore, be supposed that the thorina is replaced by zirconia in Edwardsite, which, however, cannot be assumed from the present view of their atomic composition, since, according to Berzelius, thorina is expressed by Th —|— O, zirconia, by 2 Zr —|— 3O. The tin in Monazite is

*There is evidently some error in the figures of this analysis, for the sum of those given is 101.49.

evidently accidental from its minuteness remarkably enough, as Rose remarks, he found it also in Edwardsite, by means of the blow-pipe. If the presence of zirconia in Edwardsite be confirmed and its isomorphy with thorina, then these two minerals can only be separated as species; if not, then both will probably agree in their chemical composition; in which case, it will be more proper to retain the name Monazite, which it first received."

It is proper in the first place to observe that Monazite is the same mineral as that described by Mr. H. J. Brooke under the name of Mengite in the Philosophical Magazine and Annals for September 1831, (p. 189.) Having received from this gentleman a good crystal of the Uralian mineral, and being very forcibly struck by the considerations presented in the foregoing paper, I instantly set about such an examination of the Edwardsite as the nature of the case solicited from my hands.

Their identity in crystalline form appears to be nearly complete. Brooke gives M on M 95° 30', and I find the crystals of Edwardsite to measure from 95° to 95° 30'. Again his angle between the base (P) and the prism (M) corresponds exactly with mine as given in my first paper on the Edwardsite. Being unwilling to fracture my crystal of Monazite to learn its cleavages, I can only add on this head that lines, or rifts of diagonal cleavage are very conspicuous in it, in exact accordance with those which are so striking in the American mineral.

The discrepancy in specific gravity between the two minerals disappears on subjecting larger crystals of Edwardsite to examination. I obtained on a fragment weighing 2½ grs. the specific gravity of 5.00; whereas the Monazite crystal, whose weight is 3 grs. equals only 4.61. It will no doubt be a difficult point to establish an exact agreement between the two, since the specimens to be examined are not only exceedingly minute, but much entangled with other substances, as mica, tin ore, æschenite, &c.

After proceeding thus far in the examination, I felt but little hesitation in concluding that the analogy would be found to hold further, and extend to an identity in chemical composition; the more so, as I distinctly remembered several ambiguous and nearly irreconcilable circumstances connected with it.

All the Edwardsite I could collect by breaking up numerous specimens of the rock amounted to but 5.1 grains. In examining each fragment in order to separate foreign matters prior to pulverigation, I detected one very perfect crystal of zircon, (of the form *binotrianilaire*, fig. 495 of my Treatise,) which taken with

the fact that they have frequently been observed in proximity to the Edwardsite, leads me to attribute in part the zirconia of my former analysis to this source.

My present object was not so much to determine the number and proportions of the ingredients as to ascertain whether the oxide of lanthanium and thorina are constituents of the mineral. It was heated to whiteness for half an hour, with twice its weight of anhydrous carbonate of soda. The mixture fused into a hard, yellowish gray compact mass, which was treated with boiling water and the insoluble part separated on the filter.

The alkaline fluid was supersaturated with acetic acid, and precipitated by acetate of lead. The phosphate of lead was ignited and weighed 7·5 grs., which is equivalent to 1.38 grs. phosphoric acid, or 27.04 per cent.

The insoluble matter from the aqueous solution of the calcined mineral was now treated with nitro-hyhrochloric acid, and digested for several hours. The insoluble portion was separated, ignited and weighed. It amounted to 2 grs. Its color was reddish brown. Believing it still to contain oxide of cerium, I subjected it to a new calcination with carbonate of soda, in order to its more complete decomposition. It had the desired effect; for after the digestion of the calcined mass afresh in *aqua regia* for two hours, the insoluble matter was reduced to 1 gr. which still retained, however, a pale tinge of red, evincing that it was not wholly deprived of the oxide of cerium, or lanthanium, or of both.

This powder was now treated with concentrated sulphuric acid, diluted with its weight of water. A solution was effected with some difficulty, requiring for its completion a digestion of at least two hours. Nothing remained behind, save a feeble trace of titanic acid. The colour of the fluid was yellow.

It exhibited the following properties: ammonia threw down a white hydrate, which absorbed carbonic acid from the air, and was really soluble again in hydrochloric acid, with effervescence. Ferrocyanide of potassium threw down a precipitate when added to the neutral sulphate, which it does not do in the case of zirconia alone. From these facts, I think it safe to infer that the solution in question contained principally thorina.

The nitro-hydrochloric solutions of cerium above mentioned, were mingled and precipitated by ammonia. The precipitate was dissolved in nitric acid, and the solution evaporated to dryness. It assumed a rose red, and had a decided astringent, but metallic taste. A solution of hydrochlorite of ammonia was added, and on the application of heat, the insolubile oxide was dissolved with the evolution of ammoniacal gas. I feel fully authorised, therefore, to announce the existence of the oxide of lanthanium in the Edwardsite; but as to the ratio which it bears to the oxide of cerium, I was unable to determine anything satisfactorily.

Whenever I am able to procure a sufficient quantity of the
mineral, I shall renew the research into its composition; but in
the mean time, I am sufficiently satisfied of its relationship to
Mengite, to withdraw the claim I at first advanced to its distinct
specific character.

By the above investigation, new elements are added to those
already known in the State of Connecticut.   Mr Rose detected
tin also by the blowpipe in Edwardsite.   I may add that I have
the same metal from two other places in the state, an account of
which, together with a notice of selenium which accompanies
the tin at one of its deposits, I reserve for a future occasion.   The
list of our elements has therefore been augmented to the number
of four, within a short period of time.

·I detect the Edwardsite in a solitary crystal at the original lo-
cality of Sillimanite, in the town‹ of Chester, upon the Connecti-
cut river; but it does not appear probable that this will prove so
abundant a source of the mineral as the deposit at Norwich.

·   I bid

Analysis of Meteoric Stone, which fell near Little Piney, Missouri, Feb. 13th, 1839.

This specimen was obtained by Mr. Forrest Shepherd, and
described by Mr. E. C. Herrick, in this Journal, Vol. xxxvii. p.
385.   Mr. Shepherd kindly placed the mass at my disposal, which
enables me to extend the account already published by the
following notice :—

On first inspection, the stone appears rather compact and close-
grained ; it is nevertheless composed for about one half of small
imperfectly defined globules of the mineral which has been called
meteoric olivine.   In colour they are light gray, inclining to
pearl-gray, and when freshly broken across, show tints of yellow
and green.   The remaining stony ingredient is white and semi-
decomposed, resembling the feldspathic mineral in certain trachytic
lavas.

Through the whole is sprinkled meteoric iron in little shining
points, which are often invested with a coating of magnetic iron
pyrites.   By the aid of a glass, a few little black points were
discovered of a mineral which appeared to be chrome-iron ore.

Notwithstanding the apparent firmness of the mass, arising out
of its close-grained structure, it is still possessed of but little
cohesion, since a slight strain of the fingers is sufficient to produce
a fracture, even in a rounded shaped fragment of the stone.   When
broken up in this manner, however, the pieces are not prone to
separate still farther, so as easily to give rise to a powder.

The meteoric iron is not tarnished by exposure to the air.   It
was examined for chlorine, without affording any traces of this

element. The most striking peculiarity found in this stone, was the small proportion of nickel. At first I failed to detect it altogether, but on a repetition of the search with eight grains of the alloy, whose nitro-hydrochloric solution in a concentrated form was decomposed by ammonia in excess, I noticed an exceedingly faint blue tinge in the fluid. The chromium, however, is more abundant than usual, amounting to above 3 p. c.* I did not search for tin or manganese.

The following is a summary of the results obtained :—

Silicic acid,..........................	31.37	⎫
Magnesia, ...	25.88	⎪
Protoxide of iron, ...	17.25	⎬ Earthy portion.
Alumina,....... ....,...............	.49	⎪
Soda, ...	traces.	⎭
Iron.	16.	⎫
Cobalt, ⎫		⎪
Chromium, ⎬	4.28	⎬ Meteoric iron.
Nickle, ⎭	traces.	⎪
Sulphur, (phosphorus?) and loss,	4.73	⎭
	100.00	Ibid

BRITISH ASSOCIATION PROCEEDINGS

AT GLASGOW, 1840.

On the Cause of the Increase of Colour by the Inversion of the Head. By Sir DAVID BREWSTER.

It has been long known to all artists and tourists, that the colours of external objects, and particularly of natural scenery, are greatly augmented by viewing them with the head bent down and looking backwards between the feet, that is, by the inversion of the head. The colours of the western sky, and the blue and purple tints of distant mountain scenery, are thus beautifully developed. The position of the head, however, which I have described, is a very inconvenient one; but the effect may be produced, nearly to the same extent, by inverting the head so far as to look at the landscape backwards beneath the thighs or left arm. It is not easy to describe in any precise language, the degree of increase which the colours of natural scenery thus receive; but an idea may be formed of it from

the fact that the colours of distant mountains, which appear
tame and of a French grey colour when viewed with the head
erect, appear of a brilliant blue or purple tint with the head
inverted. I am not aware of any author, except Sir John
Herschel, having attempted to explain this phenomenon. He
has, if I rightly recollect, done this in his work " On Light;"
but whether it is in that work or not, I remember well, that he
ascribes the increase of the colour to the circumstance that the
inversion of the head causes the pictures of the coloured objects
to fall upon a part of the retina not accustomed to the exercise
of vision, and therefore less fatigued by the impressions of
external objects : in the same manner as when we look long at
coloured objects, the brilliancy of their colour, or of any adja-
cent object is greatly diminished. An incidental observation
led me to suspect the accuracy of this explanation, and upon
inverting the landscape by reflection, I found that no increase
of colour took place. I then viewed the inverted landscape
with the head inverted, and found the colour to be increased as
before. Hence, it appears that the increase of colour is not
owing to the simple inversion of the object, or to our viewing
it under unusual circumstances. That the augmentation of
tint is not owing, as Sir John Herschel supposes, to the impres-
sion falling upon a part of the retina not so much accustomed
to receive such impressions, is obvious from the fact that the
tint is the same upon whatever part of the retina the image
falls ; and it is easy to see, that the very same part of the retina
is affected, whether we look at an object with the head upwards
or downwards, or in any other position, provided we look at it
directly. In order to acquire some information on this subject,
I requested a friend who was unacquainted with any theoretical
views that had been advanced, to make some observations on
the change of colour of distant mountains. The result of
these was to convince him that the increase of tint arose from
the protection of the eye from lateral light, owing to the position
of the head when inverted. On submitting the opinion to
examination, I found that the tint was not increased by pro-
tecting the eye from lateral rays, even to a much greater extent
than is done by the inversion or inclination of the head ; and,
therefore, that this could not be the cause of the increase of
colour. In this perplexity about the cause of the phenomenon
in question, I had an opportunity of observing the great in-
crease of light which took place in an eye in a state of inflam-
mation. This increase was such, that objects seen by the sound
eye appeared as if illuminated by twilight, while those seen by
the inflamed eye, seemed as if they were illuminated by the
direct rays of the sun. All coloured objects had the intensity
of their colours proportionally augmented ; and I was thus led
to believe, that the increase of colour produced by the partial

or total inversion of the head, arose from the increased quantity of blood thrown into the vessels or the eye-ball—the increased pressure thus produced upon the retina ; and from the increased sensibility thus given to the sentient membrane. Subsequent observations have confirmed this opinion, and though I cannot pretend to have demonstrated it, I have no hesitation• in expressing it as my conviction, that•the apparent increase of tint to which I have referred, is not an optical, but a physiological phenomenon. If this is the case, we are furnished with a principle which may enable us not only to appreciate faint tints, which cannot otherwise be recognised, but to perceive small objects which, with our best telescopes, might be otherwise invisible. •

On the Illumination of Microscopic Objects. By Dr. BREWSTER

Considering a perfect microscope as consisting of two parts, viz. an illuminating apparatus, and a magnifying apparatus, he stated that it was of more consequence that the illuminating apparatus should be perfect, than that the magnifying apparatus should be so ; and that the essential part of his method consisted in this, that the rays which form the illuminating image or disc shall have their foci exactly on the part of the microscopic object to be observed, so that the illuminating rays may radiate as it were from the object, as if it were self-luminous. Now, this can be only well obtained, by illuminating with a single lens, or a system of lenses, without spherical or chromatic aberration, whose focal length, either real or equivalent, is less than the focal length of the object-glass of the microscope. The smaller the focal length of the illuminating lens or system of lenses, the more completely do we secure the condition that the illuminating rays shall not come to a focus either before they reach the object, or after they have passed it. When Dr. Wollaston recommended for an illuminating lens, one of three-fourths of an inch in focal length, in which the microscopic object was placed in a vertex of foci, where the rays crossed in a thousand points both before and after they fell upon the object, he could have had no idea of the new method of illumination. In the construction of a perfect microscope, Sir David Brewster recommended that the illuminating and magnifying apparatus should have seperate and similar movements along the same rod or bar, and that the stage for the objects should be unconnected with both, and should also have a motion independent of both. •

The improvement which he communicated on the Polarizing Microscope, was one which he had used for several years, and

which consists in placing the analysing prism or simple rhomb
immediately behind the object glass, that is, on the side of the
object-glass next the eye.   The great inconvenience of placing
it between the eye-glass and the eye, had induced several skilful
observers to reject the prism altogether as an analyser, or to
substitute for it a plate of tourmaline, which is quite unfit for
any observations in which colour is to be considered.   The
analysing prism may remain constantly on the microscope
behind the object-glass, without in the least injuring the per-
formance of the microscope, and it should have a motion of
rotation independent of the body of the microscope.

On a New Method of Ascertaining the Refractive Powers of Minute Bodies, and its Application to Mineralogy.   By ALEXANDER BRYSON.

The means at present employed in ascertaining the refractive
powers of crystalline substances, rendered it necessary to pro-
cure pieces not less than a quarter of an inch in size, which
were then to be ground into prisms, before any idea of their
refractive powers could be obtained.   The microscope is well
suited, with a slight alteration, to give minute differences in
refractive powers.   On the stage of the microscope is placed a
piece of crown-glass, with fine lines drawn on its first surface.
If a piece of beryl, or any other mineral with parallel sides, is
now placed on the glass, the lines will no longer be visible
through the microscope, until it is raised above the crystal three
hundredths of an inch.   The difference of focus becomes an
index of the difference of refractive power, between the glass-
plate and the chrystal.   The means adopted to ascertain minute
changes in focal length, is a scale of hundredths of an inch,
with a vernier dividing it into thousandth parts.

On the Preparation of Alloxan, Alloxantine, Thionurate of Ammonia, Uramile, and Murexide.   By Prof. GREGORY.

To prepare alloxan from uric acid, Liebig and Wöhler used
nitric acid sp. g. 1.42, and separated the acid liquid from the
crystals by means of a porous brick, thus losing the whole
mother liquid.   The author uses nitric acid of sp. g. 1.35.
The action of this acid on uric acid must be kept moderate.
When chrystals of alloxan are formed, the whole is thrown on
a filter, the throat of which is stopped with asbestus.   That
portion of the acid liquid which remains in the crystals is dis-
placed by a few drops of cold water, and the crystals are purified

by re-crystallization. The liquid is again employed in the same way, and the crystals collected as before. Five such operations may be performed with the same liquid, each yielding a large crop of crystals; while the mother liquid is preserved, and yields a large quantity of parabanic acid, or oxalurate of ammonia. By this process the author obtains from 100 parts of uric acid, 65 of anhydrous alloxan, or 90 of alloxan—|— 6 aq. From alloxan, alloxantine is easily obtained by the action of sulphuretted hydrogen. Thionurate of ammonia is easily formed, by boiling a solution of alloxan with sulphite of ammonia and free ammonia. Uramile is also easily obtained by boiling a solution of thionurate of ammonia, with an excess of diluted sulphuric acid. Murexide is obtained, as described by Prof. Gregory on the Thursday (p. 742.) He now exhibited the last three processes. He also stated, that the theory of the formation of murexide was of great importance in reference to organic colouring matters.

On the Application of Native Alloy, for Compass Pivots.
By Captain E. J. Johnson, R.N.

Among those portions of a ship's compass which most affect its working, are the pivots and caps on which the needle and card traverse, and which, like the balance of a chronometer (but of far more importance to the practical navigator), should not only be fitted with the most scrupulous attention to accuracy, but be made of materials capable of maintaining a given form under the trials to which such instruments are necessarily exposed. Having examined a great variety of compasses which had been used at sea, wherein Captain Johnson noticed that their pivots were generally injured and often by rust, he searched numerous records of experiments for its prevention, and for improving the quality of steel in other respects, by means of alloys of platinum, palladium, silver, &c. (he alluded particularly to the experiments of Dr. Faraday and Mr. Stoddart); and Mr. Pepys having obligingly supplied Captain Johnson with specimens of similar kinds of steel to those used by them, these examples, together with pivots made of the ordinary steel, and hardened and tempered in the manner recommended by eminent instrument-makers, were placed in a frame for experiment; and to these again Captain Johnson added certain contrivances of his own, such as rubbing a steel pivot with sal.ammoniac, then dipping it into zinc in a state of fusion, and afterwards changing the extreme point. Some specimens he coated with a mixture of powdered zinc, oil of tar, and turpentine; and others again were set in zinc pillars, having small zinc caps, through which

the extreme point of the pivot protruded after the manner of black lead through pencil tubes. The whole of the specimens were then placed in a cellar, occasionally exposed to the open air, examined from time to time, during more than half-a-year, and their several states, as respected oxidation, duly registered. Without going into details of this register, the general result was, that not any of the kinds of steel pivots used in this trial, except such as were coated with zinc, remained free from rust, while the pivot made of the "native alloy" which is found with platinum, completely retained its brilliancy. Captain Johnson then applied a more severe test to this singular substance, first, by placing sulphuric acid, and then nitro-muriatic acid upon it; but even under this trial he could not observe that any change had been effected, although the blade of a penknife, subjected to a similar process, was rusted to the centre. Having enumerated the facts respecting the trials to which he had subjected this curious material, Captain Johnson stated the conclusions that he had come to, namely; that it is sufficiently tough not to break, and hard enough not to bend, under the trials to which it would be *fairly* exposed; and that being alike free from magnetic properties and liability to oxidation from exposure to the atmosphere, it possesses the requisite qualities for the pivot of the mariner's compass; and he could not but anticipate that, when fitted with a ruby cap to correspond, it would be found greatly to improve the working. Besides the application of this substance for compass pivots, Captain Johnson stated that it might probably be found advantageous for other instruments, and especially for the points of the axes of the dipping needles, fitted on Mr. Fox's plan, for use on board ship.

On a New Rain Guage. By Mr. JAMES JOHNSTON, of Greenock.

Mr. Johnston described a new rain guage, so constructed that the receiving funnel or orifice at which the rain enters, is always kept at right angles with the falling rain. By the action of the wind on a large vane, the whole guage is turned round on a pivot, until the front of the guage faces the quarter from whence the wind blows; and by the action of the wind on another vane attached to the receiving funnel, the mouth of the funnel is moved from a horizontal towards a perpendicular position according to the strength of the wind. The receiving funnel and vane attached to it are balanced with counterpoise weights, in such a manner that the wind, in moving them, has as much weight to remove from a perpendicular position, in proportion to their bulk, as it has when moving an ordinary-sized drop of rain from the same position; by this means the mouth of the guage is kept at right angles to the falling rain.

On an Improved Rain Guage. By Mr. Thom.

It consists of a cylinder two feet long, and seven inches diameter, sunk in the earth until the mouth of its funnel (which (receives the rain) is on a level with the ground surrounding it. Into this cylinder is put a float, with a scale or graduated rod attached to it, which will move up or down as the water rises or falls in the cylinder. There is a thin brass bar fixed within the funnel, about half an inch under its mouth, with an aperture in the middle just large enough to allow the scale to move easily through it. The upper side of this cross bar is brought to a fine edge, so as to cut but not obstruct the drop which may alight on it. There is an aperture also in the bottom of the funnel, through which the water must pass into the cylinder, and through which also the scale must move ; but this aperture requires to be made no larger than just to permit the scale to move through it freely. When the guage is firmly fixed, and the float and funnel in their places, water is to be poured in till the zero of the scale is level with the upper edge of the aperture.

On the Combustion of Coal and the Prevention of the Generation of Smoke in Furnaces. By Mr. Williams.

Mr. Williams observed, that in treating on steam and the steam-engine, the subject divides itself into the following heads :—1st, The management of *fuel* in the generation of *heat ;* 2nd, the management of *heat* in the generation of *steam ;* 3d, the management of *steam* in the generation of *fuel.* The first belongs to the *furnace ;* the second to the *boiler ;* and the third to the *engine.* The first, although exclusively in the department of chemistry, is to be considered in the Mechanical Section, for the purpose of showing its connection with the practical combustion of fuel in the furnace. The main constituents of coal are carbon and bitumen : the former is convertible in the *solid* state, to the purpose of generating heat ; the latter in the *gaseous* state also, and to this latter is referable all that assumes the character of *flame.* The greater part of the practicable economy in the use of coal being connected with the combustion of the gases, this division of the subject is peculiarly important. We all know that combustible bodies cannot burn without air : the actual part, however, which air has to act is little inquired into beyond the laboratory : yet on this part depends the whole of effective combustion. Mr. Williams went on to show, that all depended on bringing the combustible and

the air into contact in their proper quantities, of the proper
quality and at the proper time—the proper place, and the
proper temperature. The conditions requiring attention were,
1st, the quantity ; 2nd, the quality of the air admitted ; 3rd,
The effecting their incorporation or diffusion ; 4th, The time
requiring for the diffusion ; and 5th, The place in the furnace
where this should take place. Mr. Williams exhibited several
diagrams, representing the several processes connected with
the combustion of a single atom of coal-gas or carburetted
hydrogen, and also of bodies or masses of such gas. The
essential difference between the ordinary combustion of this gas
in combination with atmospheric air, and that resorted to by Mr.
Gurney in combination with pure oxygen, in what is called the
Bude light, was then explained. By these diagrams it was shown,
1st, What was the precise quantity of air which the combustion
required; and 3rd, That the unavoidable want of time in the
furnace to effect this degree of diffusion was the main impediment
to perfect combustion, and the cause of the generation of smoke.
From the consideration of these details, the inference followed,
that smoke once generated in the furnace cannot be burned,—
that, in fact, smoke thus once generated became a new fuel, de-
manding all the conditions of other fuels. Mr. Williams dwelt
much on the chemical error of supposing that smoke or gas can
be consumed by bringing it into contact or connexion with a
mass of incandescent fuel on the bars of a furnace ; that, in fact,
this imaginary point of incandescence, or the contact with any
combustible body at the temperature of incandescence, was pe-
culiarly to be avoided, instead of being, as hitherto, sought for ;
and hence the failure of all those efforts to prevent or consume
smoke. The great evil, then, of the present furnaces was their
construction, which did not admit the necessary extent of time
(or its equivalent), time being essential to effect the perfect dif-
fusion or mixture of the gas, of which every chemist knew the
importance, and on which the experiments of Professor Graham
were so conclusive. Mr. Williams then proceeded to show,
that unless some compensating power or means be obtained,
and practically and economically applied, we can never arrive
at full combustion, or prevent the formation of smoke. This
compensating power was shown to be obtainable by means of
surface, and was well exemplified in the blow-pipe : the remedy
then, for the want of time in the furnace, may be met, by in-
troducing the air in the most effective situation, by means of
numerous small jets. Mr. Williams considered the primary law
to be this ; viz. that no larger portions of air, that is, no greater
number of atoms of air, should be introduced into any one lo-
cality, that can be absorbed and chemically combined with the
atoms of the gas with which they respectively come into con-
tact. Again, that the effecting, by means of this extended sur-

face, this necessary diffusion was the main condition which required attention, and not that of temperature. Mr. Williams then exhibited the diagram of a boiler to be constructed on the above principles, and stated that he had an experimental boiler at work, which fully proved the accuracy of the principle. •

Sir John Robinson stated, that the Committee of Recommendations had suggested the appointment of a Committee to make a further investigation of Mr. Williams's plan, and report the result of the enquiry to the Association at their next meeting.—Mr. Vignoles observed, that the gradual increase of the aperture for the blast of cupolas for second meltings of metal, the areas of which were now at least fifty times larger than formerly, proved the necessity of admitting large quantities of oxygen in combustion, which could only be obtained in its combination with the nitrogen, the other component part of atmospheric air.

MISCELLANEOUS ARTICLES.

Portable Match-bougies.

M. Chaussard, one of the most industrious gilders of the Capital, (Paris) has just patented a charming little affair which must soon come into general use, for it will be as convenient an article to the workman, the artist and the gentleman, as to people of fashion. It consists of a small case, not more than half an inch in diameter and three inches long, and of course as easily carried in the smallest pocket and held in the fingers as the most delicate snuff box. Within is a quantity of phosphoric matches and of little bougies. You take of the cover, hold it between the fore and middle fingers, take out a bougie, place it in a little opening in the cover which supports it, then draw out a match, rub it lightly on the case, the end of which, carved into concentric circles, produces a friction that kindles it. The bougie is lighted and the case being closed, you hold in the hand a veritable little wax candlestick, with a light which will burn for five minutes, and of course long enough to go up stairs and to light up a room, to read a letter, which any one may hand you of an evening in the street, to hunt for an object dropped in a dark place, to render assistance in an infinity of cases in domestic life when a light is suddenly wanted,—in a carriage, in a stage, to kindle a fire, light a candle, &c. &c. If both hands are wanted for any of these purposes, the match case may be placed

on the floor, on a table, or in the chimney, and it stands quite
secure.   The price of these little untensils is very moderate,
and within the reach of all.

Franklin Journal   Rec. 8oo Polytéch Jan, 1840,

Interesting Facts in Acoustics.

M. Jobard, a skilful artisan, states the following fact :

" An iron rule of considerable size, was left by chance resting
on a bladder partly filled with gas in my laboratory.   I hap-
pened to hit it in passing; with a hard body, and was surprised
at the long continuance in the sound which escaped from it.   I
repeated the experiment, and ascertained that a metallic rule,
supported on one or two moist bladders, filled with air or gas,
had its vibrations not more rapidly weakened or checked than
if it had been freely suspended in space.   We may even derive
from it various kinds of sounds, by varying the intensity and
place of the strokes.   Other occupations have prevented me
from pursuing the experiments.   I suppose that a metallic or-
gan key, (clanier) supported by two thin tubes of caoutchouc,
filled with air, would give the same results as a piano a cordes,
and that it would better preserve its accordance."   Ibid

Absolute Alcohol.

M. E. Soubeiran, at the conclusion of an article on the recti-
fication of alcohol, gives the following direction :

" If you wish to obtain absolute alcohol easily, abundantly,
and economically, it must first be rectified over carbonate of
potash, which will bring it to 94° or 95°, thus :

1. Bring it to 97° by distilling it with 100 grammes (= 155
grs. troy) per quart of fused chloride of calcium, or, letting it
digest on 2325 grains per quart of quick lime, in a warm place
for two or three days, and then distil slowly from 3875 grains
per quart of quick lime. •

2. Add to the alcohol at 94°, 7750 grains troy, per quart of
quick lime ; leave them in contact two or three days in a warm
stove, and then distil slowly.   The lime communicates to the
alcohol no unpleasant taste or odour, as some have imagined
and written; that happens only when the alcohol has not been
previously rectified.

After this has been done over the alkaline carbonate, such an
effect need not be apprehended, and the alcohol obtained has all
the qualities that can be desired."   Jour de Pharm Jan, 1838

On the Influence of Native Magnesia, (Giobertite) in the Germination, Vegetation, and Fructification of Plants. By ANGELO ABBENE.

It has been thought that the presence of magnesia may be numbered among the various causes which render land sterile, because it has been remarked that magnesia soils have an arid character. This opinion has begun to lose credit since Bergman, on examining the composition of fertile soils, considered magnesia as one of their principal constituents.

Professor Giobert has made many trials to discover the parts which the native magnesia acts that is found in several arable lands. In the vicinity of Castellamonte and Baldissero, this substance is abundantly diffused, in soils which are cultivated with great success, and on which a vigorous vegetation prevails. There are many localities in Piedmont and other places, where the double carbonate of lime and magnesia abounds in cultivated territories, which produce beautiful crops. Giobert has inferred from these experiments, 1st, that native carbonate of magnesia is not adverse to the fruition of plants; 2nd, that in consequence of the solubility of the magnesia in an excess of carbonic acid, the earth may exert an action analogous to lime; 3rd, that a magnesian soil may become fertile when used with the needful quantity of manure employed.

The consequence which naturally flows from these facts is, that the magnesia has been dissolved in an excess of carbonic acid and water, and enters, like lime, into the composision of the sap, and ought to be found in the plant like potash, lime, oxide of iron, &c. This M. Abbene has assured himself of by the analysis of the ashes of plants which vegetated in magnesian mixtures. He has also tried by comparative experiments the question, whether the influence of magnesia in vegetation is analogous to that of lime. The conclusions which he thinks are deducible from these trials are:

1st. Native magnesia is not unfavourable to the germination vegetation, and fructification of plants, but appears favourable to these functions.

2nd. Magnesia, being soluble in an excess of carbonic acid, exerts an action similiar to lime, and when a soil contains magnesia not sufficiently carbonated, a remedy is found in the addition of manure, which, by its decomposition, furnishes the needful carbonic acid. The amelioration will be the more efficacious if the land be well stirred up, because the air will then better perform its office.

3rd. When in arable soils both lime and magnesia exist, the first is absorbed in preference by plants, because it has a greater affinity for carbonic acid.

4th. In sterile magnesian soils, it is not to the magnesia that the sterility is to be attributed, but either to the cohesion of their parts, to the want of manure, clay, or other ingredients, to the great quantity of oxide of iron, &c.

5th. Sterile magnesian soils may be fertilised by means of calcareous substances, such as plaster, chalk, ashes, marl, &c., provided the other conditions are attended to.   Ibid.

Dilatation of Oils. By Prof. F. Preisser, *of Rouen.*

· On the 27th of March, 1835, M. M. Levavasseur Frères, of Rouen, foreseeing a rise in the price of oil, purchased a quantity, partly of seed and partly of fish oil, amounting to 4232 hectolitres, which were stored in the magazines. On the 14th of July the Octroi took a fresh account of the oil, and found that instead of 4232 it amounted to 4279, hectolitres 40 litres.— Granting 30 hectolitres 40 litres for inevitable loss and waste, there remained 17 hectolitres (about 450 gallons) which could not be accounted for, and these merchants were accused by the Octroi of a fraudulent introduction of this excess.

Prof. Preisser being consulted on the case, he undertook an investigation of the amount of dilatation which oil undergoes by a given rise of temperature. He found, by several methods of trial, that in rising from the freezing to the boiling temperature of water, olive and linseed oil expands one part in 1200, and whale oil one part in 1000. Neatsfoot oil expands one part in 980, oil of colza one in 1120, nut oil one in 1100, and white oil one in 1250.

Thus it appeared, that in taking into account the mean difference in atmospheric temperature between the time of storage and subsequent measurement of the oil, the quantity in excess was at once accounted for by the natural expansion of the mass.

The calculation for finding the increase of volume of any number of hectolitres (and of course other measures) for a given difference of temperature, is extremely easy : it is only to divide the number of measures by the co-efficient of increase, stated as above, for one degree of temperature, and to multiply the quotient by the number of degrees constituting the difference of temperature.

The above facts show the imprudence of completely filling barrels with oil in winter, and leaving them unmoved through the summer. The oil must necessarily find its way through the joints of the casks, or otherwise burst the containing vessel.

The same principle is applicable to other liquids. The co-efficient for alcohol is 1-900.

By attending to these facts, errors of opinion, and perhaps expensive law-suits may be avoided.   Idem, Fev., 1839.

Development of Odours.

Every one is acquainted with the rotation which a piece of camphor undergoes in water, and the explanation of the fact which usually ascribes it to the disengagement of the odorant vapours which exhale from it. It is known also that the leaves of the *schinus molle* placed on water, forcibly retract when the surface of the water is covered by a layer of odoriferous oil. M. Morren has just observed a similar phenomenon produced by the volatile oil secreted by the down of the *passiflora fœtida*. When some of the down or hair is placed under water, a small drop of green oil detaches from it, and swims on the water. This drop expands; contracts, expands, contracts again, then seems to burst with force, but the fragments unite to expand again a moment after, and thus the action goes on for about ten minutes, after which the oil is by degrees concentrated, and becomes motionless. These facts may serve, perhaps, to point out a physical theory of odours.   Idem, Avril.

Easy preparation of Anhydrous Phosphoric Acid. By RICHARD FELIX MARCHAND. *(Journ. für Praktische Chemie.)*

In a large porcelain dish place a small support, surmounted by the cover of a crucible, or a little porcelain capsule. Put into this capsule a few pieces of dry phosphorus, and place over it a large bell glass, with an opening at top, stopped by a cork, through which passes two tubes, the one large, and extending down almost to the capsule. It may be closed at top by a cork; the other narrow, and bent at an angle on the outside.

This narrow tube is to be connected with an apparatus for preparing oxygen gas—a retort in which chlorate of potash is heated, is perhaps preferable—though the most desirable mode is to cause it to issue from a gasometer, and to dry it completely by passing it over chloride of calcium and sulphuric acid. The oxygen gas must first be passed in, so as to expel the atmospheric air ; then inflame the phosphorus, by passing a hot iron rod down the large tube. When all the phosphorus is burnt, more may be passed down the tube into the little capsule. The retort may be easily changed when all the chlorate of potash is decomposed. When the bell glass becomes too hot, the operation must be stopped till it cools, otherwise it will inevitably break. In this way, a very considerable quantity of the acid, almost pure, may be made in a very short time. With a quarter of a pound of phosphorus I have made more than half a pound of anhydrous acid. When the combustion is well managed, scarcely any vapours are disengaged. The flakes of acid attached to the bell glass and capsule may be quickly removed by a spoon. It must be preserved in well closed bottles.
                                                        Idem, Juin, 1839.

Rock Crystal Spun. By M. Gaudin.

M. Gaudin sent to the Academy of Sciences, at the last (April) session, specimens of rock crystal, which he had succeeded in melting and drawing out into threads several feet in length, with the greatest ease. One of these can be wound into a skain, and the other wound round the finger.

M. Gaudin has found also, that melted rock crystal moulds easily by pressure, and that it is very volatile at a temperature a little above its melting point. Alumin acts very differently from silica; it is always perfectly fluid, or crystalized, and cannot be brought to a state of viscosity; while viscosity, separate from all tendency to crystalization, is the permanent condition of silica under the oxygen blowpipe. Alumin is much less volatile than silica; it often, however, undergoes ebullition.

In a more recent essay, M. Gaudin has tried the temper and relations of rock crystal, which has afforded unexpected results. If a drop of melted crystal fall into water, far from cracking and flying to pieces, it remains limpid, and furnishes good lenses for the microscope. When struck by a hammer, the instrument rebounds, and the lump will sink into a brick rather than break: its tenacity is such, that pieces can be detached only as splinters. It resembles steel in elasticity and tenacity.

Silicious compounds act nearly in the same way as rock crystal. The sandstone of the pavements spin off like it, with this difference, that its threads, instead of being limpid, are of a pure white, nacreous, silky, and chatoyant, in a singular degree, so that they might be mistaken for silk; and the globules, to a certain degree, have the aspect of fine pearls. There is no doubt that in this way successful means will be employed in producing imitations which will be preferred to natural pearls, since they will possess the hardness of annealed rock crystal, instead of that of a calcareous compound.

The emerald threads perfectly well, and its threads, which scratch rock crystal, are also more tenacious than crystal threads.   Idem

Separation of Lime from Magnesia. By J. W. Dobereiner. (Journ. für Praktische Chemie.)

If anhydrous chloride of magnesium be treated in contact with air, oxygen is absorbed, and the chlorine abandoned. This decomposition, that is, the transformation of chloride of

magnesium into magnesia, is now prompt and complete when chlorate of potash is used instead of air.

This property renders the separation of lime from magnesia very easy. Dissolve the compound of these two bodies (e. x. *dolomite*, &c.) in hydrochloric acid ; evaporate to dryness ; heat the dried mass in a platina capsule till the acid vapours clear, and add to it, urging the heat to commencing redness, small portions of chlorate of potash, until there is no further disengagement of chlorine. The remaining mass is then composed of chloride of calcium, magnesia, and chloride of potassium, the separation of which is easily effected by treating the mixture with water, filtering the solution, precipitating the filtered liquor by carbonate of soda, &c.

<div align="right">Idem. Juillet, 1839</div>

Fabrication of Flint and Crown Glass.

On the 27th of January last, M. Bontems, director of the glass works at Choisy-le-roi, read to the Academy of Sciences a memoir, in which he described the process by which he succeeds in making flint glass and crown glass, exempt from streaks and bubbles, and perfectly white, (clear ?)

M. Guinand had before succeeded in making flint glass without striæ, by working (brassant) the melted glass into a perfectly homogeneous mass. He accomplished this by means of cylinders of refractory earth, like that of crucibles. These cylinders, closed at bottom, were open at top, so as to receive forked iron rods, with which the mass of melted glass could be stirred as long as necessary, fresh rods being used as they grew hot. M. Guinand had thus resolved a part of the important problem of the fabrication of optical glass, but he left some of its elements in uncertainty. Guided by the experience of this skillful manufacturer. M. Bontems discovered that in making flint and crown glass, the absence of bubbles depends on the proportion of the elements of the glass, and the arrangement of the fire toward the end of the operation. Thus far, also, fluid glass had attained a density of only 3.2 without injuring its clearness, (blancheur) while he has been able to give it a density of 3.6, and as clear as the most beautiful crystal ; and crown glass as clear as that of Saint-Gobain or Saint-Guirin. He is preparing also to furnish opticians with disks of flint glass and of crown glass, of 40, 50, and even 60 centimetres (=2 feet nearly) in diameter. He appends to his memoir a plan of his ovens and crucibles, and points out all the details of his process.

<div align="right">Idem, Mars, 1840.</div>

On the Precipitation of Gold. By A. Morin, of Geneva.

Whenever gold is dissolved for any purpose in the arts, a notable portion of it remains in the mother waters, and various means have been recommended for extracting it. The principal substances used for this purpose are sulphate of iron, and formic acid, or the formiates of potash and soda.

Though the sulphate of iron is a low priced article, compared with the formiates, the value of the metal is such, that the materials of higher price would be unhesitatingly employed, if the would extract the gold more completely. It may be interesting, therefore, to the workers in the metal, to know the comparative value of the two processes most generally recommended for its precipitation. I have attempted to resolve this question by treating the mother waters resulting from some preparations of this metal.

They were divided into two equal parts, each weighing a kilogramme, (2¼ lbs. nearly) and as each contained a little more than two grammes(30.88 grains troy)theyrepresented a solution of 1-450 of the metal.

Into one I poured concentrated formic acid, until it acquired a decided acidity. The colour became a fine deep yellow. No gold was precipitated, even when heated. The formiate of potash tried with a small portion of this liquid diluted, showed no reaction. It was only when the liquid was half evaporated that metalic spangles appeared on the surface.

The addition of a few drops of caustic potash increased the quantity, and it was added as long as it increased the precipitate, which had the appearance of dark flocculi mixed with metallic spangles. It was soon deposited. The liquid was neutral and of a green colour. The farther addition of caustic potash gave no precipitate, and formic acid only changed the colour to a deep yellow. A fresh concentration produced no separation of the metal. The precipitate, washed and dried, was black. Heated to redness, it soon assumed the golden lustre. Its weight was 1.535 grammes. The washings and the mother waters were then mixed with a solution of sulphate of iron. An abundant black precipitate was formed, which I treated with muriatic acid with heat. It became of a clear brown, and very light. Adding more sulphate of iron, metallic spangles were formed, and it was continued till nothing more appeared. The deposit, washed with warm water, and then with acid, was dried and heated. It had the metallic splendour, and weighed 0.717 grammes, about one-half the preceding. United to the former, the weight was 2.252 gr.

This essay might appear sufficient to prove the superiority of sulphate of iron over the formic compounds; but I nevertheless

tried the direct action of the sulphate on the other portion of
liquid, first acidulating it with muriatic acid, and heating it.
The sulphate of iron was added as long as any precipitate ap-
peared. At first violet, it passed to a clear brown. The preci-
pitate was separated from the supernatant liquid, washed with
water and muriatic acid, dried and reddened. It weighed
2.880 grs., sensibly equal to the two other precipitates.

The mother waters and the washings created were treated
with formiate of potash, which occasioned no precipitate, even
on the concentrated liquid.

These trials prove—

1st· That formic acid precipitates gold only when evaporated,
so that the solution contains at least 1-225 of gold.

2d. That the formiate of potash does this better than formic
acid alone.

3d, That formiate of potash separates from a concentrated
solution only about ⅔ of its gold.

4th. That sulphate of iron precipitates completely from
liquids containing only 1-450 of gold.

Sulphate of iron, therefore, is complete, more easy, and more
economical. Two precautions are nevertheless necessary for
complete success—the use of heat and a notable addition of
muriatic acid. Heat gives cohesion to the precipitate, which
facilitates its separation. The acid accelerates the action of the
sulphate of iron. Idem, Fev., 1840.

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*Phenomena observed with respect to Carbonic Acid, subjected to
pressures superior to that of the Atmosphere.* By M. COUERBE.

Water, at common temperature and pressure, dissolves about
its volume of carbonic acid ; and if the pressure is increased,
the absorption is also a volume of gas for each atmosphere, so
that by means of a manometer, we learn the state of the interior
of the vessel. The law, however, does not hold good for all
pressures, and even at 5 volumes, the indicated pressure is often
7, the temperature being 15°, so that, in fact, a term must be
arrived at in which the liquid must lose its solvent power, and
the gas become nearly ready to assume the liquid state.

It follows, that gas, compressed over a given liquid, under-
goes variable pressures, which are not always correspondent
with the number of volumes dissolved. The nature of the
liquid also causes a variation in the results.

The trials which I have made to come at a knowledge of the
phenomena, were practised upon champagne bottles, in good
condition, and which support about 20 atmospheres, a sufficient

guarantee against fracture;—and yet, when wine ferments in them, we are struck with the damage which takes place in the course of a month, amounting, in the experience of some champagne merchants, to 15, 20, 30, 40, often 50, and even 60 per cent. Place a manometer however, in connexion with the resisting bottles, and it scarcely ever indicates more than 7 atmospheres. The fracture, therefore, must be due to some other cause than pressure, or that the tension of the gas, for reason I am about to furnish, suddenly increases, and transcends 20 atmospheres, the cohesive force of the glass.

Observation has proved to me, that in this liquid, the internal tension is very strong when it contains a little more than five volumes of carbonic acid; that even at three to four vols. it is great, and that between four and five vols., the bottles never break. The manometer indicates seven atmospheres.

The cause of this appears to me to be attributable to the dissolving power of the liquid to the gas, which is variable at each pressure. The tension will be the more feeble as the affinity of the water for the gas is greater. Hence in a mixture of liquid and compressed gas, there are two forces in operation, the force of solution and the force of tension. When three or four volumes are dissolved, the soluble force is weak, and cannot overcome the tension of the gas; at four to five volumes, the pressure is sufficient to bring the affinity of the liquid for the gas to its maximum, and to give the latter a tension equal to 7. At five volumes and more, the solvent power diminishes; the tension increases, surpasses the cohesion of the glass, which is equal to 20 atmospheres, and breaks it. These singular results seem foreign to all that has hitherto been known relative to the solution of gases in liquids.

It is proper, however, to notice what M. Soubeiran has said in his work on gaseous acidulated waters: "One fact worthy of remark, is, that notwithstanding the bad quality of the products, the gas contained in the bottles is sufficient to expel the corks to the end of the experiments, and yet, when we come to examine the liquid, we find but a small quantity of carbonic acid in it." This fact appeared to M. Soubeiran an anomaly, and he endeavours to explain it by saying: "The operator, by his dexterity, was enabled to enclose a portion of gas in the neck of the bottle, which accumulates there with sufficient intensity to drive out the cork, but there was no coincidence between the volume of gas retained in the water and that of its superior atmosphere."

The various phenomena above presented, may be assimilated to examples of another kind—the solution of salts in water. It is known that sulphate of soda is more soluble at 40° than at 20°, at 60°, &c., so that a line may be traced in a diagram through points which indicate the quantity of salt dissolved, and

the temperature—a line which chemists call the *curve of solubility*. In the same way in the phenomena of gases, I think that a careful series of experiments might establish a curve of solubility of gases in liquids at even degrees of pressure, and that these apparently contradictory facts might be reduced to general laws. Thus the pressure over a solution of carbonic acid gas in certain liquids, acts absolutely like heat in a solution of salts in water—a correspondence which appears to me to be demonstrated by the experiments above detailed.

In the work referred to, M. Soubeiran gives a table of experiments which shows that agitation increases the tension of the gas; the difference is particularly marked at the beginning of the operation. "The agitation of the liquid," says the author, "constantly increases the pressure of the gas at the surface, and causes the water to lose a portion of the gas it held in solution." I may mention, that I made more than fifty experiments with bottles of champagne at 5 volumes, and that the manometer, which indicated 7 atmospheres, did not vary a demi-millimetre —therefore, if the observation of M. Soubeiren can be relied upon, the empty space modifies the phenomena according to its variable extent or dimensions I say the empty space, because the experiments of Soubeiran were made with casks of 30 gallons of water, charged with 4 volumes of carbonic acid gas, and having a void space of 2½ gallons at the surface.

In general, when a liquid like water contains several volumes of carbonic acid in consequence of increased pressure, the gas escapes almost instantly, as soon as the pressure is relaxed, and the liquid retains about a single volume; but champagne wine acts differently. As soon as the cork is withdrawn, about half a volume of gas escapes immediately, and the disengagement continues slowly till it amounts to a volume, and then stops; when a bottle may be left long uncorked without a total loss of gas. I am now supposing that the wine has been well prepared, and somewhat dried by tannin. This singular fact is owing to organic matter extending, in a kind of network, through the vinous mass, and which condenses and retains the gas precisely like certain powders, and a great number of porous bodies, even under the common pressure of the atmosphere.—*Actes de l'Academie Royale des Sciences de Bourdeaux,* 1839. Idem

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EDITORIAL NOTICES.

Electricity of Steam.

We have been very anxious to place before the readers of the annals, the communications of Messrs. Armstrong and Pattison,

regarding the extraordinary electrical phenomena which they have
observed whilst experimentary on the steam issuing from high
pressure boilers : and shall be glad to receive communications
from any quarter where similar experimenting have been made, with
the precise mode of carrying on those experiments, and their re-
sults.　　And as it is possible that many who try will fail in obtain-
ing sparks of electricity, we hope that no experimenter will, on
that account, consider himself incompetent for the undertaking ;
because, although perhaps, a melancholy sympathy, we can assure
him that he will not stand alone in that respect.　We happen to
know of some failures in Manchester ; and amongst the rest, our
own experiments have, hitherto, been unsuccessful ; although every
precaution has been taken to detect even the minutest development
of electric action.　We have other experiments in view which will
soon be made ; and we have access to several high pressure boilers,
where our experiments are intended to be carried on : and in the
next number of the Annals, our readers may expect a full account
of all the particulars attending them.

Several Manchester gentlemen, are about to make similar ex-
periments at different boilers, both for stationary engines and for
locomotives ; and the results will be published in an early number
of the Annals.

Electro-Magnets.—Mr. Radford's electro-magnetic disc, first
brought forward at the conversazione at the Royal Victoria Gallery
(see page　) has since that time been tried with a battery of eight
iron jars in two series of four each, and with a bundle of copper
wires, instead of the copper rod, for the conductor.　It now carries
about 22½ cwt.　Mr. Radford is making a larger battery, by the
action of which the magnetic powers of the iron disc will very
shortly be tested ; and our readers may expect to hear of the results
in the next number of the Annals.

Voltaic Batteries.—The superior powers displayed by Mr.
Sturgeon's cast iron battery even with the comparatively small ar-
rangements hitherto made, have induced a few lovers of science,
members of the Royal Victoria Gallery, to enter into a subscription
for the purpose of defraying the expense of an extensive battery
on the same principle.　Five hundred pairs of metals, exposing
four square feet of surface each, are contemplated on for this mag-

nificent battery. The iron is to be formed into rectangular boxes, open at top. The vertical sides of each box to be about thirteen inches square inside, and the ends and bottom about $1\frac{1}{2}$ inches broad, leaving that space between the sides for the reception of a square foot of stout rolled zinc. The battery is already in hands and will be proceeded with without delay.

Electro-type.—A very valuable application of the electro-type process has, for some time past, been made by Mr. Davies, Philosophical Instrument Maker, Bold-street, Liverpool. It is the growing and multiplying of the finely divided metallic scales of mathematical and philosophical instruments, many of which are very expensive by the usual process of engraving. We are not aware of a more important application of the electro-type process, and we heartily wish Mr. Davies every success in turning it to account in his own department. By making this important application public, without attempting to monopolise it to the exclusion of improvement in the hands of others Mr. Davies has secured to himself not only the credit of the discovery, but the best wishes of society. ' Mr. Davies's discovery ought to have appeared in the Annals for last month, but by some means or other his letter on that subject was mislaid.

Deguerretype.—We wish to warn the purchasers of these beautiful pictures, to keep them as much as possible from strong light, and from air, as they will otherwise spoil, and become defaced in a short time. We have already seen instances of this kind.

We have, in this number, given Dr. Priestley's own account of his experiments on the *Lateral Discharge;* which our readers will find more interesting than the *partial* account of them hitherto placed before our readers.

We shall give another paper of Dr. Priestley's on the same subject, in our next number.	•

We have also given Capt. J. L. Winn's account of an interesting electrical phenomenon, in which the lightning conductor of his ship discharged an abundance of sparks, effects of *electrical waves.*

We particularly recommend the perusal of the above two papers to every inventor of lightning conductors; and we can promise them still more valuable data, (recently obtained,) in our next number.

" A constant reader" of the Annals, may expect to see the Hon.
M'Cavendish's experiments and theory, as soon as we can find
room.

Aurora Borealis.—A very brilliant Aurora was seen in Man-
chester, on the evenings, of Sunday the 20th, and Monday the
21st of December, 1840. We happened to see this phenomenon
on the latter evening, about ten o'clock. It was then very bright,
with an immense quantity of diffuse white steamers. This, we un-
derstand, was its general appearance on the preceding evening.
The wind was very light, and easterly, with a gentle frost. The
Aurora was seen here several times in January last ; on one occa-
sion the upward waves of light were very grand.

Prize Volumes of the Annals of Electricity, &c.

In order to stimulate and promote experimental inquiry, in the
various departments of Electricity and Magnetism, the Editor
proposes to offer prize volumes of the Annals, to those experimen-
ters who may be most successful in the following subjects :—

1st. For a description of the most powerful, soft iron, or Electro-
magnet, in proportion of the weight of the iron employed in its
structure ; which is not to be less than 10lb. The voltaic battery
employed will be at the option of the experimenter ; and is to be
described by him, with the manner of using it in the experiments
with the soft iron magnet.

2nd. For the invention of an electrical-machine, more powerful,
in proportion to size, than the usual plate or cylindrical form. A
full description of the apparatus, with a suitable drawing, will be
required.

3rd. For an account of the most extensive and best conducted
experiments on the electricity of the steam of boilers of high or
low pressure engines, with all the particulars respecting the cha-
racter of the water employed in each boiler ; and such other parti-
culars as may appear interesting.

4 For the best mode of printing Electro-type Plates, different
from those published.

5 For the best paper on any branch of experimental research
in Electricity, or in Magnetism.

The prize for each of the above subjects will be Volume VI, of
the " Annals of Electricity, Magnetism, and Chemistry, &c;"
bound, and gold lettered in the first-rate style, with a suitable
emblem and motto. To be presented to the successful candidates,
or to their agents, (in London, if required,) on the first day of
August, 1841.

The communications on the above subjects are to be addressed
to Mr. William Sturgeon, Royal Victoria Gallery of Practical
Science, Manchester, on or before the first day of May, 1841.

LECTURE III.

Having already made you acquainted with the modes of deflecting the pith balls, by one, and the same, excited body, as sealing-wax, glass, amber, &c., it will now be necessary to inform you of the character of those phenomena which are displayed by the employment of two or more distinct excited bodies : whose electric forces are brought into play upon the electroscope fig. 5, plate vii., vol. 5, at one and the same time; and in order to familiarize the experimental operations we will employ two of those bodies with which we have already experimented, viz., a stick of sealing-wax and a tube of glass.

Excite the sealing-wax by some of the processes already described, and communicate its electric forces to the brass arm of the electroscope fig. 5, whose pith balls will remain divergent after the excited wax is withdrawn. Now excite your glass tube by rubbing it in the hand covered with warm black silk ; and present this excited tube to the upper side of the metallic arm of the instrument, and parallel to it, and you find that the balls collapse by the approach of the glass tube, but separate from one another again as you withdraw the tube from them. These motions of the pith balls are the very opposite to those which are displayed by the operations of the sealing wax alone; for in that case, the pith balls diverged further from one another by the approach of the sealing wax, but by the excited glass they collapse.

Let us now reverse this experiment, by first exciting the glass tube and communicating its electric forces to the arm and balls of the electroscope, by drawing the tube over the metallic arm of the instrument, and afterwards approaching the upper

side of the arm with an excited stick of sealing wax. You will
now observe that the phenomena displayed by this mode of
experimenting, is of precisely the same character as those
observed by the former, or converse method; for the pith balls
will as decidedly collapse by the approach of the excited wax,
as they did before by the approach of the excited glass tube.
These are exceedingly beautiful facts, and cannot be too soon
implanted on the mind; for although they appear simple in
themselves, they are the foundation stones upon which much
reasoning in electricity is based: and upon which alone many
of the grandest operations of nature find an easy solution.

In those of the preceding experiments in which the electro-
scope was not touched, we have held the excited body, whether
sealing wax, glass, amber, &c., directly over and parallel to, the
horizontal arm of the instrument; but there are other modes of
experimenting with these pieces of apparatus, which are pro-
ductive of some variation in the phenomena, with which it will
be necessary to become acquainted before we shall be enabled
to explain several of those already noticed.

Let us now again excite the sealing wax, and again commu-
nicate its electric action to the electroscope by drawing it along
the arm. The pith balls will diverge as usual. Now touch
the metallic arm with your finger, and immediately the pith
balls will fall close to each other, and all electric action entirely
disappears. Now perform the same experiment with the ex-
cited glass tube, and you find the same results to appear. You
may try excited amber, sulphur, or any other body in place of
the glass, or the wax, and in all cases you will observe that by
touching the metallic arm of the electroscope you deprive the
instrument of all its electric action. The same thing would
occur were you to touch the metallic arm with any of those
bodies which have the faculty of conducting the electric fluid
with a considerable degree of facility, such as metallic bodies,
charcoal, &c.

Another method of abstracting the electric action from the
electroscope, is by presenting the finger, or other good con-
ductor, to the divergent pith balls. You will first observe that
the balls will approach the finger, and after being in contact
with it for a few moments they will fall off again, having lost
all their electric force. The electroscope may also have its
electric action neutralized by the fine point of a needle, or a
pin, presented to the brass arm without touching it. In this
case the pith balls will gradually collapse until the electric
action entirely disappears.

Let us now again excite the sealing wax, and afterwards draw
it over the arm of the electroscope as in previous experiments,
leaving the pith balls divergent. Now excite the glass tube and

draw it also over the metallic arm. The result of this experiment is very equivocal, and depends upon the proportional electric forces of the two excited bodies. If the force communicated to the instrument by the wax be exactly of the same extent as that communicated by the glass tube, they will balance each other, and the pith balls will colapse, and remain close together after the glass tube is withdrawn. But, as is more frequently the case, when the two forces do not balance one another, the balls will diverge, and remain divergent, after the glass tube is taken away from the electroscope.

Let us now reverse this experiment by first exciting the glass tube, by communicating a part of its electric action to the electroscope, by drawing it over the metallic arm; and afterwards exciting the sealing wax and drawing it also over the arm of the instrument. In this case the behaviour of the pith balls will be similar to that exhibited by the preceding experiment. They will remain close together after the wax is withdrawn, if the action communicated by the wax be equal to that communicated by the glass, but in all other cases they will remain divergent. Moreover, the remaining divergency may be due either to the action of the electricity of the wax, or to that emanating from the glass tube, accordingly as their respective forces predominate. And the angle of divergency will, in all cases, depend upon the degree of the remaining force.

Now, to understand whether the remaining electric action in electroscope be due to the electricity of the wax, or to that communicated by the glass tube, we have only to excite either the one or the other again, say the glass, and present it to the upper side of the arm of the electroscope. If the balls diverge farther, their previous divergency was due to the action of the glass tube, but if they collapse, that divergency was due to the action of the wax. But if, instead of the excited glass tube, we employ the excited wax, for the purpose of discovering the residuum electric action in the electroscope, then, should the balls diverge farther by its approach, the residuum of electric action is due to the electricity of the wax, and if they collapse, it is due to the glass.

From several of the results of the preceding experiments, we have obvious instances of the electric powers of sealing wax, and those of glass, by the hitherto described modes of excitation, counteracting one another, and when the experiments are made with great care, the electric forces which emanate from the two excited bodies, are found to neutralize each other very exactly. Hence we learn, that those forces are of opposite kinds, and must necessarily originate from different electric conditions, which the exciting process have occasioned in the sealing wax and the glass tube.

In consequence of the electric forces emanating from glass and
sealing wax, by the methods of excitation hitherto described, be-
ing found to neutralize each other, some philosophers have been
of opinion that there are two kinds of electric matter, one belong-
ing to the glass, and the other to the sealing wax; and as the
electric action of all *vitrious* bodies has been found to correspond
with that emanating from glass; and the electric action of amber
and all *resinous* bodies to correspond with that of sealing wax; the
former has obtained the name of *vitrious* electricity, and the latter
that of *resinous* electricity.   These technicalities were brought
forward in a very early period of the science, and answered the pur-
pose of illustrating the principles of a certain hypothesis, now
nearly exploded by subsequent discoveries which show that, at
least, the terms *vitrious* and *resinous* are decidedly incorrect, as
the *character* of the electric force emanating from either class of
bodies, can be varied at pleasure : an instance of which I will now
place before you.

Let us excite a stick of sealing wax by rubbing it against the
sleeve of the coat, or against fur, &c., as hitherto described, and
then draw it over the metallic arm of the electroscope, and leave
the pith balls divergent.   Now change the fur, &c., for a piece of
tin foil, and rub the surface of the wax a few times, very briskly,
with the foil held in the hand.   Present the newly excited sealing
wax towards the upper side of the arm of the instrument, and the
pith balls will collapse in the same manner as by the approach of
an excited glass tube.   And if the sealing wax be drawn over the
metallic arm, the balls will remain together even after it is taken
away from the instrument.   And, indeed, all those phenomena
which the excited glass tube has shewn in the preceding experi-
ments, can also be shown by a stick of sealing wax when excited
by tin foil : which show that there is no peculiarity of action in the
wax, but that its electric character depends upon the nature of
substance against which it is rubbed ; or, if you please, with
which it is excited : and as this is the case with all resinous bodies,
hence the absurdity of the term *resinous electricity.*   The term
vitrious electricity is also incompatible with experimental facts,
because by varying some of the circumstances in the process of
excitation, the character of the electric action proceeding from
glass and other vitrious bodies, will vary accordingly.   This fact
is very easily shown by employing a tube of glass whose surface is
made asperous, either by means of acid, or mechanically, by
means of emery powder, or by a common grinding stone.   When
a glass tube, thus prepared, is excited by a piece of silk in the
manner already described, its electric action corresponds with that
emanating from sealing wax, and the resinous bodies, which have
been excited by woollen cloth, or by fur, &c., and is consequently
opposite to that displayed by a smooth glass tube, similarly excited.

The difference in the electric action of excited smooth glass by
dry silk, and that of sealing wax, &c., by fur, woollen cloth, &c.,

was discovered by M. Du Fay, intendant of the French king's gardens, about the year 1733 ; who, in consequence, introduced the terms *vitrious* and *resinous* electricity. After describing some other of his discoveries, Du Fay proceeds to describe the one in question in the following manner :—

" Chance has thrown in my way another principle more universal and remarkable than the preceding one ; and which casts a new light upon the subject of electricity. The principle is, that there are *two kinds of electricity*, very different from one another ; one of which I call *vitrious*, and the other *resinous* electricity. The first is that of glass, rock-crystal, precious stones, hairs of animals, wool, and many other bodies. The second is that of amber, copal, gum lac, silk, thread, paper, and a vast number of other substances. The characteristics of these two electricities are, that they repel themselves, and attract each other. Thus a body of the vitrious electricity repels all other bodies possessed of the vitrious ; and on the contrary, attracts all those of the resinous electricity. The resinous, also, repels the resinous, and attracts the vitrious. From this principle, one may easily deduce the explanation of a great number of the phenomena ; and it is probable, that this truth will lead us to the discovery of many other things."

As this discovery formed an important epoch in the history of electricity, by furnishing materials for, what was then considered, an essential part, at least, of a complete theory of the science, which met with little or no opposition for about twenty years afterwards, and even to the present day, is adhered to by certain philosophers, the above passage of the author's will always be an interesting document to refer to. But, as I have already shewn by experiments, as the glass or the sealing wax can be made to display either the one or the other kind of electric action, by varying the circumstances of the excitation, Du Fay's hypothesis of vitrious and resinous electricity is perfectly untenable. The discovery of varying the character of electric action of excited bodies, was first shown by Mr. Canton, by some experiments which that philosopher made in December, 1753, about twenty years after those made by Du Fay. Some of Mr. Canton's experiments were those I have already described with tinfoil and sealing wax, and with rough glass and silk. Till this discovery by Mr. Canton, the friction of sealing wax had always been supposed to produce one kind of electricity, and the friction of glass another kind ; which " were thought to be essential, and unchangeable properties of those substances."

Notwithstanding the prevailing idea which philosophers entertained respecting the difference in the electric actions of vitrious and resinous substances, Dr. Watson in this country, and Dr. Franklin in America, had explained electrical phenomena upon very different principles to those set forth in the hypothesis of Du Fay, about some six years previously to the discoveries of

Mr. Canton which I have already mentioned : and although Dr. Franklin has had the credit of the theory which is now generally adopted, it is certain that Dr. Watson has a prior claim to it, at least so far as the dates of their respective views were made public. " Dr. Watson showed a series of experiments to confirm the doctrine of *plus* and *minus* electricity to Martin Folkes, Esq., then president, and to a great number of Fellows of the Royal Society, so early as the beginning of the year 1747, before it was known in England that Dr. Franklin had discovered the same thing in America." See the Philosophical Transactions, vol. xliv. p. 739 ; and vol. xlv. p. 93—101. Dr. Franklin's paper, containing the same discovery, was dated at Philadelphia, June 1st, 1747.*

The principles of the Franklinean theory of electricity are similar to those which I have advanced at the commencement of these lectures ; viz., that all electric phenomena emanate from the operations of a peculiar kind of matter. The following outline of this theory is copied from Priestley's History.

" According to this theory, all the operations of electricity depend upon one fluid *sui generis,* extremely subtile and elastic, dispersed through the pores of all bodies ; by which the particles of it are as strongly attracted, as they are repelled by one another.

" When the equilibrium of this fluid in any body is not disturbed, that is, when there is in any body neither more nor less of it than its natural share, or than that quantity which it is capable of retaining by its own attraction, it does not discover itself to our senses by any effect. The action of the rubber upon an electric,+ disturbs this equilibrium, occasioning a deficiency of the fluid in one place, and a redundancy in another.

" This equilibrium •being forcibly disturbed, the mutual repulsion of the particles of the fluid is necessarily exerted to restore it. If two bodies be both of them overcharged, the electric atmospheres‡ repel each other, and both the bodies recede from one another to places where the fluid is less dense. For, as there is supposed to be a mutual attraction between all bodies and the

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* Priestley's History.—The original papers of our old indefatigable electricians, are extremely interesting to every cultivator of the science, on which account, we shall occasionally present them to our readers, as we find room in the Annals.—EDIT.

+ Those bodies which were *excited* by rubbing, such as glass, amber, sealing wax, &c., were formerly called *electrics ;* and the rubbing substances employed, were called non-electrics ; from the idea that the former class alone could be excited, and that the latter could not be excited, which is contrary to fact, as we shall see as we proceed.

‡ Electric atmospheres are supposed to surround all bodies that are in a state of electric action, and are more or less extensive as the body is more or less electrically active. I shall have occasion to illustrate the doctrine of electric atmospheres at some considerable length, in subsequent lectures.

electric fluid, electrified bodies go along with their atmospheres.
If both the bodies be exhausted of their natural share of this fluid,
they are both attracted by the denser fluid, existing either in the
atmosphere contiguous to them, or in other neighbouring bodies;
which occasions them still to recede from one another, as much as
when they were overcharged.

"Lastly, If one of the bodies have an overplus of the fluid, and
the other a deficiency of it, the equilibrium is restored with great
violence, and all electrical appearances between them are more
striking."

If we admit that the Franklinean theory embraces the true
principles of electric action, we shall be enabled to understand the
cause of many phenomena which, otherwise, would appear to be
exceedingly intricate.

The attraction of light bodies by an excited stick of scaling
wax, or by a glass tube, and the jumping motions produced in
bits of paper, &c., described in the first lecture, may now be
easily explained. When a smooth glass tube is excited by silk,
it is supposed to derive its electric action from a redundancy of
fluid which it has obtained from the silk; hence it is said to be
electrized *plus*, or positively. But when the sealing wax is excited
by fur, woollen cloth, &c., it is considered to have lost a portion of
its natural share of the electric fluid; and is therefore said to be
electrized *minus*, or negatively. Hence you will easily understand
that, in the former instance, the redundant fluid which the glass
tube was charged with after excitation must necessarily have been
obtained from the silk with which it was rubbed: and, in the
second case, some of the fluid naturally belonging to the sealing
wax, must have been carried off by the fur, or the cloth which
formed the rubbing substance. Therefore if these rubbing sub-
stances were to be insulated, they ought to exhibit electric action
of an opposite character to the bodies which they respectively
rubbed, and consequently of an opposite character to each other,
which is absolutely the case. for the electric action communicated
to the electroscope by the excited glass tube, is as decidedly neu-
tralized by the electric action of the rubbing silk, as by that of an
excited stick of sealing wax. And the electric action of the seal-
ing wax is also neutralized by that of the fur, or woollen cloth,
with which it is rubbed. It can also be shown that the electric
action exhibited by the silk, is of the opposite character to that of
the fur, when these substances are rubbed against smooth glass and
against sealing wax, respectively: and that these actions will neu-
tralize each other.

When the excited sealing wax was held over the bits of paper,
in the first experiment, it being negatively electrical, was disposed
to abstract fluid from the nearest bodies that were capable of furnish-
ing it, which in this case were the bits of paper and the table on
which they were placed; but as neither of these were in contact

with the wax, and as the light bodies were easily moved by a moderate attractive force, they were thus lifted to the wax, to which they gave off a part of their natural share of fluid, and became as decidedly negatively electrical as the surface of the wax itself. In this condition they were attracted by the table or the plate on which they were first placed, and where they were soon replenished with fluid; and now being in the same electrical condition as at first, were again attracted by the sealing wax, giving to it another portion of fluid: and by a series of journeys between the table and the wax, the latter became so far supplied with fluid as to diminish the attractive force too far to continue the motions of the paper any longer. The wax, however, was still left in a minus condition, as might be easily shown by the employment of very delicate electroscope such as will be described by and by.

When the excited glass tube is used to produce motions in light bodies, the latter carry the fluid from the tube to the table, until it is deprived of nearly all its redundant fluid: the forces then become too feeble to continue the motions. We must not forget, however, that the electrical fluid is highly elastic, and that like all other elastic fluids it makes its way, or expands to the greatest extent in that direction where the resistance is the least. Hence, in the case of sealing wax, which was rendered negative by excitation, and consequently, its attenuated fluid presenting a less resistance to that in the paper and table, than was presented on any other side, the latter expanded in that direction, and urged or carried the light bodies along with it to the surface of the wax: or at least, assists materially in producing their motions.

*A Report of some Experiments in Atmospheric Electricity, made
in the Autumn of 1840, during which the Disengagement and
Insulation of Ozone were effected. By W. H. WEEKES, Esq.,
Surgeon; Lecturer on Philosophical and Operative Chemistry,
&c. &c.*

The almost incessantly advancing importance of electrical
science, with the now obvious identity of the atmospheric,
frictional, and voltaic currents, and the manifestation of this
universal agent in connection with all physical as well as
chemical changes, are primary considerations which have united
to confer upon the subject an interest, the intensity of which
few sources of philosophical enquiry have even succeeded in
creating. The earlier cultivators of this delightful field of
knowledge, Franklin, Priestley, Cavallo and others, in order to
obtain materials for the basis of a superstructure, most industri-
ously collected and recorded every fact, however seemingly
trivial, in any way bearing upon, or derived from, their favour-
ite pursuit ; nor, though our progress in these branches of
philosophy has recently been distinguished by some gigantic
strides, does it appear less valuable or necessary, if we would
profit by our advancement, to persist in similar records of our
observation and experience. In accordance with these views is
the chief object of the present paper.

It might, perhaps, almost seem that in the grand kite ex-
periment of Franklin, and the great practical results derived
therefrom ; in the philosophical deductions of the Abbe Nollet

and Signior Beccaria, added to the sublime, and—to some minds—terrific phenomena exhibited by the apparatus of M.-de Romas, apart from the valuable labours of more modern experimenters, that the science of atmospheric electricity had been pretty well exhausted of its essential constituents. Such, however, I must submit is very far from being the case. The long continued and splendid operations of the celebrated electrician of Broomfield, Andrew Crosse, Esq., have at once shed a new and expanded lustre on this department of physics; and, I trust, that at a future day he will not withhold from his less experienced brethren in science a full development of his valuable labours in a typographical form. To the fresh ardour and excitement inspired by the extraordinary personal kindness of this gentleman, in exhibiting and explaining to me his magnificent arrangements, I owe the circumstance of having undertaken to extend about 365 yards horizontally over the town in which I reside, an atmospheric electrical machine, which, during many months past, has frequently furnished results of a highly brilliant and interesting character, independent of the minor phenomena which it almost uniformly presents. The erection and carrying out of this apparatus presented many difficulties and required numerous modifications, not incidental, I believe, to its adoption in an open country; though the successful issue has amply compensated for the labour and expenditure employed. This atmospheric exploring wire is insulated at its extremities against the balls from which arise the vane-spindles of the two principal churches of the town, one hundred and thirty-six feet above a base line supposed to be drawn between the two edifices. An intermediate station is supplied by an elevated chimney, near to which is given off a descending wire communicating with various operative arrangements inside the window of my laboratory, and, by means of these, the otherwise most terrific electric current, is managed with ease and certainty, while the experimentalist looks on with a feeling of cool philosophic satisfaction. I propose to subjoin a few notices illustrative of the effects which obtain from the passage of the atmospheric current through this instrument, and I must premise that these remarks are substantially extracts from my daily electrical journal.   •

September 16*th*, 1840.—As the autumnal months have approached, the atmosphere has become highly prolific in electrical phenomena; many of these have assumed a character of great interest and splendour, but I can scarcely hope that my descriptive memoranda will succeed in conveying an adequate idea of the magnificent exhibitions which have rapidly followed each operation during nearly three hours past noon this day. Last evening the barometer sunk suddenly to 28,60, wind W. S.W., and increasing from a previously smart breeze, with other

indications of an approaching storm. During the night some
heavy falls of rain occurred, and this morning at sun rise the
wind blew smartly from the same quarter as yesterday ; the sky
soon became over-spread with various modifications of cloud,
and among them that peculiar form which usually precedes
nimbification was predominant, moving with considerable velocity at a moderate altitude in the direction of the wind. The
instruments throughout the forenoon continued to diverge with
negative electricity, the divergence gradually decreasing as the
day advanced. A few minutes before noon a stupendous, dense,
and heavy looking black cloud, having its edges remarkably
depending and flocculent, advanced immediately over the line
of wire, and commenced discharging its aqueous contents most
furiously. This had no sooner obtained than a continuous torrent of sparks of the *first magnitude,* formed violently, from the
large globular terminus of the atmospheric machine to the inferior receiving ball, attended by corresponding sharp cracking
explosions, each being equal to the report of a moderate size
pistol, and resembling in general effect the well known running
fire occasioned by the rapid discharge of a multiplicity of small
fire arms. With short intervals of a few minutes, as clouds of
a similar character continued to arise over the line of wire,
these splendid phenomena recurred with more or less brilliancy
until about three o'clock p. m. At one period during this
grand display three distinct flashes of lighting were observed in
brief succession, and almost identically with the appearance of
each flash, streams of brilliant fire, having most extraordinary
intensity, rushed through the apparatus with a loud hissing
sound, similar in its effects upon the auditory nerves, to that
which obtains when a considerable mass of red hot iron is suddenly thrown into water. However, in one instance, the cloud
from which the electrical discharge proceeded, could not have
been very near the line of our operative arrangements, as from
five to six seconds were distinctly counted between each flash
and the thunder which followed*.

* These grand displays of the electric matter from the apparatus, were
obviously the effects of *electrical waves,* occasioned by the distant flashes of
lightning. They are phenomena of very frequent occurrence to the kite
experimenter during lightning. We have frequently met with similar appearances whilst experimenting with electrical kites; and it is somewhat
singular that Mr. Snow Harris, who says that he is a kite experimenter,
never saw any of these splendid phenomena; and consequently has no idea
whatever either of their grandeur, or of the cause of their production.

We are in hopes that Mr. Harris will read Mr. Weeke's very beautifully
descriptive account of these phenomena, with a great deal of interest. For
our own part, we hail them as additional, and, indeed conclusive data in
support of the inferences we arrived at concerning the phenomena observed
on board the Dryad and Beagle, when these ships were supposed to be
struck by lightning. (See Annals,Vol. IV, p.171.) The hissing noise in Mr.
Weeke's experiment was precisely that heard by the people in the ships :
both being like that produced by the quenching of red-hot iron.—*Edit.*

During the progress of this grand electrical drama I made numerous experiments,(that is to say, some twenty-five or thirty times,) by means of excellent test instruments, on the quality of the fluid thus abundantly yielded, and found it frequently alternating from the positive to the negative state, as though a series of highly charged zones were gradually being passed over the apparatus, successively-opposed to each other in their distinctive character.†

Owing to the long continuance of this remarkable ærial disturbance, a fine opportunity presented for testing the chemical and mechanical effects of its powerful electric current. Of this I availed myself in several instances; the results, which I shall not at present detail, proving such as to raise sanguine anticipations in my mind relative to its occasional employment as an agent of decomposition; while the mechanical phenomena incident to the discharge of large electric batteries were not less surprisingly exhibited, in the instantaneous transit of the fluid through thick folds of paper, layers of cardboard, and in other instances, piercing thin plates of glass, and shivering the thicker kinds into pulverized fragments.

Incidental to the mechanical arrangements of my atmospheric machine, and immediately beneath the large terminal ball in the window of the laboratory, is fixed a long mahogany tray for the reception of various electrical instruments, which, as circumstances may require, are connected by stout wire communications with other series of apparatus placed on the table, &c., in different remote parts of the room. Among those employed to-day, the electric fluid was seen playing in a zig-zag form, and gambling, as it were, in all directions; moreover—though found to be perfectly under command, by moderate attention to the provisional means of safety originally adopted—so profuse was the igneous stream in this instance, that while it was abundantly supplied within doors, a similar discharge of flashes and sparks was frequently given off *identically* to the knob of a secondary safety-rod, contiguous to one of the insulating stations on the outside of the building.' The intensity of the electric action (almost constant for nearly three hours) proved so great, that the surfaces of the terminal and receiving balls in the locality of the line of discharge, were afterwards found extensively oxidated, and forming a beautifully radiated appearance to the depth of nearly half a line in the solid metal. In completing the arrangements to which I am indebted for this grand display, I had certainly contemplated, that some very extraordinary

† The origin of this idea, that thunder clouds are constituted by a series of concentric electrical zones, belongs, 1 believe, expressly to Andrew Cross, Esq. I have only to add, that many recent experiments which I have had an opportunity of instituting, by means of the machinery mentioned in this paper, tend unequivocally to confirm the opinion.

and interesting results would obtain, but I must freely confess, that my most sanguine anticipations were left far behind by the splendour and sublimity of this day's exhibitions; the whole scene being one continuous presentation of astonishing magnificence, a competent idea of which I specially feel my inability to convey.

Shortly after three o'clock, p.m., the clouds gradually dispersed; a fine blue sky succeeded, and even a trace of free electricity could no longer be detected by my most delicate test instruments. Nature, as though wearied by over exertion, now seemed to express a determinate repose.

While the brilliant phenomena I have attempted to describe were passing, the atmosphere of my laboratory (a room 18 feet by 12, and 14 feet in height) became so thoroughly impregnated with a peculiar effluvia disengaged by the electric current, that persons who entered were immediately sensible of its prevalence, and repeatedly uttered expressions of surprise at the strong phosphoric odour manifested. Every practical electrician is familiar, at least in a minor degree, with the remarkable effect produced by this odour on the olfactory nerves. I have experienced its development from even comparatively moderate discharges of atmospheric electricity, on several former occasions, but, perhaps, the quantity which must have been set free in the present instance, is without a parallel in the records of the science.

In reference to what I shall have to subjoin, I feel that it will be merely an act of ordinary justice in me to state, that on the following day (Sept. 17th,) I was favoured by a visit from my friend W. G. Lettsom, Esq., who, immediately on my detailing to him the experiments, &c., of the preceding day, and more especially my observations on the prevalence of the extraordinary phosphoric odour, directed my attention to the translation of a letter from M. Schöenbein to the celebrated Arago, "On the nature of the odour manifested in certain chemical actions," which letter had appeared in the *Compte Rendu de l'Academie des Sciences, No. 28.* In this paper M. Schöenbein maintained that the phosphoric odour arising from the action of an electric current, constitutes a peculiar "odoriferous principle," to which, from its most evident character, he gives the name of *ozone*, and he feels almost sure that it should be ranked in the class of bodies to which chlorine and bromine belong, that is, among the elementary and hologeneous substances. He further concludes that *ozone* must be disengaged every time

* It is a singular coincidence, as since appears, that at the identical time of my experiments and observations, and that also of M. Schoenbein's paper becoming known to me, the latter was actually being read in the chemical section of the British Association then in progress at Glasgow.

that sparks and lightning traverse the atmosphere. To the correctness of the latter conclusion, my own experience in atmospheric electricity will enable me to bear ample testimony. From M. Schoenbein's conviction, that this body is always disengaged in the air, and in perceptible quantity, in stormy weather, he proposes a series of experiments,† which he considers sufficiently interesting to be undertaken every where—and of this there can be no doubt—by placing plates of platinum in very elevated situations, taking care to make them communicate with the earth. As this metal *invariably acquires negative electricity* (according to M. Schoenbein) by the action of the "odoriferous principle," it may be concluded that *ozone* is developed when the platinum is found to be negatively polarized, and this fact must be ascertained by means of an exquisitely delicate galvanometer. The series of experiments proposed by M. Schoenbein, to be effected by means of elevated discs of platinum connected with the earth, I find to be of as practicable nature as could well be desired, when the apparatus communicating with my ærial exploring wire is employed for that purpose; and, as I shall presently endeavour to show, this arrangement, or certain modifications thereof, would be admirably adapted, both in principle and operative detail, to a *further* prosecution of the inquiry, in relation to the electro-chemical and other effects of *ozone.* Whenever a free electric current in the form of sparks is passing from the terminal ball of the apparatus to the earth—and this is a *frequent* occurrence during hasty showers of either rain, hail, or snow ; dry easterly winds, &c., to say nothing of thunder storms—if a piece of platina foil, some three or four inches square, fastened to a wire, and held in the hand of the operator (if the discharge be not too powerful at the time, and if so it must be otherwise supported) be brought for a few seconds into the vicinity of the ball, and more especially in the direction of the current, the plate necessarily acquires a negative polarity, which may be immediately shewn by the galvanometer, and the experiment repeated with the greatest facility as often as desired.

It will be remembered that the great atmospheric disturbance, hitherto forming the subject of this paper, took place on the 16th September. The remaining part of the month proved also highly prolific in similar phenomena : the 19th, 22nd, 24th, 25th, and 29th furnishing instances of powerful electric currents, scarcely inferior in quantity and general effect to that already described. Nor was the succeeding month less interesting to the electrician, in this respect, except that the force of the atmospheric current appeared upon the whole to be very gradually declining. I shall, however, pass over any especial notice

† We have introduced M. Schoenbein's letter next article but one.—*Edit.*

of these events, and extract from my journal the Memoranda of *October* 28th.—During the greater part of last night and this morning, the atmosphere has been in a highly electrical condition. Late last evening distant lightning flashes were repeatedly observed; subsequently a gale came on from the S. W. accompanied by hail storms and heavy rains. Early this morning a powerful current of dense sparks, from two to three inches in length, and of the apparent thickness of one's little finger, having great intensity, passed from the terminal ball to the earth ; and the splendid scene it furnished has been frequently renewed in the course of the day. Many interesting experiments were highly effective, but more especially the following instituted expressly with a view to prove the disengagement of *ozone*, as recently supposed, by M. Schöenbein, to occur during the passage of the electric spark through the air. The little apparatus used on this occasion as an appendage to my atmospheric machine, and which I will now describe, had been prepared 'in readiness a few weeks since, though no convenient opportunity for employing it has hitherto presented.

The cylindrical glass vessel *a*,fig.1,pl.1,three inches in diameter and nine in height, is fitted with a metallic cap *b*, from the centre of which rises a stout wire eight inches in length, but at right angles some three inches from its extremity, where it is terminated by a brass ball *g*, one inch in diameter. The cap of the instrument, *b*, and the substantial metallic foot-stand *c*, are respectively furnished with a narrow rim into which the ends of the cylinder are accurately ground so as to fit somewhat tightly, yet capable of being easily detached at pleasure. To the under part of the circular plate *d*, which is nearly equal in diameter to the glass itself, and has a rim turned up all round, is soldered a tube *e*, sliding freely over a second tube *f*, the latter being soldered firmly to the centre of the foot-stand *c*. By means of the sliding tube the disc *d* can be raised or lowered at pleasure, whereby its required distance from the upper plate or cap *b* is readily adjusted. The metallic fittings of the instrument are neatly made of sheet zinc, well ground and polished; all sharp edges being avoided, except as regards the rim of the circular plate *d*, which presents an accute edge all round to the cap above.

The glass cylinder having been well warmed, and freed from every particle of dust, the circular plate *d* adjusted to three and a-half inches from the cap *b*, the knob *g* was brought within *striking* distance of the large ball (four-inches in diameter) forming the terminus of the atmospheric apparatus, while a tremendous current of electric sparks was being rapidly discharged. Strong flashes of brilliant light with occasioned dense sparks were immediately seen passing between the two metallic

surfaces within the cylinder, notwithstanding the broad daylight which prevailed. When about fifteen seconds had elapsed, the instrument was withdrawn from the terminal ball, and the cap *b* removed. In an instant it was evident that *ozone* had been most abundantly disengaged, and the atmosphere of the cylinder had become so strongly impregnated with a powerful pungent phosphoric odour, that I found it exceedingly inconvenient to respire over the aperture of the glass; nor could I find any individual out of several present willing to permit its approach towards their nostrils beyond a second or two. A piece of platina foil about three-inches in length and two and-a-half broad was next placed vertically on the stand *d*, and the instrument a second time brought within the striking distance. The former appearances were renewed, and at the expiration of twelve seconds the platinum being withdrawn, it was found, agreeably to the theory of M. Schöenbein, to have acquired a strong electro-negative polarity.

The electric current continuing favourable for some time longer, I was induced, upon a little reflection, to expose several solutions of the different salts, contained in small watch glasses, and supported upon the plate *d*, to the action of an atmosphere strongly impregnated with *ozone* produced by the passage of a dense electric steam through the cylinder. That certain chemical changes took place in these solutions admits of no ambiguity in my own mind, though I was not enabled to pursue the experiment for a sufficient length of time to arrive at satisfactory conclusions. As opportunity may present, I design to repeat these experiments, and also, if possible, to ascertain the action of *ozone* on several gaseous and other bodies.

In conclusion I will mention that, while experimenting with the little apparatus above described, I placed a great number of small irregular pieces of dry elder pith, the angles of which were acute and uncertain, upon the moveable plate *d*, and, while the electric current was passing at the acme of its vigour, several of these took fire and burnt with great brilliancy. The fine and delicate angles presented by the fragments of pith, acting as so many points opposed to the cap of the instrument, the intensity of the current being great at the time, ignition almost immediately took place; the fragments of pith the while remaining stationary, or exhibiting only very slight motion occasionally.—During all the experiments recorded to have been made on the 28th, the electric current was invariably negative.

Sandwich, Dec. 22nd, 1840,

On the Identity of the Ordinary and Voltaic Electricity. By J. GOODMAN, Esq., M. R. C. S., &c. (continued from page 13.)

Read at the Royal Victoria Gallery, November, 1840.

29. To imitate the electro-chemical actions of the galvanic fluid by ordinary electricity, we are obliged, as before stated, to proceed in our experiments with a *perfectly continuous current*, and, for reasons hereafter to be mentioned, to obtain a view of the gas generated, (40, 43) to confine the current in conductors of a most limited magnitude*.

30. It appears to me there have been two principal causes why analogical decompositions have not been effected. The first has been that a sufficiently fine and delicate diameter has not been given to the conductors or guarded poles; (26) (Wollaston's method of constructing such as would decompose by current alone being difficult and tedious .he first obtained a nitro-muriate of gold in solution, placed the same in a glass tube, melted the tube with the blow-pipe, by which means he also evaporated and drove away all the acid, and then, by drawing out the tube, rendered the metallic lamina of gold exceedingly minute. His results, however, being, on certain accounts, unsatisfactory, as has already been pointed out.) The magnitude of the poles being too great, has induced the experimentalist, from the great paucity of fluid in the ordinary electric current, to *separate* some portion of the conducting material and *produce sparks or shocks* in the circuit, and thus render gas visible where none can be obtained by the mere current, which, as shown in my last, destroys the identity of effect of the two fluids, (21.22.27).† The second cause of failure has generally been the *deficiency in qnantity* of the current fluid—producing such scanty developement of gas—that it would have been folly in the operator to have attempted to procure as much as could be submitted to the test of explosion by the electric spark.

31. The method which I found necessary to adopt in the construction of the guarded poles, was the following :—I precured a proportion of the finest platina wire obtainable in town, *hammered its extremity* until it formed a flat plate, whose *area* was about 10 or 12 *times the diameter of the wire ;* and with a pair* of scissars *cut it into as fine a point and filament as possible*, and placed the wire in a small glass tube, melted the extremity until it adhered to and covered the filament, and afterwards by grinding exposed the point sufficiently *for a small spark from the machine just to pass*—which point could by no means be discovered either with the naked eye or a microscope. A curve was also given to the lower part of the glass tube, see g. g. fig. 2.

* This applies only to the guarded poles, (26) see now 47.
† See Dr. Faraday's 3rd series, (329)

32. Finding the electro-chemical powers of the current alone from the machine exceedingly low, I became desirous of availing myself of the powers of the increasing battery, before described. (22). But the great difficulty of discharging a succession of *charges* in such a manner as to produce a continuous current appeared insurmountable. Meanwhile proceeding to construct a machine for the purpose of producing the two kinds of electricity upon the opposing surfaces of an electric in the state of proximate polarization, (14.15) imagining that by this means the quantity might not only be augmented, but a fluid more nearly resembling the galvanic produced. I mounted a circular plate of common window glass upon an axis similar to a plate machine. One side of the plate was covered with a layer of sealing wax, and a rubber of hair skin applied to it in communication with the ground. On the opposing surface a rubber of leather with amalgam, was made use of, &c. In all ways in which the same could be tested, I obtained vitreous or positive electricity upon each surface, which fluid also could only be collected on one side. The reason was obvious. The positively excited surface by polarization caused the developement of the natural fluid belonging to the opposite surface, (in defiance of hair skin, sealing wax, or any other resinous exciting electric)—as in the phenomena of the Leyden jar or plate machine, (19).

33. The failure of this experiment, nevertheless, put me in possession of a method of obtaining a continous current from the increasing battery. I remembered that the Leyden jar, with moveable linings, can be charged positively and negatively on its surfaces, the coatings can be then removed, and a slow and gradual discharge of the whole of its fluid produced by connecting the inner and outer surface of first one portion and then another by the common discharging rod. It struck me that a modification of this experiment might readily fulfil the object desired. Removing from the glass plate, already described, the rubbers, and in place of them applying two metallic moveable coatings, c d, fig. 1, in imitation of the Leyden jar, I charged the surface by one coating positively, and removed the natural positive fluid from its opposing surface by connecting the other coating with the insulating negative conductors of my machine. The *fluid from the charged surfaces* was collected by two other insulated metallic coatings, to which were attached two metallic poles. On receiving the fluid through each hand, I was delighted to find an entirely novel physiological effect. The sensations produced by the electro-magnetic machine were exactly imitated. I proceeded to effect electro chemical decomposition, and obtained my first decomposition of water by this machine, (21) but the quantity of fluid in the ordinary electrical current being so exceedingly low, now induced me to take ad-

vantage in its construction of the principles of the increasing battery already described, (18 g, 22.) I procured six plates like the one in question (except the coating of sealing wax), see plate fig. 1, a a a a mounted them on an insulating axis, b. b. b. and applied twelve moveable coatings, c. d. c. d. &c. as in the last described apparatus. The first coating was placed in connection by the wire p. with the positive conductor of the machine—its outer surface and coating *d* being in communication by the wire *h* with the inner surface of the next plate at *c*, the disturbed natural fluid was given to the latter—and so on throughout the series. The last surface was also deprived of its fluid by connecting the wire *n* with the negative conductors of the machine. By this method, however, it was found that the expected accumulation of fluid could not be obtained,* the great distance of polarization required, on account of the thickness of the glass, being too much for the powers of my electrical machine, and I have since found that the greatest quantity is produced by dividing the fluid from the prime conductor, and charging in two situations instead of one. I now charge the inner surface of the first—and that of the fourth with positive fluid—and remove the fluid from the outer surface of the third and sixth by communication with the negative conductors. The surfaces thus charged are, by rotation, brought round to the insulated coatings of the poles—all the positively charged surfaces by the medium of f f f f being in communication—and all the negative also connected one to the other by the communicating wires g g g g. To each of these the wire poles p p and n p are attached, and the increased current of fluid so obtained is transmitted through them to any object which it is intended to subject to the action of the current. From its chemical effects, I have reason to believe that the fluid from the machine is by this means increased—(supposing that polarization has no direct influence in decomposition)—(26) to threefold its original amount.

34. I have endeavoured, for some time, to procure *six plates of mica*, of a sufficient magnitude, to enter into the construction of a polarizing machine, which, from its *extreme thinness*, would, I doubt not, augment prodigiously the amount of fluid in the current through the poles—probably 12 or even 24 plates might as easily and as perfectly be polarized as one-eighth their number of common window glass—and the fluid so eliminated would also *resemble much more nearly* (as before remarked) the fluid from a galvanic battery, both in tension, quantity, and all other properties, as described in the chart, (5).

35. The method of receiving the electric fluid by *moveable coatings*, as at first adopted, when submitted to the test of chemical decomposition, was found to be as objectionable as the

use of the Leyden jar, (22) and sparks, as exhibited in my former paper. The gas generated was found to be a mixture of hydrogen and oxygen at each pole, but more nearly aproaching the galvanic decompositions—being at the negative pole *only one-sixth oxygen* and *five-sixths hydrogen*. This was readily accounted for. *Once or twice* in every turn of the machine, a *distinct shock* could be perceived by its usual *diagnostic*—the *stdr* on the platina point—which shock caused the elimination, as before stated, of both hydrogen and oxygen at each pole.

36. I next collected my current at the origin of the positive pole by *needle points*, and the *true* electro chemical decomposition of water appeared now to be obtained, and which has since been established. The appearances, when true decomposition is taking place are as in galvanism. •A *constant stream of minute equimagnitudinous, bubbles*, issuing from the positive pole *in one continuous thread-like current*, the bubbles from the *negative* in *double size or quantity*, *and there being* in the positive current at *no period an increased bubble*, unless the gas adheres to the termination of the pole. Still, occasionally an accumulation presented itself in the current, and I determined at once to collect the fluid by means of the points (f f f f, &c.,) in connexion with each individual surface, and thus prevent the occurrence of shock in the current altogether, since no production of pure hydrogen at the negative, or of oxygen at the positive pole could ever be obtained, whilst shocks continued, to make their appearance. This being effected, I have now the satisfaction to state that, from the result of decomposition with this machine, the gas is obtained in a manner perfectly identical with galvanic decomposition.

37. In conjunction with Mr. Sturgeon and Mr. Eaton Hodgkinson, on the 13th of October instant, I succeeded in obtaining by this means, in less than two hours' turning, a small bubble of about one-eighth of an inch in length in a tubular receiver. The gas arose from the negative pole from whence hydrogen arises in the voltaic arrangements, and in passing *ten or twenty sparks through it no diminution in the magnitude of the bubble was at all effected*, see (22). On *adding a portion of atmospheric air* to the same, (no oxygen being collected) and passing a spark, an *immediate diminution* in the volume of the gases was observed, shewing evidently that the former gas was pure hydrogen. At the Victoria Gallery, Nov. 18th, 1840, I passed sparks through two or three portions of gas (hydrogen) already obtained, and no diminution in the quantity could be perceived. On adding a portion of oxygen from the other receiver, and passing a spark, the gas instantly collapsed. It is worthy of remark that one portion of gas exhibited at the Victoria Gallery, *one-eighth of an inch in length in the tube receiver*, was produced in *one hour's* turning by the aid of the *electrical machine alone*. I have reason

however, to doubt the purity of the gas in this case. There must have been a break in the current, but none could be detected, although a suspicious sound of exceedingly minute sparks was perceived and mercury connexions were not used throughout the circuit. When a decidedly unbroken current is employed generally double this length of time is required to produce this quantity of gas. The reason of so rapid a formation of gas was, that a high pressure *steam engine,* was employed in turning the machine, which revolved in consequence steadily, with about three times the rapidity that could have been effected by manual labour. This is indeed the most simple and least expensive method by which these long, and otherwise tiresome and laborious experiments can be performed—and which I now adopt in all researches of this nature.—See Dr. Faraday's 3rd series, (356).

38. I now beg your attention to the statements made by electricians, *some of whom* have, it is said, produced decomposition of water with ordinary electricity, identical with galvanic, in which separate gases are reported to have been given off from the poles ; and I think you will agree with me, from facts which I shall shortly exhibit, and others already before you, that if we are to believe their statements, we must at the same time deny that identical decomposition could have been effected by the methods adopted. See also (11). Dr. Wollaston's experiments were performed chiefly by sparks from the machine, and in one instance, by current alone, with the fine thread of gold already described. But in the philosopher's own words we have a candid acknowledgment that the decomposition was not identical ; for, says he, "in every way in which I have tried it, I observed that each wire gave both oxygen and hydrogen gas, instead of their being formed separately, as by the electric pile. Sir Humphrey Davy immersed a guarded platina point, connected with the machine, in distilled water, and dissipated the electricity from the water into the air by moistened filaments of cotton."—(Dr. Faraday, note to 471, series 5.) Had *two poles* been used in this experiment there would have been some plausibility in the stated result, that oxygen and hydrogen were separately obtained. I find, indeed, that what appears to be true decomposition does occur where *two guarded poles* are employed, and the negative terminating in a point and dissipating the electricity into the atmosphere, but I also find that the *quantity of gas* is by this means *much less* than when in connexion with both the positive and negative conductors ; and judging from the exceedingly small amount given off in this way, I should be inclined to disbelieve that the gas was obtained in quantity sufficient to be tested in the usual manner. But to suppose that gas would be eliminated from cotton filaments, which, as we gather from the statement, formed the

negative pole, would be very improbable. I should, indeed, from an experiment immediately to be related, (40,43) doubt the reality of any appearance of hydrogen altogether. In the experiment of Mr. Barry, communicated in 1832 to the Royal Society, it appears that he used two tubes, each having *a wire* within it passing through the closed end. as is usual for voltaic decompositions. Further on in his description we find him stating that the decomposition was performed by lightning, obtained by an electrical kite, and that the *intensity* of the electricity was exhibited by the " *usual shocks on touching the string.*" Now, to attempt to procure decomposition of water and development of the gases by a wire of moderate dimensions, uncoated, and exposed to the surrounding liquid for its whole length, merely with electricity, the shocks of which any human being could endure, would be, according to all known results, utterly useless. Nor is, I believe, the statement of Barry at all relied upon, see Dr. Faraday, (339, 340, 341, 342). Lastly, Dr. Faraday, 1833, in imitation of the experiments of Wollaston, says, (330) " when what I consider the true effect only was obtained, the quantity of gas given off was so small that I could not ascertain whether it was as it ought to be, oxygen at one wire and hydrogen at the other." " The quantities were so small that on working the machine for *half an hour*, I could not obtain at either pole a *bubble of gas larger than a small grain of sand. If the conclusion which I have drawn,* (377) (that the chemical power is in direct proportion to the absolute quantity of electricity which passes), *this ought to be the case.*" We have, therefore, as it appears upon record, no authenticated case of true electro-chemical decomposition of water by frictional electricity. Dr. Faraday, after recording these statements, even doubts (356, 359) the possibility of any common electrical machine having as yet supplied electricity enough in a reasonable time to cause true decomposition, although the plate of the electrical machine with which he performed his experiments was fifty inches in diameter, and from which, according to his own statement, sparks of ten to fourteen inches in length could easily be drawn from the conductors, (see 19, 14).

39. It had frequently occurred to me, that if the union of the galvanic and ordinary electricities could be effected, so as to act in concert in the decomposition of water, a perfect proof of identity might by this means be exhibited. For this purpose I effected decomposition with a voltaic battery of ten or twelve jars, (couroûnes de *tasses*) by the medium of the guarded platina poles. The formation of gas was exceedingly feeble. The quantity generated by no means exceeded that of decomposition by three polarizing plates with ordinary electricity. I then arranged two wine glasses of distilled water, in which four guarded poles were inserted (two in each) so that there should

be a galvanic and an ordinary electrical pole in each glass, viz,,
a negative pole from the machine and a positive from the voltaic
battery in one glass, a negative from the voltaic battery and a
positive pole from the machine in the second glass. Knowing
that the voltaic battery could effect no decomposition without an
opposing pole, and that the same might be stated of the frictional
poles, I believed that if this experiment should succeed, the
final identity was perfectly established. The experiment suc-
ceeded, and gas was generated by each of the four poles. This
occurred on the 18th September. But shortly afterwards I
found that a similar effect might take place with the frictional
fluid alone, from its exceedingly great tension, the plates and
fluid of the battery serving to connect the poles of one wire glass
with those of the other, and thus decomposition might take place,
(as it were,) in one continuous current from the positive to the
negative poles of the polarizing machine. I afterwards found
that by six poles thus arranged in the circuit, decomposition went
on as well as it had done by two only. Such, indeed, is the
tension (17.b) of ordinary electricity, that it is difficult to say at
what number of poles thus arranged it would effect decomposi-
tion. The effect, indeed, seemed to be heightened by increas-
ing the number of poles.

40. Now by this time I was perfectly satisfied that a true de-
composition of water was effected not only by the polarizing
apparatus, but by the current alone from the electrical machine.
Yet as some remarks are made by electricians *objecting to the
use of the guarded poles*, I determined upon attempting decom-
position by unguarded ones. Dr. Faraday, for instance, states
in a note to (p. 133) that the experiments of Sir H. Davy with
Wollaston's poles, do not remove any of the objections he has
made to the use of Wollaston's apparatus as a test of true chemi-
cal action. The objections are as above stated (328)—" That the
water is decomposed at both poles, independently of each other."
" That the poles have *no mutual decomposing dependence*, may
be shown by substituting a *wire*, or the *finger*, for one of them,
a change which *does not at all interfere with the other*, although
it *stops all action* at the changed pole ;" and (337, that the ef-
fects with Wollaston's apparatus are probably resulting "*from
high temperature acting on minute portions of matter*," or " *con-
nected with results in air*," " nitrogen being able to combine
directly with oxygen under the influence of the electric spark."
When shocks or sparks are used in the decomposition, water
probably may be decomposed by one pole independently of the
other, or with no mutual decomposing dependence, there being
an elimination of combined gases, as before exhibited, (see now
46). But where *a perfect decomposition is effected* where *oxygen
alone* is produced by one pole, to imagine that the *hydrogen*
becomes *annihilated*, and decomposition still proceeding, would

be highly unphilosophical. In imitating the related experiments of Sir H. Davy, I found, moreover, that a thick wire substituted in the place of one pole *does not prevent* decomposition at the remaining pole, and that *gas is eliminated in just the same thread-like stream as before.* I found also that by connecting the thick wire with the ground, in imitation of Barry's experiment, a similar current of gas was still obtained; and yet *none* made its appearance upon the wire substituted for the guarded pole— *oxygen being generated* and *hydrogen not making its appearance.*

41. Having fortunately in reserve *two hammered platina poles* (31) rendered exceedingly fine, which were constructed for decomposition, and *without any glass or other coating upon them.* I passed the point of one anxiously yet slowly beneath the surface of the water, and when about the eighth of an inch immersed, I had the satisfaction to behold the same speedily covered with minute bubbles—the decomposition proceeding as usual at the guarded pole during the whole period. I speedily removed the guarded pole from its electrical connections—and substituted a second unguarded pole of a similar magnitude, and on turning the machine for a very short period, *both poles were entirely covered with gas,* the *negative* in about *two-fold* quantity—and thus a decomposition of water was effected *perfectly identical with galvanism,* and that from the prime conductor of the machine alone, and *subject to no objections of electricians on the ground of the metal being coated with non-conducting material,* (see now 49).

42. With these poles I have also decomposed sulphate of copper by current from the machine alone—metallic copper appeared at the negative pole which may also be observed now upon the same—and gas (which I suppose to have been oxygen) was given off continually from the positive—and none from the negative pole.

43. It is not, therefore, difficult to suppose that in the substitution of a *thick wire*; the *finger,* or *filaments of cotton,* for the guarded pole, *particles or atomic portions of hydrogen might be continually being deposited upon the surface of* the latter, *and yet not visible* to the naked eye. For taking into consideration the length of time required for the deposition of bubbles upon the minute unguarded platina pole—whose thickness is but the 960*th* part of an inch, length immersed *one-eigth,* and total breadth one 96*th* of an inch, it seemed probable that the length of time required to render bubbles visible to the naked eye, or seen through a microscope upon the whole surface of an unprepared wire, (see now 47, 48, &c.) the finger, &c., might occupy a considerable period. The same reasoning would also, I believe, apply equally to any other conducting body which might be introduced for the purpose of forming a negative pole, or convey-

ing away the electric fluid (if the formation of gas at the opposing pole still remain unaltered), and it is worthy of remark that *without some conducting material no decomposition takes place at all,* shewing that there is at least *some mutual decomposing dependence.* (46).

44. By diminishing the powers of the voltaic battery we find perfectly identical results, (a). Thus, if a syphon be interposed between the vessels in which a small galvanic pair of zinc and copper wire is introduced, or a long column of acidulated water between the copper and the zinc by employing a thick copper wire, *ten or twenty minutes* is required *before any bubbles make their appearance,* especially if it be immersed the depth of an inch or two, (b). Under such circumstances it would not be improbable after waiting ten minutes or a quarter of an hour and no bubbles making their appearance, that an electrician might give up the experiment, imagining that decomposition was not taking place at all, (c). But with a moderately fine platina wire, or the unguarded platina pole, (41) gas is generated and makes it appearance *immediately.* (d) Now any individual seeing this voltaic decomposition or that of the F.E* (41), with the same unguarded pole, I am sure would be utterly unable to point out any difference. (e) From these and other facts I have been led to conclude that in many electrical experiments, where a current appears to circulate through a given amount of water, *no current really passes,* but decomposition takes place upon the whole or part of the surface of the conducting bodies, the *natural electric fluid of the water, affording to them a sufficient supply to cause the re-appearance* of the current at the same— and which decomposition may not be cognizable on account of the time required to render it apparent to the senses; at all events the line of demarcation is not yet laid down, *upon what quantity of metallic surface* electro-chemical decomposition *is effected* by ordinary electricity and *where it is not.*

45. If we now refer to the chart (5.) we shall find a remarkable coincidence between the properties of ordinary electricity during decomposition, and the characters of the galvanic fluid, taking into consideration the effects of the electrical machine alone—since, as has been already demonstrated, perfectly identical decomposition can by it, under given restrictions, be effected. 1st. We have now " a *continuous current,*" both by the polarizing machine and by the ordinary method. 2nd. That the currents have a *mutual attraction* for each other, and the poles, *consequently a mutual decomposing dependency,* (in addition to what has already been stated,) is admirably exhibited in the following manner :—Let the *positive conductor* of the machine be in conducting *communication* with the *building, floor, table,* or even with the *gas pipes* of the town, the " *discharging train"* of Dr. Faraday, and by which, as is *supposed, all electrical effects are annihilated ;* let the *negative conductors*

* F. E. means frictional electricity, and G. E. galvanic electricity.

also be in conducting communication *with any other part* of the building not in connection with the *latter*, and yet *decomposition shall still proceed at the decomposing apparatus*. The *positive* conductor may even be *directly connected* with the *negative*, providing the *lacquer* of the same *intervene* and decomposition will be unaffected. By this latter experiment we produce perfect agreement in character with the galvanic fluid, the tension or passing distance *within* the circuit you observe is *atomic* or thereabout—although this machine will give four-inch sparks or more when no decomposition is taking place—(see 5. 3.)—I should say indeed, that the *fluid* under these circumstances is *Galvanic, not ordinary electricity.*   4. Inconductible by water when decomposing (44a e) 5. No one would doubt at all the *diminished powers in attraction and repulsion* when not a spark can be obtained. Light bodies (pith balls, &c.) may be held indeed in the vicinity of the conducting wires or attached to them without any exhibition of attraction or repulsion.   6. Physiological effects only by making and breaking contact, powerful or not, according to the tensive force.   7. I have just shewn you that F. E. *does not require insulation* more than G. E.   8. I have reduced the quantity of the voltaic current to little more than that of an electrical machine.   9. And lastly as long as the fluid is incapable of passing *at all to any body out of the circuit, we cannot expect any accumulation*, more than by galvanic arrangements.   As to the number 10 we have already shewn to what modification it belongs.

46. Not being perfectly satisfied whether the poles would effect decomposition *by shocks* (40) separately, or without any mutual action : I placed two guarded poles in *separate* wine glasses, filled with distilled water, one being in connection with the inner coating, and the other with the outside of an insulated Leyden jar. It did not appear impossible that the two *oppositely electrical conditions* (27) of each pole, might be the means of eliminating oxygen and hydrogen in each vessel independently of the other. Upon attempting to pass shocks, no effect at all could be produced, and not the slightest trace of gas observed. Upon inserting the ends of a copper wire in the glasses to form a communication, decomposition instantly proceeded, visibly at the guarded poles, which could not occur according to any acknowledged principle, unless the copper wire acted the part of a pole in each glass.—This experiment by being prolonged led me to the following :—

47. A piece of copper (bell) wire being inserted in distilled water in which a guarded pole was placed, by passing a number of slight electric shocks through the same (as above) in fifteen or twenty minutes turning, *the copper wire itself* became *covered with bubbles of gas,* decomposition proceeding at the guarded poles as usual, (40.48). The same also occurred by the agency

of the current alone. The results of this experiment apparently
so contradictory of that in which the *guarded* pole of too great
magnitude (26) was employed ; where decomposition could
not be effected by platina wire reduced to a moderately fine
point (and which I have frequently attempted with similar re-
sults) induced me to believe that the decomposing effect was
due to the agency of the guarded pole, and (with 45) led me
to the following conclusion which I believe will still remain
correct.

48. By the use of the *guarded poles*, or indeed *any* which per.
se. are capable of effecting aqueous decomposition, the ordinary
electricity appears to be placed (with the exception of slightly
encreased tension) (17.*b*) in the entire condition of the galvanic
fluid. Remote polarization (13.14) is by this means exchanged
for atomic (5). Its expansive state reduced to the concentrated
(17.*c*) tension exchanged for intensity, or quantity in a given
amount of matter.

49. Entertaining this view of the subject I arranged three
wine glasses A. B. C. fig. 2, filled with distilled water, so as to
form a line between the wire soldered to the positive conductor,
and that proceeding in the same manner from the negative.
Into A I inserted a guarded pole g in connection by a mercury
cup with the positive wire. In C I also placed a second guar-
ded pole g. The view I had taken (48) led me to conclude that
if the positive, and negative currents were introduced by poles
capable of effecting decomposition, the fluid being thus placed
in the condition of galvanic, whatever might be the nature of
the intervening poles, if capable of decomposing by galvanism,
they would also perform the same by this means, provided that
a sufficient quantity of electric fluid was supplied by the introduc-
ing poles. Two common platina wires e. f. (the latter of consider-
able thickness) were bent at right angles at their extremities, and
placed in a line between the guarded poles—their terminations
being inserted about one-eighth of an inch below the surface of
the water in the glasses, and a slight distance from each other
and the guarded poles—one end of e being in A, the opposite
in B,—one termination of f in B, the opposite in C. On turn-
ing the machine for about *five minutes, very minute bubbles*
began to make their appearance on the inserted termination of
each wire. In from *ten to twenty minutes*, very considerable
bubbles were formed, as visible and distinct as in any galvanic
decomposition, which also *commenced ascending* from the sur-
face of the wires. Thus we have a perfect decomposition of
water by the *current alone,* effected in the central glass B, *by*
two unprepared and moderately thick platina wires, as truly as
by galvanic agency.

I next arranged a piece of copper bell wire between B and C
instead of the wire f. *Decomposition became visible by the*

deposition of bubbles on this also, but from its magnitude requiring a longer period of turning. (Dec. 19, 1840.)

50. I now proceed to introduce the currents into the glasses A and C, without any guarded pole whatever. In A, a thin copper wire was inserted about one-eighth of an inch, in connexion with the positive conductor. From A to B one of the former platina wires e, and between B and C, the other f inserted as formerly. The pole from the negative mercury cup being a thick bell wire, inserted about the same depth. In *two minutes and a half, gas was evident upon every termination,* especially *upon the C end of f.* This experiment is exceedingly remarkable when contrasted with the attempt at decomposition by guarded poles (26.)

51. Having observed so unexpected and rapid decomposition in the last, I determined to attempt the experiment by two thick unguarded wires, inserted in a single wine glass. I placed about one-eighth of an inch of each of the copper (bell wire) poles proceeding from the positive and negative conductors of the electrical machine in distilled water. *In three minutes* (the revolutions of the cylinder being about 60 per minute) *one or two bubbles manifested themselves upon the termination of each wire, and by the microscope, smaller, ones could be seen all over the inserted surface of each.* In *five minutes distinct bubbles* were seen by the *naked eye, upon all parts of the wires below the water, assuming a frosted appearance,* and about double the quantity upon the negative pole. *In half an hour* the covering, especially of the negative pole, *might be seen at the distance of two yards,* and as fair an electro-chemical effect, as is ever observed by voltaic electricity.

January 5th, 1841.

Researches on the nature of the Odour which becomes manifested during certain Chemical actions. By M. SCHOENBEIN, in a letter to M. Arago.[*]

The very interesting notions which you have made known in the *Annuaire* for 1838, encourage me to communicate to you the results of some researches which I have lately made on the nature of the odour named *electric.*

Some years ago, I was struck with the perfect analogy which exists between the odour which is developed when ordinary electricity passes from the points of a conductor to the

[*] *Comptes Rendus,* May 4th, 1840.

surrounding air, and that which is disengaged whilst water is
decomposing by a voltaic current.

After having made many ineffectual experiments for the
purpose of discovering the connexion which exists between the
two above mentioned phenomena, I finally arrived, not at a
complete solution of the problem, but at a point, from which
one may get a glimpse at the true cause of the electric odour.
The facts which relate to this subject are the following:—

1st. The phosphoric odour developed during the electro-
lization of water is disengaged at the positive electrode only.

2nd. The disengagement of the odourous principle depends:
—(1st) on the chemical nature of the substance employed by
the positive electrode: (2nd) on the chemical constitution of
the electrolytic fluid placed between the electrodes: (3rd) on
the temperature of that fluid. As to the first condition I have
found that, of all the metals, gold and platinum are the only
ones that permit of the disengagement of the peculiar odour.
The more easily oxidable metallic substances do not develope
the least trace of it, neither does carbon, which is also a good
conductor of electricity. With regard to the connexion that
exists between the chemical constitution of the electrolytic
fluids and their facility of disengaging the odourous principle,
my experiments have demonstrated the following:—The electric
odour is developed at the positive electrode, when the fluid
consists of distilled water mixed with sulphuric acid, phosphoric
acid, nitric acid, or potassa, or with a variety of oxy-salts.
The odour is not observed when the water contains chlorides,
bromides, iodides, fluorides, proto-sulphate of iron, or any
of those substances which eagerly combine with oxygen. The
disengagement of the odourous principle does not occur if any
of the first mentioned fluids are mixed with small quantities of
proto-sulphate of iron, or with nitrous acid, or with any sub-
stance whose affinity for oxygen is very great. The fluids
which develope the electric odour abundantly at a low temper-
ature, do not disengage it when heated to ebulition. It
sometimes happens that the odour is not in the least manifested,
although the circumstances connected with the operation would
lead us to expect a contrary result. Such cases are most
frequent when the fluid employed is an aqueous solution of
potassa. There are some reasons to believe that the disengage-
ment of the odourous principle is impeded by the impurities
deposited on the positive electrode. According to my experi-
ments, the odourous principle is obtained most abundantly
when the electrolitic fluid consists of water mixed with a sixth
part of sulphuric acid.

3rd. The odoriferous substance disengaged at the positive
electrode may be confined and preserved in well corked bottles.

4th. When into the flask containing the odoriferous principle (mixed with oxygen) we drop a pinch of pulverized charcoal, or fine filings of iron, zinc, tin, lead, bismuth, arsenic, antimony, or a few drops of mercury, or of nitric acid, or of a solution of proto-sulphate of iron, or the proto-chloride of tin, or of iron, the electric odour is almost instantly destroyed. At high temperatures, gold and platinum produce the same effect.

5th. When we immerse, for some moments, in the flask containing the odorific principle, (mixed with oxygen), a plate of gold or of platinum whose surface is very dry, clean and cold, that plate becomes electro-negative; that is to say, it acquires the faculty of producing an electric current to which it serves as a negative electrode. In other terms, a plate of platinum treated in the manner described, constitutes, with a similar piece of the same metal in its ordinary state, a voltaic element. This element is such, that the current which it produces, proceeds from the ordinary platinum to traverse the liquid to the plate modified by the odorific principle. I call this extraordinary condition of the platinum plate, *negative polarity.* The metals which are easily oxidable, do not polarize negatively by the treatment I have just mentioned. I have demonstrated, about five years ago, that the precious metals acquire negative polarity by being immersed for a few moments in an atmosphere of chlorine, or of bromine.

6th. The state of negative polarity is not developed either in gold or in platinum, or in any metal whatever when these bodies are placed in a flask in which the electric odour has been destroyed by the means above mentioned.

7th. The negative polarity of platinum is destroyed when suspended for a few moments in an atmosphere of hydrogen. Platinum negatively polarized by the influence of chlorine, or of bromine, resumes its ordinary state when suspended in hydrogen. We also obtain the same result by heating to redness the polarized metal.

On the Phenomena of Polarization, and on the Odour produced by Ordinary Electricity.

8th. When a plate of platinum or of gold, whose surface is very dry, clean, and cold, and communicates with the earth, is exposed to the action of ordinary electricity proceeding from a pointed wire attached to the prime conductor of the machine, and when this exposure is accomplished in such a manner that the surface of the plate is situated at a proper distance to receive the electric *brush,* the platinum or the gold acquires negative polarity. This peculiar state is immediately destroyed by submitting these metals to the action of hydrogen or of heat.

9th. The gold or the platinum being attached to the prime conductor as *points of emission*, do not acquire the negative polarity, although the electricity is very powerfully discharged from them.

10th. Those metals which are easily oxidized, do not become polarized by ordinary electricity.

11th. The electric brushes (auras) lose their polarizing force, also their phosphoric odour, when the points from which they proceed are enclosed in a piece of linen cloth steeped in distilled water, or in saline or acid solution. We also obtain the same effect by heating considerably the points of emission of the prime conductor.

There are other facts in connection with the phenomena of voltaic polarization, which I shall not here mention, because a memoir to which all my observations on this subject is consigned, will very shortly appear in the *Bibliothèque Universelle*.

Before concluding my letter permit me to draw some conclusions from the facts that I have hitherto stated.

1. The phosphorous odour disengaged during the electrolyzation of water is due to the same gaseous substance which is developed at the metallic points by ordinary electricity, whether that electricity be positive or negative.

2. With respect to its voltaic action, the odoriferous principle is absolutely similar to chlorine and bromine. With regard to its chemical properties, there exists a strong analogy between the odourous substance and the bodies which I have just now mentioned.

3. The odorous principle is chemically combined with hydrogen, and in that state of combination it is found diffused, either in water or in the atmosphere.

4. This compound, like water, is an electrolytic body.

5. The electric odour is manifested when this compound is electrolyzed, and its electro negative element set at liberty.

6. As the electric brushes, and also lightning, constitute a real electric current; and that the compound, of which I have spoken all this time, is distributed throughout the atmospheric air, it becomes obvious that the odourous principle must be set at liberty every time that lightning traverses the atmosphere; that is to say, the peculiar odour must be developed. I have observed that the odour of this principle is pungent when the latter is very concentrated, and that it has much the resemblance of the odour of phosphorous when that substance is mixed with much atmospheric air. This circumstance explains perfectly, the difference of opinion respecting the nature of the odour produced by lightning.

Being almost certain that the odourous principle should be classed in that genera of bodies to which chlorine and bromine apparently belong; that is to say, amongst the elementary and hologeneous substances, I propose to give the name of *ozone*. As I am convinced that this body is always disengaged in the air in sufficiently notable quantities during stormy weather, I propose to make a series of experiments, during this year, for the purpose of rendering evident the presence of *ozone* in the atmosphere. With that view I shall place plates of platinum in situations sufficiently elevated, taking care that they communicate with the earth. This metal acquiring negative polarity from the odourous principle, we may conclude that the *ozone* is developed as soon as the platinum becomes negatively polar. This kind of meterological experiments I consider sufficiently interesting to be undertaken every where; and I dare venture to engage you to make similar experiments at the observatory for the purpose of verifying the negative polarity which platinum acquires by the influence of *ozone*. I use a galvanometer in which the wire forms two thousand turns, and in which is placed an *astitic* magnetic needle.

I cannot conclude my observations without informing you that, I am, in a great measure, indebted to the truly admirable pile of M. Grove, for the results which I have related: that is to say, to a pile of which the dimensions are very small, but which, nevertheless, gave off fifteen cubic inches of detonating gas, (oxygen and hydrogen from the decomposition of acidulated water) per minute.

On the Formation of Electro-Type Plates, independently of any Engraving, by M. P. Moyle, Esq., in a letter to Mr. Sturgeon.

Helston, January 9th, 1841.

Sir,

In forwarding to you the electrotype plates, representing a view of St. Michael's Mount in Cornwall, &c., it may be desirable to accompany it with a brief description of the apparatus and manifestations requisite for a correct performance of the process.

On a highly polished brass plate of the required size, I, in the present instance, sketched the view with a common steel pen and simple black oil paint, and allowed it to become perfectly dry and hard; it was then polished with a little prepared chalk and the hand, and which appeared to be necessary to remove any stains on the brass, arising from the fingers, breath, &c. &c.

When thus prepared, it was placed on a copper plate about one inch larger than the brass one, and it was cemented to the copper all around at its edge by bees-wax. The copper plate had two handles turned up at right angles, as at a, in margin, about one inch in width, to communicate with the tambourines. (to be described) The copper plate together with the painted brass one, is then deposited in a glased earthen vessel, (a foot tub in my case) and a perfectly clear and saturated solution of sulphate of copper formed on it to the depth of three or four inches; a triangular wooden frame with glass legs cemented to the wood, as in fig. b, is then placed over the copper plate. Two tambourines, made simply by stretching some parchment or bladder over hoops and then placed on this triangular frame. Into these are placed a piece of amalgamated zinc, separated from the parchment by a few pieces of glass tubing, and on the zinc and in contact with it, is placed another piece of copper with a handle also at right angles, and which is to be brought in direct contact with that of the first named copper plate, and bound together with a thumb screw :—into each of these tambourines is placed water acidulated with sulphuric acid, in the proportion of one of acid per cent. Galvanic actions instantly commence, and copper from its solution became quickly deposited on the brass plate, and all other metallic surfaces not protected by bees-wax or some other coating. A muslin bag containing crystals of the sulphate of copper, must be kept suspended in the solution to supply the waste accruing from deposition, and if this is not carefully attended to, the copper becomes deposited in a coarse brown and non crystalline state, and consequently greatly detrimental to the process.

The more perfect the connexion of the back of the brass plate to the copper plate on which it is fixed, the more regular will be the growth of the electrotype plate ; and this necessary regularity of deposition is much facilitated by having the two handles above named, to the copper plate, and having two tambourines for the generation of the galvanic influence, and thus forming two electric currents instead of one, most commonly in use; and even in the latter case, the two handles are of the greatest use for causing regularity of deposit, by alternately changing the connexion with the tambourines as it may be found requisite.

The greater the degree of polish of the plate to be deposited on, and freer it is from oil, wax, &c., the greater I have found the facility of the separation of the two. The diluted acid in the tambourines require to be renewed daily, and I found from six to eight days quite sufficient for the deposition of the copper plate, one-eighth of an inch in thickness.

The above method I find far superior to the suspension of the plate perpendicularly in the solution and subject to the galvanic current from any other kind of battery, although it cannot be denied that it may by such means be much more quickly deposited.

It may not be irrelevant to the subject to state in this place that I have formed many an useful and ornamental article, in beautifully crystallized copper, such as vases, tea kettles, cups, bottles, &c., also fine tubes, and a variety of other articles; the first by first moulding the article in wax, and then covering it with Dutch metal, to give it a metallic surface, and afterwards melting out the wax tubes, by depositing on iron wires, &c. Similar things I have accomplished in silver, from the solution of the nitrate giving the articles the most beautifully embossed appearance ; and it is very evident that gold, platina, tin, &c., &c., may be acted on in a similar manner, from their respective solutions.   It is but proper that I should here acknowledge that several hints respecting these latter performances I received from Mr. Jordan, the late Secretary to the Cornwall Polytecnic Society.

To preserve the beautifully embossed appearance of the article, it should be well washed in a strong solution of the bitartrate of potash, which will remove the acid adhering to the surface of the plate, or article.

The specimens now sent convince me, and I think it will also others, that the finest productions of the art of engraving may be in this manner accomplished.   I am now preparing one on a much larger scale, and with a different preparation of paint, and with which I find that I can produce much finer lines on the plate than I have hitherto been able to execute.   In the instance here alluded to, the brass plate is first rubbed over with a very small portion of almond oil), and after two or three days it takes the paint with great facility.

<div align="center">Believe me, Sir,
Yours, faithfully,
M. P. MOYLE.</div>

Mr. Moyle has been kind enough to favour us with two of his Electrotype Plates, from which prints will be seen at the end of this number. We believe they are the first of the kind that have been made in this country.—EDIT.

The Second Lecture on the Nutrition of Plants; Delivered by J. A. RANSOME, ESQ., Lecturer on Surgery, &c., at the Royal School of Medicine, &c., Pine-street, at the Conversazione held at the Royal Victoria Gallery of Practical Science, Manchester, on the 24th December, 1840.

Mr. Ransome commenced by stating, that at the last meeting, in introducing the subject of vegetable and animal nutrition, he had alluded in general terms to the nature of the materials em-

ployed as aliments, or as engaged in the first processes of
nutrition. He had remarked, that man, although an omnivor-
ous animal, was yet ultimately dependent upon the vegetable
kingdom for his nutriment ; for the flesh meat he took as food
derived its nutriment from the vegetable creation ; so that man
is dependent, directly or indirectly, upon the vegetable king-
dom, for the materials which constitute the aliment from which
his own frame is to be formed. This then, led to the considera-
tion of the materials composing the vegetable kingdom ; and he
had shown, experimentally, that a simple vegetable product—
sugar—contained three elements in a state of combination, car-
bon, oxygen, and hydrogen, which were the three principal
elements entering into the composition of vegetables. But he
had prepared them to look for other elements, in order to com-
plete the series. They next considered from what sources these
respective elements were derived ; and after a short review of
the notion that carbon, their principal constituent was derived
from *humus*, or, as it was also called *humin*, or *humic acid*, he
had ventured, on the authority of Professor Liebig, the celebra-
ted German chemist, to show that much doubt might be enter-
tained on this subject, and that it was more reasonable to
suppose, that, instead of carbon being derived from *humus*, it
was derived from one of the constituents of the atmosphere, for
carbonic acid was found to constitute one-thousandth part of the
weight of the air. From experiments which bore some analogy
to the process of vegetation, he had shown that a much greater
weight was taken up by chemical substances, having an affinity
for carbonic acid, than was required for plants growing in the
same surface in the same time ; without denying the use of
humus, or the ingredient constituting vegetable mould, which
exists in almost all good soils. In connexion with the subject,
he had shown, that, if we examine the constitution of plants, we
shall find almost every one of their elements, such as woody
fibre *(lignin)*, starch, sugar, tannic and tartaric acids, and the
essential oils, contain less oxygen than carbonic acid, with which
the air furnishes them ; and consequently, that, in the act of
taking them in, oxygen must be given out. He had mentioned
this, in reference to the main question—the maintenance of the
purity of the atmosphere ; for if we suppose, from the respira-
tion of man and animals, and the combustion of immense
quantities of carbonaceous matter (amounting, perhaps, to thou-
sands of tons in this town alone), that the quantity of carbon in
the atmosphere had gone on increasing, and that of oxygen de-
creasing, and that thus, after a given time, the materials of which
the atmosphere is composed would become unfit for the support
of life—we should see that, by this beautiful arrangement, the
decomposition of carbonic acid, and the giving out of oxygen, the
purity of the air was constantly maintained ; and that the di-
rection given to the winds by different temperatures also tended

to equalise the purity as well as the temperature of the air
throughout the globe. He had shown, that we need not look
very far for the sources of hydrogen and oxygen, inasmuch as
oxygen might be taken in as a constituent of the air, or might
exist as a constituent of water, in combination with hydrogen,
and hydrogen is necessarily taken in from the water which is
abundantly provided for plants, both in rain and in the dews
which occur in the absence of rain.

We will now (continued Mr. Ransome) proceed to a consi-
deration of what other materials are found to be contained in
plants, and, in fact, found to be essential to their growth.
But we have still one other substance to rank with those
already considered,—nitrogen or azote, the fourth principal
element of plants. Some are disposed to think, that this does
not enter materially into the composition of a plant, except in
the case of some poisonous plants, or those which possess
strong medicinal qualities; but a few words only are necessary
to show, that, unless plants did contain this element, animals
could not derive much nutriment from them. It has long been
known, that, if dogs are fed upon pure sugar, which is
acknowledged to be nutritive when mixed with other things,
they fall into a kind of consumption, lose flesh and strength,
and ultimately die with symptoms of emaciation and decline.
Sugar is one of those substances which contains no azote or
nitrogen. On the other hand, we have whole tribes of men
and animals who live upon nothing but vegetables: yet the
bulk of their muscle or flesh contains a large proportion of
azote. Whence can that be derived, unless the food they take
contains a large portion of it, or we consider man as possessing
the power to assimilate the gaseous elements around him? But
the fact is, they do not take in azote from the atmosphere, and
they would die unless they were provided with something as
an aliment containing azote. We shall see, that, although
sugar and starch exist in the leaves of plants, yet every particle
is surrounded by a thin lamina, of a substance which contains
azote; and it is owing to this that many changes take place
spontaneously, or by means of the root. The simple operation
of fermentation takes place from the gluten, which contains a
quantity of azote, reacting upon the sugar which it invests;
and we shall see many instances in which chemical changes are
effected through the instrumentality of the azotous principle
which accompanies the other more truly vegetable principle.
A computation was made by Boussingault, a French chemist,
that hay contains one hundredth part of its weight of azote.
Hence we see the source of the nutriment of cattle fed on
grass. Wheat contains even a larger quantity; and, in pro-
portion to the nutritious quality of the wheat, shall we find the
greater quantity of azote. And upon this depends the practical

azote; and, in order to get a fair proportion of this principle, they have to take a larger quantity of food. It is singular enough, that one of the articles in daily use in most families should be a substance containing a large quantity of azote—viz. coffee. The caféine or active principle of coffee, contains more azote than almost any other vegetable body. I was in hopes to have exhibited before you this evening a few proofs that some plants contain azote: but unfortunately it has very intractable qualities; and, though it would put out a light, it would not display any very active properties. The form in which it is best known in combination is that of ammonia, or the gas which is the pungent element of the salts contained in the smelling bottle. Ammonia is known to contain a certain proportion of azote, represented by the formula N. H_3, or one proportion of nitrogen or azote, with three proportions of hydrogen; and these two, combined in these proportions, constitute ammoniacal gas. This gas is formed under many circumstances, in the decomposition of vegetable and animal substances, and in the elimination of hydrogen; and therefore we may naturally expect, that, from the immense masses of putrefying matter upon the surface of the earth, the relics of former generations, ammonia must be given out, into the atmosphere. Most of us were struck with the very small proportion of carbon or carbonic acid found in the air, only one-thousandth part of its bulk; yet I showed you also, that when coral reefs are formed in the ocean by myriads of animalcula, these little animals contrive to abstract from the water sufficient carbonate of lime, to form future islands; and yet the quantity of lime in sea-water amounts to only the 12,400th part of its bulk, and iodine is contained in salt water in the proportion of one-millionth. The air itself contains 79 per cent of free azote, about 21 per cent of oxygen, and one-thousandth of carbonic acid; but, unless we provide azote for plants in some other form than that in which it exists in the atmosphere, it is useless. That form is ammonia; and in that form we must look for it in the air, as required for plants. If we take a jar full of atmospheric air, and look for ammonia in it, most of us will be disappointed, the quantity exists in so small a proportion. By

recent calculations, Liebig assumed that every cubic foot of air contains only a quarter of a grain of ammonia: Liebig has, however, found it in the air. Ammonia is a highly volatile substance, at an ordinary temperature existing as a vapour; but if that vapour comes into contact with water, the water absorbs it so rapidly as almost to produce an explosion. Liebig conceived the idea, that if we wish to search for ammonia, we must look for it in the water which falls from the air, and by so doing has found it, as I have done, by repeating his experiments. Liebig took 100 gallons of rain water; he applied heat, and distilled over four or five pints; he saturated it with acid, so as to fix the ammonia; evaporated it, and it left crystallized muriate of ammonia. It appears, however, that our air in Manchester is rich in ammonia; for, instead of 100 gallons, we had only to use 10 gallons; and from the first pint distilled, Mr. Nield and I succeeded in procuring this quantity of ammonia [exhibiting a quantity crystallized in a glass]. It is natural to expect, where so much coal is burned, and where there is so large an accumulation of human beings, that if ammonia is to be found at all, it is in Manchester. We see, therefore, that the atmosphere will provide for plants a quantity of ammonia, and so small as is the quantity entering into the composition of a plant, it is sufficient for the developement of those principles which are requisite to the nutrition of the plant. If one pound of rain water contain only a quarter of a grain of ammonia, then a field, having an area of 40,000 square feet, must receive annually upwards of 80lb of ammonia, or 65lb of nitrogen. This is much more nitrogen than is contained, in the form of *albumen* or *gluten*, in 2,650lb of wood, in 2,800lb of hay, or in 200 cwt. of beet-root, which are the usual products of that surface. It also happens, that the quantity of ammonia thus brought down by rain, after a drought, is larger than ordinary. In summer, a thunder shower after drought is very likely to bring down, in the first part of the shower, a large proportion of ammonia. Liebig took different portions or strata of snow, and found the larger proportion of ammonia in the lowest stratum, which of course fell first. The sensation of greater hardness in rain water (felt on washing the hands) than in distilled water, is owing to the quantity of ammonia which rain water contains as compared with distilled water. We have next to consider how this ammonia appears in plants, or whether it appears at all. Of this there is abundant evidence presented in the evaporation of the juice, taken from the stem of the maple tree, which is generally saturated with lime, for the purpose of throwing down the gluten it contains, and the presence of the lime causes a disengagement of the gaseous ammonia sensible to all about. In the manufactoriesof maple and beet-root sugar, this escape of ammonia is very strikingly experienced; indeed this circumstance is one of very serious loss to the beet-root sugar manu-

facturer; for the ammonia given off, leaves behind it an acid salt which prevents the sugar crystallizing, and causes considerable loss by reducing the sugar to a treacly state, in which only a portion, instead of the whole, can crystallize. The products of the distillation of flowers, herbs, and roots, with water, and all the extracts of plants, for medicinal purposes, contain ammonia. The tobacco leaf contains ammoniacal juice; the juice of the cut vine also gives off ammonia. In connection with these facts, we may allude to some articles which the farmer employs to increase the fertility of hisland. Most of the composts used are rich in nitrogen, particularly bone-dust, crushed bones, and the shavings of horn, being parts of dead animals; but there are also other substances, which, when thrown over the field, increase its fertility, simply because they combine with the ammonia which comes down in rain water, and deprive it of its ammonia. Gypsum, or the sulphate of lime, is extensively used, and, when applied to a meadow exposed to alternations of wet and dry weather, causes it to produce abundantly; but it is found not to answer upon a dry meadow; and the reason is, that when rain falls, if carbonate of ammonia exists in the air, and comes into contact with gypsum, it is converted into carbonate of lime, and the ammonia is disengaged and absorbed into the soil. The next shower dissolves a portion of it, which passes down to the root of the plant, and is assimilated by the plant itself. Many soils contain ammonia. If you take a piece of common pipe-clay, and moisten with a strong alkali, you perceive at once a smell of ammonia given off, which will even continue for a couple of days. Other aluminous earths retain ammonia. Burned clay is often used by farmers to their land; and burned clay is now found to absorb and retain ammonia. The ferruginous earths (those which contain an oxide of iron) also retain ammonia; and one of the most solid of these oxides, the *hematite* or red oxide of iron, a stone, contains one per cent of this gaseous principle. It is from these facts, now for the first time explained by Liebig,—that these materials used by the farmers prove beneficial in their application to the land. *Humus* is a spongy body, which absorbs ammoniacal gas to a considerable extent; and, with every shower of rain, it gives it out to be taken up by the roots of the plants. Plants, then, derive their nourishment from carbonic acid, ammonia, and water; these being the principal sources from which plants derive the greatest part of their bulk and weight. The plant assimilates to itself these respective elements from carbonic acid, ammonia, and water. In the decay of plants, these elements float to other plants, are again assimilated by them, and thus the destruction of one generation of plants furnishes the materials out of which another is to be formed. The intermediate processes are obscure; but still lights are to be thrown upon it, by analogy, which may form the subject of

another communication.—(Applause.) Let us now consider what other materials we find in plants. If a plant consisted merely of the elements mentioned, it would vaporize, be entirely dissipated, and nothing would be left; but very few plants will do this. Some of the elements of plants will do it, as starch, sugar, and the essential oils; but generally we find, that, after the burning away of a plant, there is an ash left. In coals, which are but vegetables transformed, we find the value of the coal depends upon the greater or less quantity of ashes left; and this ash we will term the inorganic principle of vegetables. This consists of potash, soda, lime, magnesia, some of the metallic oxides, phosphoric, sulphuric, and other acids, chlorine, iodine, and I need hardly enumerate them all; but they are a number of fixed elements, capable of existing in a solid form, unalterable and unchangeable by heat. [He exhibited a quantity of ashes obtained from burning six ears of corn, after threshing, and without the wheat; in addition to a quantity of soluble matter, which had been dissolved.] In reading the works of the older physiologists, we meet with the statement, "Such a plant contains a little soda and a little magnesia," without specifying proportions, or any stress being laid upon the fact. But more accurate investigations have shown, that there are certain relative proportions between the quantity of these fixed materials and the plant itself; that, in proportion as these exist in the soil or the plant, its developement is more or less perfect. If a farmer attempt to grow wheat on a soil containing neither flint nor potash, he may get wheat, but it will not stand, the stems will not support it; because the stalk of wheat contains a species of glass—silex in combination with potass. He would have nothing in the ear, unless the soil were provided with a salt called the phosphate of magnesia and ammonia. This has been tried in the mosses, which contain *humus* enough, but without the addition of some compost they never bear. In the tribe of plants called *equisetaceæ*, the stem contains large quantities of *silica*. The Dutch rush contains so much that it is used for the purpose of polishing; and it is found in large quantities in some tropical plants. The bamboo in some of the joints contains absolute nodules of a substance consisting of 70 per cent of silica, and 30 per cent of potash. How came they there? There has been some controversy upon this subject among chemists; and one analysis has been made to show, that it could not be a permanent proportion, inasmuch as two pine trees which grew in different situations contained different proportions. Liebig took these analyses, compared them together, and then introduced (triumphantly I think) the principle of equivalents, laid down by our venerable townsman, Dr. Dalton,—showing that although the quantities appear very dissimilar, yet in their proportions they are exactly the same, although the data were taken from a hostile source. Two pine trees were

taken, one growing on Mont Brevon, and the other on Mont La Salle. The first contained potash, lime, and magnesia ; and the sum of the carbonates of these amounted to 56·71 per cent in the ashes ; while those on the pine grown on Mont La Salle contained 58·55 of these carbonates ; but this latter contained only potash and lime. In the first the quantities were carbonate of potash 3·60, carbonate of lime 46·34, carbonate of magnesia 6·77 ; in the second, carbonate of potash 7·36, carbonate of lime 51·19. So that the one containing no magnesia contained more lime and potash ; and when we examine the equivalents (the proportions required to neutralize an acid), we find them in the one to amount to 9·01, in the other to 8·95, being a difference of only ·06 (six hundredths), a difference of weight which few scales would detect. Liebig took another analysis in which the disproportions are much greater. He took two fir trees,—one growing in Norway, and the other at Allevard, in France. That in France contained potash, soda, lime, and magnesia ; the sum of these carbonates was 49·5 : while that of the carbonates found in the fir grown in Norway was 51·45 ; yet these, when reduced to the equivalents in which they combine with acids, were found to be 11.62 and 11.47 ; from which result Liebig is disposed to infer, that the presence of these elements is not accidental ; but that they form a certain proportion, and enter into that proportion in the ratio of their equivalents. Now from what sources are these matters derived ? Take a sandy heath, which contains, to all appearance, nothing but sand, and where the most expert analyst will detect nothing else ; no *humus* whatever, and, if you attempt to grow wheat upon this heath, there will be no crop. But, if on this heath plants are grown which require but little of the inorganic principle, and these are destroyed either by decay or combustion, it is found in practice that the heath acquires fertility. Take, for example, the Luneburg Heath, in Germany, which is covered with heath-plants generally, especially the *erica vulgaris*. Every 30 or 40 years the practice is to burn down all the vegetable growth on the surface, to let the ashes sink into the ground, and then to sow wheat ; and thus these plants, which have ,for the space of 40 years, been constantly collecting a little of these elements, when burned, contain in their ashes the product of that number of years' growth ; which, when returned into the soil, is sufficient for one crop of plants which require a good large proportion of these elements. In the neighbourhood of Heidelberg, one of the perquisites of the woodcutters, after felling and clearing timber, is to be allowed to burn the roots, stumps, twigs, and leaves, and to raise one year's produce from the ground. They do so, and get one good crop ; for, whatever the trees and plants collect goes on accumulating, and then, by destroying the carbonaceous parts by fire, the

inorganic products are returned to the soil, and provide for one year's growth of wheat. Does not this show us what is meant by exhausting a soil? If wheat is grown year after year on the same soil, it is found that the crop becomes less and less productive. And why? •Because, with every crop of wheat, so much of the inorganic elements is removed or taken away from the ground; therefore, in order to allow this ground to recover these inorganic elements, it is necessary to resort either to the now exploded (?) system of allowing it to remain fallow, or to a rotation of crops; which, we shall show, will furnish the ground with potash and lime. But how does a soil happen to contain potash originally? All soils are formed from the disintegration of the harder rocks; they gradually accumulate in the lower parts of the country, being brought down by floods and other causes. We must look, then, to the composition of the rocks themselves. Suppose of each of these which I will enumerate, a field of 40,000 square feet (a Hessian acre) of surface, with a depth of twenty inches, were decomposed, the quantity of potash we might expect to find would be:—In felspar, 1,152,000lb; in clinkstone, 200,000lb to 400,000lb; in basalt, 47,500lb to 75,000lb; in clay slate, 100,000lb to 200,000lb; and in loam, from 87,000lb to 500,000lb. We also find, that the aluminous (or clay) earths contain a large proportion of potash; for we can obtain it not only from pure felspar, but from the granites. The potash these contain is not washed away by every shower of rain; for clay is a very impervious sort of material; and therefore, though the surface may be washed away, the interior still contains a large proportion. Wherever water penetrates, there the soil gives off its potash to the water, and this will be taken up by the *spongeoles* of the roots of plants. A single cubic foot of felspar, if decomposed in clay, is sufficient to supply a wood of 40,000 square feet, with the quantity of potash necessary for the growth of timber upon it, for five years. Do we not, then, see the use of many of the composts now introduced upon land? The very dirt collected upon our roads must contain quantities of potash. The gritty portions of this dirt undergo decomposition; it is reduced to the finest powder, and, from exposure to air and moisture, undergoes disintegration; and by this means an abundant supply of potash may be obtained. How does the earth, after being exhausted by the growth of plants, recover itself by lying fallow? It has the double advantage of not only having its *humus* exposed to the air, and converted into a sort of sponge; but the inorganic materials are still further decomposed by the action of the air, and thus become ready to furnish to the next crop the quantity of alkali required. We see that where silica is required as an ingredient in portions of a plant. it is necessary that potash should exist with it, to render it in some degree soluble. Thus the hard part of the

bamboo, and the stalk of wheat, which contain silica, have it accompanied with potash, which assists the silica to enter into a state capable of being assimilated to the plant. There is another beautiful provision in plants, for taking up some of the more insoluble elements, viz. that in one class of plants, the *gramineæ*, an acid is given, as an excretion from the roots, producing an acetic fluid, which, entering into combination with alkaline earth, forms soluble elements, and allows them to be taken up by the spongeoles, and reduces them to a state of assimilation. Plants growing there will assist in the disintegration of rocks more than mere weather will do. But suppose a soil pretty rich in potash, and that the farmer is misled by the desire of making the most of his land in a short time. He grows upon that land plants, which, when they attain their proper growth, are removed from the land, burned, and sold for potash—an article greatly in demand. The plant generally selected for this purpose is the *artemisia*, or wormwood ; if he grows it and sells the ashes, and next year thinks he will have a crop of wheat, he will be disappointed ; for he has taken from the soil that very material which is essential to the welfare of his wheat the following year. Again, it is essential to the farmer to know which of his crops take out the most of this principle, as contained in potash, lime, or magnesia. Tobacco and wheat require pretty much the same proportion of potash, at one period of their growth. If the farmer attempts to cultivate the one after the other, the result will be pretty much the same as if he tried to grow two crops of the same plant in two succeeding years. It would seem from this, if the view taken by Liebig be correct, that it is important the farmer should always know what are the inorganic elements contained in the crops he wishes to have ; for these must either be found in, or supplied to, the soil ; they cannot exist in the atmosphere. He ought next to examine his soil, and see if it contains them. If not, they must be added. Upon this, it seems to me, the important principles of agriculture depend. It is not necessary that each farmer should be himself an analyst ; but a number of farmers might unite to procure the services of one ; and it is certain that success would follow the application of these principles. The quantity of inorganic elements required by the plant, in 100 parts of the stalk of wheat, are 15.5 of ashes. In the same quantity of the dry stalks of barley, 8.54 ; in 100 parts of the stalk oats, 4.42 ; and thus we see, that the same field that yields only one harvest of wheat, might be made to produce two crops of barley, or three of oats, year after year. The illustration of the practice at Heidelberg, of allowing the woodcutters to burn the timber on the ground, will also show how land, which has been covered by forests for years, will, when the forest is consumed, become abundantly fertile. It is owing to this, that the trees themselves require but little alkali in proportion to the grasses ;

they have been assimilating this for years, and, when they are destroyed by decay or combustion, that ground is abundantly supplied with inorganic products necessary for a crop, and also with a fair proportion of vegetable soil. The common practice with farmers in the rotation of crops is to follow grass with *leguminosæ*, which class of plants contains no free alkali, and only one per cent of the phosphates of lime and magnesia ; buck wheat contains only .09 per cent. These belong to the fallow crops; and the cause they do not exercise any injurious influence on the land cultivated, is, that they do not extract the alkalies from the soil, and only a very small proportion of the phosphates.

Now, is there not something in all this deserving the attention of agticulturists ? They have hitherto gone on blindly ; they have arrived at a certain state of knowledge from experience, and it is so far a useful guide ; but are they possessed of sufficient knowledge of facts connected with this important subject, to form a principle and rule, in reference to crops and soils of various kinds, and the order of succession ? And when we find a work published by a celebrated organic chemist, Dr. Liebeg, in which these principles are developed for the first time in Europe, are they not, I ask, highly deserving the attention of agriculturists ? They not only apply to all the articles of our daily food, but to the articles of raw produce which we consume in our manufactures ; and, in fact, it seems that culture is not only useful, as supplying our animal wants, but also to lay the foundation of the prosperity of states, particularly of those engaged in commercial enterprise ; and therefore, in the speculations now afloat as to the transfer of the products of one part of the earth to another, it is important to set at rest or establish the principle upon which this must be effected. I do not stand here, in order to defend the principles of Liebig, but shall be glad to listen to the experience which any gentleman here has to offer ; assuring you, that I have at least as much pleasure in listening to others as in hearing myself.—The lecturer concluded his communication (which was delivered extemporaneously with great ease and fluency) amidst the loud applause of his deeply-interested auditory.

The Chairman* said, it was very gratifying to hear the suggestion of Mr. Ransome as to the practical application of chemistry to agriculture ; for, though a farmer could scarcely be expected to become an analytic chemist, a number of farmers might congregate to talk matters over, and employ a person competent to try experiments, and in this way the important principles developed in Liebig's work would become of immense practical importance to generations yet unborn. Perhaps we might not immediately see the application of these apparently dry details to the subject of animal and vegetable

* Alfred Binyon, Esq.

nutrition; but we should doubtless see afterwards the connec-
tion between the principles now developed, and the food we
take into our stomachs.

Mr. Ransome said, the plan he proposed to adopt was this:
—To follow out the elements which constitute our food, and
see whence they are derived as vegetable substances; and then
to apply these principles to the subject of animal nutrition; and
this appeared to him the only perfect way,—first, to treat of the
materials of which they were made; next, what forms these
were capable of assuming; then what forms they did really
assume; and then what articles are more or less fit, and why,
for the purpose of animal nutrition.

The Chairman: Has Mr. Ransome verified the statement of
Liebeg as to the development of oxygen from plants during the
action of the sun's light?

Mr. Ransome: I have several years ago, quite to my own
satisfaction. This is a very unfavourable season of the year to
repeat the experiment; but I shall repeat it as soon as the
season will permit.

The Chairman said, it was desirable that the fact should be
established. He was quite delighted to see the quantity of
ammonia on the table obtained from rain water. It was an
extremely interesting and novel experiment.

Mr. Ransome said, that one subject would require verifica-
tion—the doctrine of vegetable equivalents, or whether certain
plants of the same class, and under similar circumstances, con-
tain equal qualities of alkaline or other bases. He had only
mentioned four facts bearing on this point, all taken from
Liebeg's book; but it was a subject requiring verification, and
it would form an interesting inquiry for any chemist practised
inorganic chemistry. It would not require much skill; the
experiments were very easy. At their last meeting the subject
of the *epiphites* was mentioned,—a subdivision of the parasitic
tribes of plants; and, in answer to a question he (Mr. Ran-
some) put, Mr. Moore had given an interesting statement as to
some plants which grow attached to other plants, but not
growing from them. He had since thrown out a valuable hint,
—that, as *epiphites* grow upon the surface of other plants. the
question was, is there an exudation from the parent plants,
which is taken up after decomposition by the air, by these
radicals, although they do not live upon the juices of the parent
plant?

Mr. Moore said he thought there could be no question that
secretions were thrown out upon particular parts of plants; but
in this point he had merely referred to those rough-coated
plants, which the *epiphites* liked to select to fasten upon, and
which he produced the other evening at the Philosophical

Society. There was one plant, the *epidendron elongatum,* which seemed to have no radicals whatever, nothing to take up even the *humus* from the tree upon which it fixed itself. The lichens might be considered *epiphites,* for they fixed themselves upon trees, they did not get upon the *alburnum;* but, having sponges, they very likely received their food from the *humus* secreted by the plant to which they were attached; but the *epidendron elongatum* would grow suspended from a wall, provided the atmosphere were kept moist. All that is wanted to be known, and this would seem to prove it, is, that there is a tribe of plants that may and do receive all their juices independently of any connection with the ground; and that they elaborate their food by means chiefly of the leaves rather than the radicals. The parasites that get into the *alburnum* would prove nothing, because they draw their nourishment from the parent plant, which gets it from the ground. The most important part of Liebig's book seemed to him to be this,—that the theory which he endeavoured to establish, corroborated all those facts which the farmers had found to be true from experience. He (Mr. Moore) could point out fields in the neighbourhood in which he was born, where the farmer, from improper motives, had taken two wheat crops in succession, without the land resting, and that land would not recover for years. In the rotation of crops, they never took two of wheat in succession. The common order of rotation was,—potatoes, with additional rich manure ; then wheat, then oats, next clover ; and then a good farmer would let his land rest before he began with crops again. Mr. Ransome mentioned a curious fact in connection with lichens ; in many of which, instead of woody fibre, was found a chemical salt, the double oxalate of lime. • In several plants, crystals of chemical compounds were found existing in their tissues. A plant had been found (the *chara hispida*) covered over with crystals of the carbonate of lime. In the bamboo it existed as a sort of nodule, and Mr. Bowman doubtless knew other cases.—Mr. BOWMAN : Yes.

Mr. RANSOME then exhibited an interesting experiment,— the solidification of ammonia in connection with a metal ; forming with mercury an amalgam, as tin or lead would do, and considerably and visibly increasing the bulk of the mercury. Into a solution of the salt of ammonia he poured some mercury, which lost much of its original fluidity, and attained a consistency something like that of soft butter. He then stated, that, as he wished for an opportunity of meeting a number of agricultural gentlemen, he should be glad to give a supplemental lecture between this and the next lecture for a general audience ; and as the subject of the art of culture involved some details which could hardly be discussed in public, on that occasion only his audience would be exclusively composed of gentlemen.

Mr. J. E. Bowman moved the thanks of the meeting to Mr. Ransome for his very lucid exposition of the important principles which had formed the subject of his lecture.

The Chairman then called the attention of the company to the present position of the establishment, which he was sure they must be convinced was one of very great utility to various classes of the community ; more especially to the parents of large families growing up and wishing to become acquainted with the elementary parts of science, to whom this gallery would afford opportunities, not perhaps equalled by any other institution of a similar kind in the town. His brother directors and himself earnestly requested those present to make it known amongst their friends.

Mr. Wm. Boulton, as a visitor, and by the committee privileged to be present, could not be satisfied without seconding the vote of thanks to Mr. Ransome. The subject was of immense importance ; and he trusted, that, through the kindness of Mr. Ransome, the application of the principles of chemistry to the cultivation of the land would be productive of additional benefit to the community, by diffusing information as to the principles enunciated by Professor Liebig.

Experiments on the Lateral Force of Electrical Explosions By Joseph Priestley, LL.D., F.R.S.*

Dr. P. being informed, in accounts of damages done by lightning, of persons and things being removed to considerable distances, without receiving any hurt, he was excited to try whether he could produce similar effects by electricity. All the other known effects of lightning had been frequently imitated by the application of this power ; but he did not know that this effect had ever been taken notice of by any electrician. The experiments he presently found to be very easy; and he thinks it not difficult to ascertain the cause of this striking effect, and the manner in which it is produced.

If pieces of cork, wood, powder of any kind, or any light bodies whatever, be placed near the explosion of a jar, or battery, they will not fail to be moved out of their places, at the instant of the discharge. If the explosion of a large battery be made to pass over the surface of animal or vegetable substances, in the manner described in the printed account of his experiments, and large corks be strewed along, or near the path intended for it, it is surprising to observe with what violence they will be

* From the Transactions of the Royal Society for the year 1769.

driven about the room ; and this dispersion is in all directions from the centre of the explosion ; and it makes no difference whether the rods, between which it is made, be sharp-pointed or otherwise. The effect of this lateral force is very remark-àble in attempts to fire gunpowder in electrical explosions. If the gunpowder be confined ever so close in quills or cartridges, and they be held fast in vices, yet, when the explosion is made in the centre of them, it will sometimes happen, even when a wire has been melted in the midst of the powder, and the frag-ments have been seen red-hot for some time in different parts of the room, that the powder has not been fired, or only a few grains of it, the rest being dispersed with great violence, part of it flying against the faces of persons who assisted in making the experiments. This circumstance, together with the charcoal being a conductor of electricity, makes it so extremely difficult to fire gunpowder by electrical explosions ; and it is evidently owing to this lateral force, that parts of the melted wire fly so many ways, and to so great a distance from the place of ex-plosions

This lateral force is exerted not only in the neighbourhood of an explosion, when it is made between pieces of metal in the open air, but also when it is transmitted through wires that are not thick enough to conduct it perfectly ; and the smaller the wire, and the more complete the fusion, the greater is the dis-persion of light bodies placed near it. At one time, when the wire was not melted, but turned blue by the explosion (in which case it generally assumes a dusky red, which lasts but for a moment), there was a small dispersion from every part of the wire, but by no means so great as it would have been if it had been melted, or only heated to a greater degree. By a consi-derable number of trials Dr. P. found, that a greater force of explosion would move light bodies at a greater distance ; but the smaller the bodies were, the less was this difference ; so that he supposed, that if they had no weight at all, they would probably be moved at the same distance by the explosion from any quantity of coated surface, charged equally high ; but there was a great difference in the weights removed by different forces at the same distance. Placing the same piece of cork at the same distance from the place of explosion, he found that the discharge of one jar removed it $\frac{1}{4}$th of an inch, two jars 1$\frac{1}{4}$th, three 1$\frac{3}{4}$ths, and four about 2 inches ; so that he does not wonder at very heavy bodies being moved from their places, and to considerable distances, by strong flashes of lightning.

That the immediate cause of this dispersion of bodies in the neighbourhood of electrical explosions, is not their being sud-denly charged with a quantity of electric matter, and therefore flying from others that are equally charged with it, is, he thinks, evident from the following experiments and observa-

tions. He never observed the least sensible attraction of these light bodies to the brass rods, through which the explosion passed, or to the electric matter passing between them, previous to this repulsion, though he used several methods which could not have failed to show it, if there had been any such thing. Sometimes he suspended them in fine silken strings, and observed that they had contracted no electricity after they had been agitated in the manner described above. Sometimes he dipped them in turpentine, and observed that no part of it was found sticking either to the brass rods themselves, or to any part of the table between them and the place where the light bodies had been laid. He even found that the explosion of a battery made ever so near to a brass rod did not so much as disturb the equilibrium of the electric fluid in the body itself: for when he had insulated the rod, and hung a pair of pith balls to the end opposite to that near which the explosion passed, he found that the balls were not in the least moved at the time of explosion, which they would have been, if part of the electric fluid, natural to the body, had been driven, though but for a moment, towards the opposite end. He also observed, that the effect was the same, when the explosion was made to pass through one of the knobs of the insulated rod. This lateral force was evident through thin substances of various kinds interposed between the explosion and the bodies removed by it, as paper, tin-foil, and even glass ; for when some grains of gunpowder were put into a thin phial, close stopped, and held near the explosion of a battery, they were thrown into manifest agitation.

Dr. P. therefore thinks it most probable, that this lateral force is produced by the expulsion of the air from the place where the explosion is made. For the electric matter makes a vacuum of air in its passage ; and this air, being displaced suddenly, gives a concussion to all the bodies that happen to be near it. Hence the removal of the light bodies, and the agitation communicated to the thin substances, and to the air, and the light bodies placed beyond them. The only objection to this hypothesis is, that this lateral force is not so much less in vacuo as might be expected, when the air is supposed to receive the concussion first, and to communicate it to other bodies ; but it must be considered, that the most perfect vacuum we can make with a pump is not free from air. Dr. P. tried to make this experiment in a Torricellian vacuum, but could not succeed at that time. Besides, as the electric matter, of which an explosion consists, must take a wider path in vacuo, if not equally fill the whole space, it may affect a body in its passage, without the intervention of any air. In condensed air, this lateral force was not, as far as he could perceive, much increased.

Willing to feel what kind of an impulse it was that acted on

bodies, when they were driven away by this lateral force of electricity ; Dr. P. held his finger near the path of an explosion of the battery, passing over the surface of a green leaf, when he felt a stroke, as of something pushing against his finger. Several corks, placed in the same situation, were driven to a considerable distance by the same explosion. Recollecting that this power, which he now, calls the lateral force of electrical explosions, must be the same with that which gives the concussion to water, mentioned in his experiments to imitate an earthquake, and to vegetable and animal substances, over the surface of which it passes ; and being determined to make a more satisfactory trial of it than he had ventured to do before, he laid a green leaf on the palm of his hand, intending to make the explosion pass over the leaf; but the leaf was burst, and torn to pieces, and the explosion, passing over his hand, gave it a violent jar, the effect of which remained, in a kind of tingling, for some time.

Lastly, in order to judge the most perfectly of his force, Dr. P. laid a chain communicating with the outside of the battery on his bare arm, above the wrist, and bringing the discharging rod near the flesh, within about two inches and a half of the chain, he made the explosion pass over the quantity of the surface of the skin. Had he taken a greater distance, he was aware that the explosion would have entered the flesh ; which, he was sensible, would have given a painful convulsion to the muscles through which it passed. In this case the sensible effect was very different from that, being the same external concussion as before ; and he sometimes thought, that the sensation is not disagreeable. However, the hairs on the skin were singed, and curled up along the whole path of the explosion, and for the space of about half an inch on each side of it: also the papillæ pyramidales of the skin were raised, as when a person is shivering with cold. This was also the case in every part of the arm which the chain touched, and even that part of it which was not in the circuit. Both the path of the explosion, and the place on which the chain lay, had a redness which remained till the next day. Sometimes the flesh has contracted a blackness by this experiment, which has remained for a few hours.

Various Experiments on the Force of Electrical Explosions.
By Joseph Priestley, LL.D., F.R.S.[*]

Making the explosion of a battery pass over the surface of a green cabbage-leaf, he observed that it left a track near ¼th of an inch in breadth, exceedingly well defined, and distinguished by a difference of colour from the rest of the leaf. Along this path also the firmness of texture in the leaf was entirely destroyed, that part becoming quite flexible, like a piece of cloth. Presently after, it turned yellow, withered, and became perfectly brittle.

Willing to try the effect of this explosion passing along the surface of other substances, he laid a piece of common window-glass on the path, pressed by a weight of six ounces ; but it was shattered to pieces, and totally dispersed, together with the leaf on which it lay. Placing the blackside of a piece of corkwood upon it, pressed by a weight of half a pound, the leaf was not rent, but the cork was furrowed all the way, a trench being made in it about half an inch in breadth, and a quarter of an inch in depth. Laying the smooth cut surface of a piece of cork, it was furrowed all the way, as if it had been cut with a file, but not near so deep as before. Many of the small pieces, which had been rubbed off in the explosion, remained in the furrow. Also the substance of the cork seemed to be shattered, and it was easily rubbed off, a little way into it.

He made this explosion on the surface of some red wine, in a small dish, and kept a part of the same quantity exposed in a similar manner : but he could perceive no difference between them after several days.

The track of an electrical explosion on the surface of the cabbage-leaf, being so well defined, suggested an experiment to ascertain whether there was any sensible momentum in the electric fluid, when it is rushing with violence from one side of a battery to the other. For this purpose he made the explosion pass over the leaves when they were cut in right and acute angles ; so that the shortest path, from the inside to the outside of the battery, was to turn close at the angle ; and observed that it was not diverted from its course, in the least degree, by the rapidity of its own motion, but that it had turned exactly at the angle, and kept as close to the opposite side, as if the motion had begun at the angle. The electric matter had however been evidently attracted by the veins of the cabbage-leaf, having pursued them a little way, at least having sensibly affected them, wherever it met with them in its passage.

This experiment suggested another, intended to determine whether the force of an explosion was at all diminished by being

[*] From the Philosophical Transactions for 1769.

diverted from a right-lined course, and made to turn in a great
number of angles.   To do this, he first found, by a great num-
ber of trials, what length of a small iron wire he was able to
melt with a battery of about twenty square feet, in the middle
of a circuit of about three yards of brass wire, considerably
thicker than the iron, and stretched in two right lines, suspen-
ded on silken strings. The length of the iron wire, melted in
these circumstances, was about three inches.   He then took the
same brass wire, and fixing pins into a board of baked wood,
twisted it about them, making it turn in a very great number
of acute angles ; and he put three inches of the same iron wire
in the middle of this crooked circuit, that he had done in the
straight one ; so that the electric matter in the explosion was
obliged to make a great number of turns at acute angles, before
it could come to the iron wire ; but he always found that the
same length of iron wire was melted in these circumstances as
in the other, and not the least difference was perceived in the
force.   But though the form of the wire through which an ex-
plosion passed, made no difference in its force, he found a very
remarkable difference occasioned by the length of the circuit in
wires of the same thickness ; and which surprised him very
much.

To ascertain the practicability of firing mines by electrical
explosions, Dr. P. took twenty-two yards of small brass wire
(but so thick, however, that he could not have melted the least
part of it by the force of any battery he had ever constructed),
and extending it along a dry boarded floor, with a small piece
of iron wire, and a cartridge of gunpowder about it, in the
place that was most remote from the battery : he found that, on
the discharge, the wire was not melted, nor the gunpowder ex-
ploded ; also the report was very faint.   In other circumstances,
a charge of the same battery was able to melt more than nine
inches of this iron wire ; and this same cartridge was easily
fired near the battery, connected with shorter pieces of the
same brass wire ; so that the diminution of force must have
been owing to the length of the circuit.

In the place of this small brass wire, Dr. P. substituted an
iron wire one-fifth of an inch thick, when about half an inch
of the small iron wire was exploded ; so that the force was
not lessened so much, in a circuit of the thick iron wire, as it
had been in one of the small brass wires.   To judge how much
of the force might be lost by nearer circuits, consisting of less
perfect conductors, he joined the middle of the circuit made
by the iron wire with water, in which both the wires were im-
mersed.   The effect was, that the small iron wire was only
made red-hot, but not exploded as before.   Being sensible how
much depended on avoiding lesser circuits, by which part of
the fire of an explosion might return to the battery, without

reaching the extremity of the circuit, where he intended the whole of its force to be exerted, in the remaining experiments, he insulated half the circuit of iron wire. There was no occasion for insulating the whole circuit; for if there was but one passage to, or from the middle of it, there could be but one from, or to it. In this method it was easy to ascertain what loss of force was occasioned by the length of the circuit, as every other circumstance was carefully excluded; and it presently appeared to be very considerable; for though it could melt nine inches of the small iron wire at the distance of fifteen yards from the battery; when he tried twenty yards, he found that he was just able to make six inches of it red-hot. The battery in these experiments was in the house, and the wires of which the circuit consisted were conveyed by silken strings into a garden adjoining to the house.

Mentioning this loss of force occasioned by the length of the circuit in electrical explosions to Dr. Franklin, he said that the same observations had occurred to him, and that he had also been disappointed in an attempt to fire gunpowder at a distance from his battery. Struck with this appearance, Dr. P. endeavoured to ascertain the quantity of this obstruction, by trying what other courses the electric fire would chuse preferably to a long metallic circuit. In the first place, taking about a yard of the small brass wire, mentioned above, he disposed it in the manner described below, connecting one of the ends with the outside of the battery, and the other with the inside. In the first place, he brought the parts near the two extremities into contact, and, on the discharge, found there had been a fusion in that place, and that a great part of the fire had taken the shorter circuit, though it had been obliged to quit the wire in one place, and enter it again in another. Afterwards he removed these parts to a small distance from one another, and, on the explosion, observed a strong spark pass between them. Removing them to greater and greater distances, he found the explosion to pass above one-third of an inch in the air, rather than make the circuit of the continued wire. Using a longer and smaller iron wire, the passage through the air exceeded half an inch. He then took four or five yards of iron wire one-tenth of an inch thick, when the passage through the air was still half an inch; and taking three yards and a half of wire that was one-fifth of an inch thick, the spark in the air was half an inch, and sometimes near three quarters of an inch. Making use of only half the length of this wire, the passage through the air was only half that distance, or one-fourth of an inch. When he kept the place of near contact about the middle of this wire, and made the explosion at the extremities of the whole wire, he was obliged to bring them about as near again, i. e. to little more than the eighth of an inch, before the passage would be

through the air; so that the force of the whole explosion must have been greatly weakened by its passing through so much of the wire. Lastly, he took a pair of kitchen tongs, the legs of which were two feet, and the smallest part of them above half an inch in diameter; when the circuit was made about one-sixth of an inch in the air (for at that distance from one another the ends of the tongs had been fixed) rather than through four feet of that thick iron.

Notwithstanding this evident passage of the electric matter through the air, at the same time that a metallic circuit was provided for it; it was certain that the whole of the charge did not pass in the air: for when he extended one-third of an inch of small iron wire between the nearest parts, it was only made red-hot by the discharge; whereas above two inches of it would have been exploded, if there had been no other metallic circuit at all.

As the electric fire meets with so much obstruction in passing through a circuit of iron of this thickness, Dr. P. makes no doubt but that it is considerably obstructed in passing through metallic circuits of any thickness whatever; and that it would prefer a very short passage through the air, if they were made even of no great length. In this method the different degrees of conducting power in different metals may be tried, using metallic circuits of the same length and thickness, and observing the difference of the passage through the air in each. N.B. A common jar answers as well, in these experiments, as a large battery. It is evident, from many experiments, that the whole fire of an explosion does not pass in the shortest and best circuit; but that, if inferior circuits be open, part will pass in them at the same time. Of this Dr. P. made the following satisfactory trial. He took an iron chain, and laid it upon a table, in contact with a charged jar; so that the parts of it made two circuits for the discharge, which he could vary at pleasure; and he observed that, when one of the circuits was but half an inch, and the other more than half a yard; yet, if the charge was high, it always went in them both, there being considerable flashes between the links of the remotest part of the chain. If the charge was weak, it passed in the shortest circuit only.

It is evident, that when the wires of a battery are not in close contact, there must be some loss of force in the discharge; but this never appeared to be very considerable. To ascertain it by experiment, he first found, by repeated trials, what length of a piece of iron wire he was able to melt with a battery consisting of twenty jars, with the wires and connecting rods quite loose, and a chain to join the rods belonging to each row of jars, which is the manner in which he had generally constructed them. In these circumstances, he found the battery was able

to melt something more than two inches and a half of the wire. He then soldered the wires of each jar to the rod which connected them, and also soldered another rod to all these, instead of the chain which he had used ; so that he avoided near one hundred sparks in the discharge, at each of which there must have been some loss of force ; but he did not find, after many trials, that the strength of the battery had been thus sensibly diminished : for he could not melt three inches of the same piece of wire in these circumstances. It was only made red-hot, which is equivalent to the melting and exploding of little more than two inches and a half.

Observations on Atmospheric Electricity. By M. PELTIER.[*]

The instruments which serve to measure electricity do only indicate the electric difference of bodies to which they are applied, and not the *absolute quantity* which either of them contains. This defect is a grave inconvenience for meterological observations, since the instruments may be plunged into a strong electric atmosphere without giving any indication. This defect is common to two kinds of electric measurers, but the multiplier has a very great inferiority in point of sensibility, as I have shown in a memoir published in 67th volume of the *Annals d Chimie et de Physique;* I have proved that, with the best multiplier, it is necessary to discharge 7069 degrees of statical electricity through its wires to produce one degree of dynamic divation in the needle. These two defects of the multipler, (indication of electric difference of bodies, and want of sensibility), render that apparatus insufficient for studying atmospheric electricity under a serene sky, at low altitudes ; for instance, making the ground for one of the extremities of the conductor, and the height of buildings for the other. Moreover, for exploring the atmosphere, the multiplier, hitherto, has only been employed for indicating the electricity of stormy clouds.

For the purpose of resuming this question, and interrogating the atmosphere at great altitudes, I have taken advantage of the serenity of the weather, which has continued for a long time. The experiments were made on an elevated plane three leagues from Corbeil, on the grounds of M. Ant. Breguet, where I met with all the attention and assistance that I could desire, both in instruments, skill, and devotion to the science. Professor Guttierez took with us a very active part in these researches.

On the 21st of April last, the sky being sufficiently clear, notwithstanding vapours forming long *cirri*, occasionally com-

bining in long strata, and advancing slowly at a distance, the
air gave feeble electric indications at three meters from the
ground: the wind, in the lower strata, being north west, whilst
that at the height of the clouds was due south.   About noon
we sent up a kite attached to a copper wire of 400 metres long.
The bobbin round which the wire was coiled had a calculater
attached for the purpose of ascertaining the quantity of wire let
off to the kite; and every part of the apparatus was susceptible
of insulation when required. A multiplier of 3000 convolutions
of wire, communicated with the wire on the bobbin, by one of
its extremities, and with the ground by the other: and an elec-
troscope could testify the indications of the multiplier at each
instant.   The angle formed by the kite-string and the ground
was easily ascertained: the former being the hypothenuse.

The kite being arrived at the height of 30 metres, the
multiplier showed but little sign of a current, whilst the elec-
troscope gave indications of an increasing tension of positive
electric action.   From 30 to 40 metres the multiplier deviated
from 2° to 3°, and indicated a positive current *descending.*
Above that height, both the multiplier and the electroscope
indicated a neutral zone.   Afterwards, we had a negative *de-
scending* current, with a deflection of 2° or 3°.   The electros-
cope indicated the negative zone, or strata, to be about the
thickness of 20 metres, above which, we again found the at-
mosphere positive.   The new positive current was feeble at
first, but the kite having mounted to 120 metres the needle
commenced a rapid deflection; and when it had arrived at the
height of 180 metres, the current gave 60° of deflection, cor-
responding to 160 proportional degrees.

This reversal of the current being a fact too curious not to
attract our attention; we again interrogated the atmosphere the
next few following days; but there being an uniform serenity
of weather we could not find any reversal of electric action,
such as we obtained in the first day's experiments; the atmos-
phere being *positive* from the commencement at 2 metres from
the ground.   The tension increased to about 30 or 40 metres;
and it was not till the kite arrived at that height that the elec-
tricity became sufficiently intense to act on the needle of the
multiplier.   From 40 to 100 metres altitude the needle ad-
vanced very feebly, but at the latter elevation the deviation
proceeded rapidly, and when the kite had mounted to 247
metres, the needle became deflected 90° and remained steady
between 70° and 80°, which showed a mean current of 600
proportional degrees.

The constant fact that we have found during those fine dry
days, is, that the positive electricity increased slowly to the
altitude of 100 meters; but above that height it increased

rapidly to the highest altitude that we have attained. The needle is not tranquil in its deflections, but varies considerably according to the changes and force of the wind. It is principally when the kite's head was agitated that the variations of the needle were most considerable: at those times it would frequently deviate over an arc of from 10° to 25° in a moment; corresponding with 30° to 40° of proportionals.* .

An. Attempt to Explain some of the Principal Phenomena of Electricity, by Means of an Elastic Fluid. By the Hon. HENRY CAVENDISH, F.R.S. •

Since first writing the following paper, Mr. C. found that this way of accounting for the phenomena of electricity, is not new. Æpinus, in his Tentamen Theoriæ Electricitatis et Magnetismi, has made use of the same, or nearly the same hypothesis; and the conclusions he draws from it, agree nearly with Mr. C.'s, as far as he goes. However, as Mr. C. has carried the theory much farther than he has done, and has considered the subject in a different, and in a more accurate manner, he hopes the society will not think this paper unworthy their acceptance.

The method he proposes to follow is, first, to lay down the hypothesis; next, to examine by strict mathematical reasoning, or at least, as strict reasoning as the nature of the subject will admit of, what consequences will flow from thence; and lastly, to examine how far these consequences agree with such experiments as have yet been made on this subject. In a future paper, he intends to give the result of some experiments he was making, with intent to examine still further the truth of this hypothesis, and to find out the law of the electric attraction and repulsion.

Hypothesis.—There is a substance, which Mr. C. calls the electric fluid, the particles of which repel each other, and attract the particles of all other matter, with a force inversely as some less power of the distance than the cube: the particles of all other matter also repel each other, and attract those of the electric fluid, with a force varying according to the same power of the distances. Or, to express it more concisely, if we consider the electric fluid as matter of a contrary kind to other matter, the particles of all matter, both those of the electric fluid and of

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* We imagine that our author means *proportional intensity* as indicated by some kind of electrometer.—*Edit.*

other matter, repel particles of the same kind, and attract those of a contrary kind, with a force inversely as some less power of the distance than the cube. For the future, he would be understood never to comprehend the electric fluid under the word matter, but only some other sort of matter.

It is indifferent whether we suppose all sorts of matter to be indued in an equal degree with the foregoing attraction and repulsion, or whether we suppose some sorts to be indued with it in a greater degree than others; but it is likely that the electric fluid is indued with this property in a much greater degree than other matter; for in all probability the weight of the electric fluid in any body bears but a very small proportion to the weight of the matter; but yet the force with which the electric fluid therein attracts any particle of matter, must be equal to the force with which the matter therein repels that particle; otherwise the body would appear electrical, as will be shown hereafter. To explain this hypothesis more fully, suppose that one grain of electric fluid attracts a particle of matter, at a given distance, with as much force as n grains of any matter, lead for instance, repel it: then will one grain of electric fluid repel a particle of electric fluid with as much force as n grains of lead attract it; and one grain of electric fluid will repel one grain of electric fluid with as much force as n grains of lead repel n grains of lead.

All bodies in their natural state, with regard to electricity, contain such a quantity of electric fluid interspersed between their particles, that the attraction of the electric fluid in any small part of the body, on a given particle of matter, shall be equal to the repulsion of the matter in the same small part on the same particle. A body in this state Mr. C. calls saturated with electric fluid; if the body contains more than this quantity of electric fluid, he calls it overcharged: if less, he calls it undercharged. This is the hypothesis; he now proceeds to examine the consequences which will flow from it.

Lemma 1.—Let EAe (pl. 3, fig. 1) represent a cone continued infinitely; let A be the vertex, and Bb and Dd planes parallel to the base; and let the cone be filled with uniform matter, whose particles repel each other with a force inversely as the n power of the distance. If n is greater than 3, the force with which a particle at A is repelled by EBbe, or all that part of the cone beyond Bb, is as $\dfrac{1}{AB^{n}-3}$. For supposing AB to flow, the fluxion of EBbe is proportional to — AB -|- AB^{2}, and the fluxion of its repulsion on A is proportional to $\dfrac{-AB}{AB^{n}-2}$; the fluent of which is $\dfrac{1}{n-3-|-AB^{n}-3}$; which when AB is infinite is equal

to nothing ; consequently the repulsion of ΕΒ*be* is proportional to $\overline{n - 3 +|- AB^n - 3}$ or to $\frac{1}{AB^n - 3}$.

Corol. If AB is infinitely small, $\frac{1}{AB^{n-3}}$ is infinitely great ; therefore the repulsion of that part of the cone, between A and B*b*, on A, is infinitely greater than the repulsion of all that beyond it.

Lemma 2.—By the same method of reasoning it appears, that if *u* is equal to 3, the repulsion of the matter between B*b* and D*d* on a particle at A, is proportional to the logarithm of $\frac{AD}{AB}$; consequently the repulsion of that part is infinitely small in respect of that between A and B*b*, and also infinitely small in respect of that beyond D*d*.

Lemma 3.—In like manner, if *n* is less than 3, the repulsion of the part between A and B*b* on A is proportional to AB^{3-n} : consequently the repulsion of the matter between A and B*b* on A, is infinitely small in respect of that beyond it.

Corol. It is easy to see, from these three lemmata, that if the electric attraction and repulsion had been supposed to be inversely as some higher power of the distance than the cube ; a particle could not have been sensibly affected by the repulsion of any fluid, except what was placed close to it. If the repulsion was inversely as the cube of the distance, a particle could not be sensibly affected by the repulsion of any finite quantity of fluid, except what was close to it. But as the repulsion is supposed to be inversely as some power of the distance less than the cube, a particle may be sensibly affected by the repulsion of a finite quantity of fluid, placed at any finite distance from it.

Definition. If the electric fluid in any body, is by any means confined in such manner that it cannot move from one part of the body to the other, Mr. C. calls it immoveable : if it is able to move readily from one part to another, he calls it moveable.

Prop. 1.—A body overcharged with electric fluid attracts or repels a particle of matter or fluid, and is attracted or repelled by it, with exactly the same force as it would, if the matter in it, together with so much of the fluid as is sufficient to saturate it, was taken away, or as if the body consisted only of the redundant fluid in it. In like manner an undercharged body attracts or repels with the same force, as if it consisted only of the redundant matter ; the electric fluid, together with so much of the matter as is sufficient to saturate it, being taken away.—This is evident from the definition of saturation.

Prop. 2.—Two over or undercharged bodies attract or repel

each other with just the same force that they would, if each
body consisted only of the redundant fluid in it, if overcharged,
or of the redundant matter in it, if undercharged.—For, let the
two bodies be called A and B : by the last proposition, the re-
dundant substance in B impels each particle of fluid and matter
in A, and consequently impels the whole body A, with the same
force that the whole body B impels it : for the same reason the
redundant substance in A impels the redundant substance in B,
with the same force that the whole body A impels it.   It is
shown therefore, that the whole body B impels the whole body
A, with the same force that the redundant substance in B impels
the whole body A, or with which the whole body A impels the
redundant substance in B ; and that the whole body A impels
the redundant substance in B, with the same force that the re-
dundant substance in A impels the redundant substance in B.,
Therefore the whole body B impels the whole body A, with the
same force with which the redundant substance in A impels the
redundant substance in B, or with which the redundant sub-
stance in B impels the redundant substance in A.

Corol. Let the matter in all the rest of space, except in two
given bodies, be saturated with immoveable fluid ; and let the
fluid in those two bodies be also immoveable.   Then, if one of
the bodies is saturated, and the other either over or under-
charged, they will not at all attract or repel each other.   If the
bodies are both overcharged, they will repel each other.   If
they are both undercharged, they will also repel each other.
If one is overcharged and the other undercharged, they will
attract each other.

N. B. In this corollary, when Mr. C. calls a body overchar-
ged, he would be understood to mean, that it is overcharged in
all parts, or at least no where undercharged ; in like manner,
when he calls it undercharged, he means that it is undercharged
in all parts, or at least no where overcharged.

Prop. 3.—If all the bodies in the universe are saturated with
electric fluid, it is plain that no part of the fluid can have any
tendency to move.

Prop. 4.—If the quantity of electric fluid in the universe is
exactly sufficient to saturate the matter therein, but unequally
dispersed, so that some bodies are overcharged and others un-
dercharged ; then, if the electric fluid is not confined, it will
immediately move till all the bodies in the universe are satu-
rated.—For, supposing that any body is overcharged, and the
bodies near it are not, a particle at the surface of that body
will be repelled from it by the redundant fluid within ; con-
sequently some fluid will run out of that body ; but if the body
is undercharged, a particle at its surface will be attracted
towards the body by the redundant matter within, so that some
fluid will run into the body.

N.B. In prob. 4, case 3, there will be shown an exception to this proposition: there may perhaps be some other exceptions to it: but he thinks there can be no doubt that this proposition must hold good in general.

Lemma 4.—Let BDE, *bde*, and ·$\mathcal{B}$· (fig. 2) be concentric spherical surfaces, whose centre is c: if the space* B*b* is filled with uniform matter, whose particles repel with a force inversely as the square of the distance; a particle placed any where within the space c*b*, as at P, will be repelled with as much force in one direction as another, or it will not be impelled in any direction. This is demonstrated in Newt. Princip. lib. 1 prop. 70. It follows also from his demonstration, that if the repulsion is inversely as some higher power of the distance than the square, the particle P will be impelled towards the centre; and if the repulsion is inversely as some lower power than the square, it will be impelled from the centre.

Lemma 5.—If the repulsion is inversely as the square of the distance, a particle placed any where without the sphere BDE, is repelled by that sphere, and also by the space B*b*, with the same force that it would if all the matter therein was collected in the centre of the sphere; provided the density of the matter in it is every where the same at the same distance from the centre. This is easily deduced from prop. 71 of the same book, and has been demonstrated by other authors.

Prop. 5, *prob.* 1.—Let the sphere BDE be filled with uniform solid matter, overcharged with electric fluid; let the fluid in it be moveable, but unable to escape from it: let the fluid in the rest of infinite space be moveable, and sufficient to saturate the matter in it; and let the matter in the whole of infinite space, or at least in the space B³, whose dimensions will be given below, be uniform and solid; and let the law of the electric attraction and repulsion be inversely as the square of the distance; it is required to determine in what manner the fluid will be disposed both within and without the globe.

Take the space B*b* such, that the interstices between the particles of matter in it shall be just sufficient to hold a quantity of electric fluid, whose particles are pressed close together, so as to touch each other, equal to the whole redundant fluid in the globe, besides the quantity requisite to saturate the matter in n*b*; and take the space B³ such, that the matter in it shall be just able to saturate the redundant fluid in the globe: then, in all parts of the space n*b*, the fluid will be pressed close together, so that its particles shall touch each other; the space B₃ will

be entirely deprived of fluid; and in the space *cb*, and all the rest of infinite space, the matter will be exactly saturated.

For, if the fluid is disposed in the above mentioned manner, a particle of fluid placed any where within the space *cb* will not be impelled in any direction by the fluid in *Db*, or the matter in B*3*, and will therefore have no tendency to move; a particle placed any where without the sphere *βδι* will be attracted with just as much force by the matter in B*3*, as it is repelled by the redundant fluid in B*b*, and will therefore have no tendency to move: a particle placed any where within the space B*b*, will indeed be repelled towards the surface, by all the redundant fluid in that space which is placed nearer the centre than itself; but as the fluid in that space is already pressed as close together as possible, it will not have any tendency to move; and in the space B*3* there is no fluid to move, so that no part of the fluid can have any tendency to move.

Moreover, it seems impossible for the fluid to be at rest, if it is disposed in any other form; for if the density of the fluid is not every where the same at the same distance from the centre. but is greater near *b* than near *d*, a particle placed any where between those two points will move from *b* towards *d*; but if the density is every where the same, at the same distance from the centre, and the fluid in B*b* is not pressed close together, the space *cb* will be overcharged, and consequently a particle at *b* will be repelled from the centre, and cannot be at rest: in like manner, if there is any fluid in B*3*, it cannot be at rest: and, • by the same kind of reasoning, it might be shown, that, if the fluid is not spread uniformly within the space *cb*, and without the sphere *βδι*, it cannot be at rest.

Corol. 1. If the globe BD*E* is undercharged, every thing else being the same as before, there will be a space B*b*, in which the matter will be entirely deprived of fluid, and a space B*ε*, in which the fluid will be pressed close together; the matter in B*b* being equal to the whole redundant matter in the globe, and the redundant fluid in B*3*, being just sufficient to saturate the matter in B*b*: and in all the rest of space the matter will be exactly saturated. The demonstration is exactly similar to the foregoing.

Corol. 2. The fluid in the globe BD*E* will be disposed in exactly the same manner, whether the fluid without is immoveable, and disposed in such manner that the matter shall be every where saturated, or whether it is disposed as above described; and the fluid without the globe will be disposed in just the same manner, whether the fluid within is disposed uniformly, or whether it is disposed as above described.

Prop. 6, *prob.* 2.—To determine in what manner the fluid will be disposed in the globe BD*E*, supposing every thing as in

the last problem, except that the fluid on the outside of the globe is immoveable, and disposed in such manner as every where to saturate the matter, and that the electric attraction and repulsion is inversely as some other power of the distance than the square.

I am not able, says Mr. C., to answer this problem accurately; but I think we may be certain of the following circumstances.

Case 1. Let the repulsion be inversely as some power of the distance between the square and the cube, and let the globe be overcharged. It is certain that the density of the fluid will be every where the same at the same distance from the centre. Therefore, first, there can be no space, as *cb*, within which the matter will be every where saturated; for a particle at *b* is impelled towards the centre, by the redundant fluid in B*b.*, and will therefore move towards the centre, unless *cb* is sufficiently over-charged to prevent it. Secondly, the fluid close to the surface of the sphere will be pressed close together; for otherwise a particle so near to it, that the quantity of fluid between it and the surface should be very small, would move towards it; as the repulsion of the small quantity of fluid between it and the surface, would be unable to balance the repulsion of the fluid on the other side. Whence, he thinks, we may conclude, that the density of the fluid will increase gradually from the centre to the surface, where the particles will be pressed close together. Whether the matter exactly at the centre will be overcharged, or only saturated, he cannot tell.

Corol. For the same reason, if the globe be undercharged, he thinks we may conclude, that the density of the fluid will diminish gradually from the centre to the surface, where the matter will be entirely deprived of fluid.

Case 2. Let the repulsion be inversely as some power of the distance less than the square; and let the globe be overcharged. There will be a space *nb*, in which the particles of the fluid will be every where pressed close together; and the quantity of redundant fluid in that space will be greater than the quantity of redundant fluid in the whole globe BDE; so that the space *cb*, taken all together, will be undercharged. But he cannot tell in what manner the fluid will be disposed in that space. For it is certain that the density of the fluid will be every where the same at the same distance from the centre. Therefore, let *b* be any point where the fluid is not pressed close together, then will a particle at *b* be impelled towards the surface, by the redundant fluid in the space B*b*; therefore, unless the space *cb* is undercharged, the particle will move towards the surface.

Corol. For the same reason, if the globe is undercharged,

there will be a space B*b*, in which the matter will be entirely deprived of fluid; the quantity of matter in it being more than the whole redundant matter in the globe; and consequently the space c*b*, taken all together, will be overcharged.

Lemma 6.—Let the whole space comprehended between two parallel planes, infinitely extended each way, be filled with uniform matter, the repulsion of whose particles is inversely as the square of the distance; the plate of matter formed thereby will repel a particle of matter with exactly the same force, at whatever distance from it, it be placed.

For, suppose that there are two such plates, of equal thickness, placed parallel to each other, let A (fig. 3) be any point not placed in or between the two plates: let BCD represent any part of the nearest plate: draw the lines AB, AC, and AD, cutting the farthest plate in *b*, *c*, and *d*; for it is plain, that if they cut one plate, they must, if produced, cut the other; the triangle BCD is to the triangle *bcd*, as AB^2 to A*b*^{2}; therefore a particle of matter at A will be repelled with the same force by the matter in the triangle BCD, as by that in *bcd*. Whence it appears, that a particle at A will be repelled with as much force by the nearest plate, as by the more distant; and consequently will be impelled with the same force by either plate, at whatever distance from it it be placed.

Corol. If the repulsion of the particles is inversely as some higher power of the distance than the square, the plate will repel a particle with more force, if its distance be small than if it be great; and if the repulsion is inversely as some lower power than the square, it will repel a particle with less force, if its distance be small, than if it be great.

Prop. 7, *prob.* 3.—In fig. 4 let the parallel lines A*a*, B*b*, &c, represent parallel planes infinitely extended each way: let the spaces* AD and EH be filled with uniform solid matter: let the electric fluid in each of those spaces be moveable and unable to escape; and let all the rest of the matter in the universe be saturated with immoveable fluid; and let the electric attraction and repulsion be inversely as the square of the distance. It is required to determine in what manner the fluid will be disposed in the spaces AD and EH, according as one or both of them are over or undercharged.

Let AD be that space which contains the greatest quantity of redundant fluid, if both spaces are overcharged, or which contains the least redundant matter, if both are undercharged; or, if one is overcharged, and the other undercharged, let AD be the overcharged one. Then, first, there will be two spaces, AB and GH, which will either be entirely deprived of fluid, or

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*By the space AD or AB, &c. is meant the space comprehended between the planes A*a* and D*d*, or between A*a* and B*b*—Orig.

in which the particles will be pressed close together ; namely, if the whole quantity of fluid in AD and EH together, is less than sufficient to saturate the matter therein, they will be entirely deprived of fluid ; the quantity of redundant matter in each being half the whole redundant matter in AD and EH together : but if the fluid in AD and EH together is more than sufficient to saturate the matter, the fluid in AB and GH will be pressed close together ; the quantity of redundant fluid in each being half the whole redundant fluid in both spaces. 2d. In the space CD the fluid will be pressed close together ; the quantity of fluid in it being such, as to leave just enough fluid in BC to saturate the matter in it. 3d. The space EF will be entirely deprived of fluid ; the quantity of matter in it being such, that the fluid in FG shall be just sufficient to saturate the matter in it : consequently the redundant fluid in CD will be just sufficient to saturate the redundant matter in EF ; for as AB and GH together contain the whole redundant fluid or matter in both spaces, the spaces BD and EG together contain their natural quantity of fluid ; and therefore, as BC and FG each contain their natural quantity of fluid, the spaces CD and EF together contain their natural quantity of fluid. And, 4th, the spaces BC and FG will be saturated in all parts.

For, 1st. if the fluid is disposed in this manner, no particle of it can have any tendency to move : for a particle placed any where in the spaces BC and FG, is attracted with just us much force by EF, as it is repelled by CD ; and it is repelled or attracted with just as much force by AB, as it is in a contrary direction by GH, and consequently has no tendency to move. A particle placed any where in the space CD, or in the spaces AB and GH, if they are overcharged, is indeed repelled with more force towards the planes Dd, Aa, and Hh, than it is in the contrary direction ; but as the fluid in those spaces is already as much compressed as possible, the particle will have no tendency to move.

2d. It seems impossible that the fluid should be at rest, it it is disposed in any other manner : but as this part of the demonstration is exactly similar to the latter part of that of problem the first, it is omitted.

Corol 1. If the two spaces AB and EH are both overcharged, the redundant fluid in CD is half the difference of the redundant fluid in those spaces, added to the quantity in AB, which is half the sum, is equal to the whole quantity in AD. For a like reason, if AD and FH are both undercharged, the redundant matter in EF is half the difference of the redundant matter in those spaces ; and if AD is overcharged, and EH undercharged, the redundant fluid in CD exceeds half the redundant fluid in AD, by a quantity sufficient to saturate half the redundant matter in EH.

Corol 2. It was before said, that the fluid in the spaces AB and GH (when there is any fluid in them) is repelled against the planes A*a* and H*h*; and consequently would run out through those planes, if there was any opening for it to do so. The force with which the fluid presses against the planes A*a* and H*h*, is that with which the redundant fluid in AB is repelled by that in GH; that is, with which half the redundant fluid in both spaces is repelled by an equal quantity of fluid. Therefore the pressure against A*a* and H*h* depends only on the quantity of redundant fluid in both spaces together, and not at all on the thickness or distance of those spaces, or on the proportion in which the fluid is divided between the two spaces. If there is no fluid in AB and GH, a particle placed on the outside of the spaces AB and EH, contiguous to the plains A*a* or H*h*, is attracted towards those plains by all the matter in AB and GH, *id est*, by all the redundant matter in both spaces; and consequently endeavours to insinuate itself in the space AD or EH; and the force with which it does so, depends only on the quantity of redundant matter in both spaces together. The fluid in CD also presses against the plane D*d*, and the force with which it does so, is that with which the redundant fluid in CD is attracted by the matter in EF.

Corol 3. If AD is overcharged, and EH undercharged, and the redundant fluid in AD is exactly sufficient to saturate the redundant matter in EH, all the redundant fluid in AD will be collected in the space CD, where it will be pressed close together: the space EF will be entirely deprived of fluid, the quantity of matter in it being just sufficient to saturate the redundant fluid in CD, and the spaces AC and FH will be every where saturated. Moreover, if an opening is made in the planes A*a* or H*h*, the fluid within the spaces AD or EH will have no tendency to run out at it, nor will the fluid on the outside have any tendency to run in at: a particle of fluid too placed any where on the outside of both spaces, as at P, will not be at all attracted or repelled by those spaces, any more than if they were both saturated; but a particle placed any where between those spaces, as at S, will be repelled from *d* towards *e*; and if a communication was made between the two spaces, by the canal *de*, the fluid would run out of AD into EH, till they were both saturated.

Prop. 8, *prob.* 4.—To determine in what manner the fluid will be disposed in the space AD, supposing that all the rest of the universe is saturated with immoveable fluid, and that the electric attraction and repulsion is inversely as some other power of the distance than the square. I am not able, says Mr. C., to answer this problem accurately, except when the repulsion is inversely as the simple or some lower power of the distance; but I think we may be certain of the following circumstances.

Case 1.—Let the repulsion be inversely as some power of
the distance between the square and the cube, and let AD be
overcharged. 1st. It is certain that the density of the fluid
must be every where the same, at the same distance from the
planes A*a* and D*d*. 2d. There can be no space, as BC, of any
sensible breadth, in which the matter will not be overcharged.
And, 3d. The fluid close to the planes A*a* and D*d* will be
pressed close together. Whence he thinks, we may conclude,
that the density of the fluid will increase gradually from the
middle of the space to the outside, where it will be pressed
close together. Whether the matter exactly in the middle will
be overcharged, or only saturated, he cannot tell.

Case 2. Let the repulsion be inversely as some power of the
distance between the square and the simple power, and let AD
be overcharged. There will be two spaces, AB and DC, in
which the fluid will be pressed close together, and the quantity
of redundant fluid in each of those spaces will be more than
half the redundant fluid in AD; so that the space BC, taken all
together, will be undercharged; but he cannot tell in what
manner the fluid will be disposed in that space. The demon-
strations of these two cases are exactly similar to those of the
two cases of prob. 2.

Case 3. If the repulsion is inversely as the simple, or some
low power, of the distance, and AD is overcharged, all the
fluid will be collected in the spaces AB and CD, and BC will be
entirely deprived of fluid. If AD contains just fluid enough to
saturate it, and the repulsion is inversely as the distance, the
fluid will remain in equilibrio, in whatever manner it is disposed;
provided its density is every where the same, at the same dis-
tance from the planes A*a* and D*d*, but if the repulsion is
inversely as some less power than the simple one, the fluid will
be in equilibrio, whether it is either spread uniformly, or
whether it is all collected in that plane which is in the middle
between A*a* and D*d*, or whether it is all collected in the spaces
AB and CD; but not, he believes, if it is disposed in any other
manner. The demonstration depends on this circumstance;
namely, that if the repulsion is inversely as the distance, two
spaces, AB and CD, repel a particle, placed either between them,
or on the outside of them, with the same force as if all the
matter of those spaces was collected in the middle plane
between them. It is needless mentioning the three cases in
which AD is undercharged, as the reader will easily supply the
place.

Though the four foregoing problems do not immediately
tend to explain the phenomena of electricity, Mr. C. chose to
insert them; partly because they seem worthy engaging our
attention in themselves; and partly because they serve, in
some measure, to confirm the truth of some of the following

propositions, in which he was obliged to make use of a less accurate kind of reasoning.

In the following propositions, Mr. C. always supposes the bodies he speaks of to consist of solid matter, confined to the same spot, so as not to be able to alter its shape or situation by the attraction or repulsion of other bodies on it: he also supposes the electric fluid in these bodies to be moveable, but unable to escape, unless when otherwise expressed. As for the matter in all the rest of the universe, he supposes it to be saturated with immoveable fluid. He also supposes the electric attraction and repulsion to be inversely as any power of the distance less than the cube, except when otherwise expressed.

By a canal, he means a slender thread of matter, of such kind that the electric fluid shall be able to move readily along it, but shall not be able to escape from it, except at the ends, where it communicates with other bodies. Thus, when he says that two bodies communicate with each other by a canal, he means that the fluid shall be able to pass readily from one body to the other by that canal.

Prop. 9. If any body, at a distance from any over or undercharged body, be overcharged, the fluid within it will be lodged in greater quantity near the surface of the body than near the centre. For, if you suppose it to be spread uniformly all over the body, a particle of fluid in it, near the surface, will be repelled towards the surface by a greater quantity of fluid than that by which it is repelled from it; consequently the fluid will flow towards the surface, and make it denser there: moreover, the particles of fluid close to the surface will be pressed close together; for otherwise, a particle placed so near it, that the quantity of redundant fluid between it and the surface should be very small, would move towards it; as the small quantity of redundant fluid between it and the surface would be unable to balance the repulsion of that on the other side.

From the four foregoing problems it seems likely, that if the electric attraction or repulsion is inversely as the square of the distance, almost all the redundant fluid in the body will be lodged close to the surface, and there pressed close together, and the rest of the body will be saturated. If the repulsion is inversely as some power of the distance between the square and the cube, it is likely that all parts of the body will be overcharged, and if it is inversely as some less power than the square, it is likely that all parts of the body, except those near the surface will be undercharged.

Corol. For the same reason, if the body is undercharged, the deficiency of fluid will be greater near the surface than near the centre, and the matter near the surface will be entirely deprived of fluid. It is likely too, if the repulsion is inversely

as some higher power of the distance than the square, that all parts of the body will be undercharged: if it is inversely as the square, that all parts, except near the surface will be saturated, and if it is inversely as some less power than the square, that all parts, except near the surface, will be overcharged.

Prop. 10. Let the bodies A and D (fig. 5), communicate with each other, by the canal EF; and let one of them as D, be overcharged; the other body A will·be so also. For as the fluid in the canal is repelled by the redundant fluid in D, it is plain, that unless A was overcharged, so as to balance that repulsion, the fluid would run out of D into A. In like manner, if one is undercharged, the other must be so too.

Prop. 11. Let the body A (fig. 6) be either saturated or over or undercharged; and let the fluid within it be in equilibrio. Let now the body B, placed near it, be rendered overcharged, the fluid within it being supposed immoveable, and disposed in such manner, that no part of it shall be undercharged; the fluid in A will no longer be in equilibrio, but will be repelled from B; therefore the fluid will flow from those parts of A which are nearest to B, to those which are more distant from it; and consequently the part adjacent to MN (that part of the surface of A which is turned towards B) will be made to contain less electric fluid than it did before, and that adjacent to the opposite surface RS will contain more than before.

It must be observed, that when a sufficient quantity of fluid has flowed from MN towards RS, the repulsion which the fluid in the part adjacent to MN exerts on the rest of the fluid in A, will be so much weakened, and the repulsion of that in the part near RS will be so much increased, as to compensate the repulsion of B, which will prevent any more fluid flowing from MN to RS. The reason why he supposes the fluid in B to be immoveable, is, that otherwise a question might arise, whether the attraction or repulsion of the body A, might not cause such an alteration in the disposition of the fluid in B, as to cause some parts of it to be undercharged; which might make it doubtful, whether B did on the whole repel the fluid in A. It is evident however, that this proposition would hold good, though some parts of B were undercharged, provided it did on the whole repel the fluid in A.

Corol. If B had been made undercharged, instead of overcharged, it is plain that some fluid would have flowed from the farther part RS to the nearer part MN, instead of from MN to RS.

Prop. 12. Let us now suppose that the body A communicates by the canal EF, with another body D, placed on the contrary side of it from B, as in fig. 5; and let these two bodies be either saturated, or over or undercharged; and let the fluid within them be in equilibrio. Let now the body B be overcharged: it

is plain that some fluid will be driven from the nearer part MN to the farther part RS, as in the former proposition; and also some fluid will be driven from RS, through the canal, to the body D; so that the quantity of fluid in D will thus be increased, and the quantity in A, taking the whole body together, will be diminished, the quantity in the part near MN will also be diminished: but whether the quantity in the part near RS will be diminished or not, does not appear for certain; but Mr. C. imagines it would be not much altered.

Corol. In like manner, if B is made undercharged, some fluid will flow from D to A, and also from that part of A near RS, to the part near MN.

Prop. 13. Suppose now that the bodies A and D communicate by the bent canal MP*N*npm (fig. 7), instead of the straight one EF: let the bodies be either saturated or over or undercharged, as before; and let the fluid be at rest; then if the body B is made overcharged, some fluid will still run out of A into D; provided the repulsion of B on the fluid in the canal is not too great.

The repulsion of B on the fluid in the canal, will at first drive some fluid out of the leg MP*pm* into A, and out of NP*pn* into D, till the quantity of fluid in that part of the canal which is nearest to B is so much diminished and its repulsion on the rest of the fluid in the canal is so much diminished also, as to compensate the repulsion of B: but as the leg NP*pn* is longer than the other, the repulsion of B on the fluid in it will be greater; consequently some fluid will run out of A into D, on the same principle that water is drawn out of a vessel through a syphon: but if the repulsion of B on the fluid in the canal is so great, as to drive all the fluid out of the space GP*H*pG, so that the fluid in the leg MG*pm* does not join to that in NB*pn*; then it is plain that no fluid can run out of A into D; any more than water will run out of a vessel through a syphon, if the height of the bend of the syphon above the water in the vessel, is greater than that to which water will rise in vacuo.

Corol. If B is made undercharged, some fluid will run out of D into A; and that though the attraction of B on the fluid in the canal is ever so great.

Prop. 14. Let ABC, fig. 8, be a body overcharged with immoveable fluid, uniformly spread; let the bodies near ABC on the outside be saturated with immovable fluid; and let D be a body inclosed within ABC, and communicating by the canal DG with other distant bodies saturated with fluid; and let the fluid in D and the canal and those bodies be moveable; then will the body D be rendered undercharged.

For let us first suppose that D and the canal are saturated, and that D is nearer to B than to the opposite part of the body C; then will all the fluid in the canal be repelled from C by the

redundant fluid in ABC ; but if D is nearer to C than to B, take the point F, such that a particle placed there would be repelled from C with as much force, as one at D is repelled towards C ; the fluid in DF, taking the whole together, will be repelled with as much force one way as the other, and the fluid in PG is all of it repelled from C :. therefore in both cases the fluid in the canal, taking the whole together, is repelled from C; consequently some fluid will run out of D and the 'canal, till the attraction of the unsaturated matter there is sufficient to balance the repulsion of the redundant fluid in ABC.

Prop. 15. If we now suppose that the fluid on the outside of ABC is moveable ; the matter adjacent to ABC on the outside will become undercharged. Mr. C. sees no reason however to think that that will prevent the body D from being undercharged ; but he cannot say exactly what effect it will have, except when ABC is spherical, and the repulsion is inversely as the square of the distance ; in this case it appears, by prob. 1, that the fluid in the part DE of the canal will be repelled from C, with just as much force as in the last proposition: but the fluid in the part BG will not be repelled at all: consequently D will be undercharged, but not so much as in the last proposition.

Corol. If ABC is now supposed to be undercharged, it is certain that D will be overcharged, provided the matter near ABC on the outside is saturated with immoveable fluid ; and there is great reason to think that it will be so, though the fluid in that matter is moveable.

Prop. 16. Let AEFB, (fig. 9), be a long cylindric body, and D an undercharged body ; and let the quantity of fluid AEFB be such, that the part near EF shall be saturated. It appears, from what has been said before, that the part near AB will be overcharged ; and moreover there will be a certain space, as A*abb*B, adjoining to the plane AB, in which the fluid will be pressed close together ; and the fluid in that space will press against the plane AB, and will endeavour to escape from it ; and, by prop. 2, the two bodies will attract each other : then the force with which the fluid presses against the plane AB, is very nearly the same with which the two bodies attract each other in the direction EA ; provided that no part of AEFB is undercharged.

Suppose so much of the fluid in each part of the cylinder, as is sufficient to saturate the matter in that part, to become solid ; the remainder, or the redundant fluid remaining fluid as before. In this case the pressure against the plane AB must be exactly equal to that with which the two bodies attract each other, in the direction EA: for the force with which D attracts that part of the fluid which we supposed to become solid, is exactly equal to that with which it repels the matter in the cylinder ; and the redundant fluid in E*ab*F is at liberty to move, if it had any ten-

dency to do so, without moving the cylinder; so that the only
thing which has any tendency to impel the cylinder in the direc-
tion EA, is the pressure of the redundant fluid in AABB against
AB; and as the part near EF is saturated, there is no redundant
fluid to press against the plane EF, and thus to counteract the
pressure against AB. Suppose now all the electric fluid in the
cylinder to become fluid; the force with which the two bodies
attract each other, will remain exactly the same; and the only

(*To be concluded in next number, with a plate*).

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On the *Principles of Electro-Magnetical Machines.* By Pro-
FESSOR JACOBI, of St. Petersburgh.

I have the honour to present to the British Association an
historical sketch of the laws which regulate the action of electro-
magnetic machines, laws which will enable us to determine in a
precise manner the important question, of the application of this
remarkable force as a moving power.   Since the commence-
ment of my labours, which had partly a purely practical ten-
dency, I proposed to myself to fill up as much as possible the
blank which still remained in our knowledge of electro-mag-
netism.   With the assistance of M. Lenz, I prosecuted those
labours, which were the more arduous as they had but few
precedents in the direction which I considered it necessary to
follow, and we began to examine carefully the laws of electro-
magnets.   The report, which contains the results of our
researches, was read in June 1838, before the Academy of
Sciences at St. Petersburgh.   I take the liberty of repeating
here very briefly, the contents of this first report. The problem
which we sought to determine may be stated as follows: If a
nucleus of malleable iron and a voltaic battery of a certain sur-
face is given, into what number of elements should this surface
be divided? what should be the thickness of the wire of the
helix which surrounds the nucleus? and, lastly, what number
of turns should this helix have, in order to produce the greatest
amount of magnetism?   I will not dilate here upon the manner
in which we have proceeded, or upon the degree of certainty
which belongs to the laws established according to our observa-
tions.   I take the liberty of appending to this statement the
report in question, and will proceed to explain the particular
laws: 1st. The amount of magnetism engendered in malleable
iron by galvanic currents, is in proportion to the force of those
currents.   2ndly. The thickness of the wire twisted into a helix,

and surrounding a rod of iron, is absolutely of no consequence, provided that the helix have the same number of turns, and the current be of the same force.* This law extends also to the case in which ribbons of copper are employed instead of wire. Nevertheless I must notice, that in order to obtain a current of equal force, it is necessary to employ a voltaic apparatus of greater force, if small wires which offer a greater resistance are employed. 3rdly. If the current remain the same, the influence which the diameter of the helix exercises may be neglected in the majority of practical cases. 4thly. The total action of the electro-magnetic helix upon the rod of iron, is equal to the sum of the effects produced by each coil separately. Adopting these laws, and submitting them to calculation according to the formula of M. Ohm, the importance of which formula has but lately begun to be appreciated by some British philosophers, we have established the formula which contains all the particular conditions required to obtain the maximum amount of magnetism, which may be expressed in the following extremely simple manner, viz. *the maximum of magnetism is always obtained when the total resistance of the conducting wire, which forms the helix, is equal to the total resistance of the pile.* On referring to the remarkable law of the definite action of the galvanic current, established by Mr. Faraday, it is found that the magnetism of malleable iron divided by the consumption of zinc,—a quantity which we have called economic effect, is with reference to the maximum of this magnetism, a constant, or an expression into which neither the thickness of the wire nor the number of the elements into which the total given surface of the battery is divided enters, but only the total thickness of the envelope.

Having finished these first researches, and having obtained these results, which were highly satisfactory, not only for their simplicity, but also for their practical value, we set about extending our inquiries to iron rods of different dimensions. Is there, it may be asked, any specific effect produced by the length or thickness of the nucleus? or does the degree of magnetism solely depend upon the construction of the helix, and the force of the current? The solution of this new problem presents a greater difficulty than the problem which we had succeeded in completely solving. Now, we are obliged to take iron rods of different dimensions, and, consequently, in all probability of different qualities. Similar conditions with reference to the action of the electro-magnetic helices are likewise difficult to obtain; and we soon perceived that these circumstances rendered it impossible to attain so close an accordance, as that which we had obtained in our former observations. Although these experiments were made two years ago, the results have not yet been published, because, being occupied with other labours, we have not been able to find the necesssary time for their re-

* This inference is at variance with experiment.—EDIT.

duction and arrangement, and for the requisite calculations.
Nevertheless I take the liberty of presenting to the Section some
results, which are not devoid of interest, and which are inti-
mately connected with the question of electro-magnetic ma-
chines. We submitted nine cylinders of malleable iron, each
eight inches in length, and of different diameters, from three
inches down to one-third of an inch, to the action of a voltaic
current of the same force in each case, and we obtained the
amount of magnetic force represented in the following table.

Diameter of the rods.	Magnetism observed.	Magnetism calculated.
3	447	442
$2\frac{1}{2}$	378	376
2	308	310
$1\frac{1}{2}$	246	244
1	175	178
$\frac{5}{6}$(?)	158	156
$\frac{2}{3}$	142	135
$\frac{1}{2}$	112	113
$\frac{1}{3}$	87	91

This calculation has been made according to the formula
$m = 131.75d + 46.75$, it which the constants have been obtained
by the method of the least squares. The differences between
calculation and observation, are not so large that they cannot
be attributed to the inevitable errors of observation, and to
circumstances inherent in the qualities of iron, &c. A similar
agreement is found between other observations, which we shall
describe in the report itself. I think, therefore, we may admit
the following law, namely, that the *amount of magnetism
received by different iron rods of the same length, and submitted
to the influence of a current of the same force, is proportional to
the diameter of the rods.* I must remark, that the constant
which we have added in the formula depends upon the mag-
netic influence which the helix exercises, independently of the
nucleus of iron which it incloses. The practical consequences
which may be deduced from this remarkable law are of consi-
derable importance. Among these, however, I will at present
mention only the following. Having found that the amount of
magnetism is proportional to the surface of the malleable iron,
and taking into account the quantity of iron employed in the
electro-magnets, it is ascertained that it is more advantageous
to employ in the construction of electro-magnetic machines,
rods of small instead of large dimensions ; or rather hollow iron,
in accordance with my own experiments of 1837, which are
found in "Taylor's Scientific Memoirs," vol. ii. &c. I cannot
pass over in silence the experiments of Prof. Barlow, who, as is
well known, proved a long time before that the induction of
the. terrestrial magnetism upon malleable iron, depends only

upon the surfaces, and is almost independent of the thickness. In order to ascertain the law of electro-magnets of different lengths, M. Lenz and I undertook numerous and laborious observations, which were extended even to rods of thirteen feet in length, and keeping in view at the same time the determination of the particular distribution of magnetism in the rods. Among these observations I shall only refer to such as seem most applicable to electro-magnetic machines, and which have yielded results as simple as unexpected. The following table contains the results of some observations made with rods of the same diameter, but of different lengths, covered with electromagnetic helices, and influenced by a current of the same force. M being the magnetism of the extremities, and n the number of the coils of the helix, we have $\dfrac{M}{n} = x$, a formula according to which we may calculate the numbers contained in the third column. The numbers in the fourth column are deduced from a series of other observations, made with the same helix of 960 turns, which did not cover the whole length of the rods, but were collected at the extremities only, where they occupied a space of about two inches in length. The helices being the same in all the observations, it was only necessary to divide the magnetism of the extremities by 960, in order to find the numbers of this column.

Table of Experiments upon the Magnetic Forces of Rods of different lengths.

Length of the Rods in ft.	Number of Coils.	Mean Value of One Coil, if the Helix occupies the whole length.	Mean Value of One Coil, if the Helix occupies only the extremities.
3	946	7,334	7,560
2'.5	789	6,993	7,264
2	634	7,402	6,871
1.5	474	7,880	7,491
1	315	7,847	7,573
0.5	163	7,766	7,691
		7,537	7,408

From these numbers, it will be seen that the influence of one coil of the helix is nearly the same for all the rods, and that their length does not exercise any specific influence. It is only in proportion to the number of the turns or revolutions, and to the force of the current, that the rods can acquire a greater or less amount of magnetism. The small rods even appear to have a slight advantage over large rods, since it has been found by experiments that the actual force of rods of three feet, bears to that of rods of half a foot the ratio of seventy-three to seventy-seven. It is also found, that there is a gain of seventy-

five to seventy-four when the whole length of the rods is covered, instead of simply collecting the same number of coils around the extremities. The differences between the observations and the simple laws are, as will be judged, quite inconsiderable for practical purposes, and will, in time, I hope, entirely disappear by a complete integration embracing. the whole length of the rods, and founded upon the effect of an elementary part of the current. I will now hasten on to the immediate object of my present address. In March 1839, M. Lenz and I presented to the Academy of Sciences at St. Petersburgh, a report, which I shall present to the association. It contains the result of the experiments by which we have been enabled to establish the remarkable law, *that the attraction of the electro-magnets is proportional to the square of the force of the galvanic current, to the influence of which the rods of iron are submitted.* This law is of the highest practical importance, as it serves for the basis of the whole theory of electro-magnetic machines.

Before proceeding, I may be permitted to make some remarks concerning an instrument which I laid before the Academy of Sciences, in the commencement of this year. It is destined to regulate the galvanic current, and is of value in many investigations of this kind. During my sojourn in London, Professor Wheatstone has shown me an instrument, founded on exactly the same principles as mine, and with very inconsiderable modifications and differences. Now, it is quite impossible that he should have had the least notice of my instrument; but as it is probable that its use may be greatly extended, I must add, that while I have only used this instrument for regulating the force of the currents, he has founded upon it a new method of measuring these currents, and of determining the different elements or constants, which enter into the analytical expressions, and on which depends the action of any galvanic combination. It is principally to the measure of the electro-motive force, by those means, that Mr. Wheatstone has directed his attention; and he has shown me, in his unpublished papers, very valuable results which he has obtained by this method.

While these purely theoretical researches were in progress, I did not fail, myself, to enter directly upon the question of the practical application of electro-magnetism. Unfortunately, I cannot here give the details either of the experiments which I have made upon a very large scale, or of the machines and apparatus of various kinds which I have constructed. The necessity of multiplying the facts or tangible results—a necessity the more urgent, because the practical applications of this force increased so very rapidly—this necessity, I say, has not allowed me time or leisure to digest and arrange them. I can only here express my readiness to afford any explanation of the

details which may be desired. I will, however, particularly notice the satisfactory results of the experiments made last year with a boat of twenty-eight feet in length and seven and a half feet in width, drawing 2¾ feet of water, and carrying fourteen individuals, which was propelled upon the Neva at the rate of about three English miles in the hour. The machine, which occupied very little space, was set in motion by a battery of sixty-four pairs of platina plates, each having thirty-six square inches of surface, and charged according to the plan of Mr. Grove, with nitric and diluted sulphuric acid. Although these result may perhaps not satisfy the exaggerated expectations of some persons, it is to be remembered, that in the first year, namely, in 1838, this boat being put in motion by the same machine, and employing 320 pairs of plates, each of thirty-six square inches, and charged with sulphate of copper, only half this velocity was obtained. This enormous battery occupied considerable space, and the manipulation and the management of it was very troublesome. The judicious changes made in the distribution of the rods, in the construction of the commutator, and lastly, in the principles of the voltaic battery, have led to the successful result of the following year, 1839. We have gone thus on the Neva more than once, and during the whole day, partly with and partly against the stream, with a party of twelve or fourteen persons, and with a velocity not much less than that of the first invented steam-boat. I believe that more cannot be expected from a mechanical force, whose existence has only been known since 1834,*when I made the first experiment at Königsberg, in Prussia, and only succeeded in lifting a weight of about twenty ounces, by even this electro-magnetic power.

I must, on the present occasion, confess frankly and without reserve, that hitherto the construction of electro-magnetic machines has been regulated in a great measure by mere trials; that even the machines constructed according to the indisputable laws established with regard to the statical effects of electro-magnets, have been found inefficient, as soon as we came to deal with motion. Being always accustomed to proceed in a legitimate manner, and feeling great regret at the irregular attempts which were being made everywhere, without any scientific foundation, this state of things appeared to me so unsatisfactory, that I could not but direct all my efforts to ascertain clearly the laws of these remarkable machines. I submit the formulæ relative to these laws, which appear to me to recommend themselves as much by their simplicity as by the natural manner in which they develope themselves. Let R represent all the mechanical resistances acting upon the machine, and v, the uniform velocity with which it moves: we have for the power or me-

* Several Electro-magnetic Engines were made before 1834.—EDIT.

chanical effect, the expression $T = R \, v$. Let n be the number
of the coils of the helix which covers the rods; z, the number
of the plates of the battery; B, the total resistance of the gal-
vanic circuit; E, the electro-motive force; k, a coefficient,
which depends on the arrangement of the bars, the distance of
the poles, and the quality of the iron; we have then for the
maximum of the mechanical effect which will be obtained, the
expression—

$$\text{I.} \qquad T_m = \frac{z^2 \, E^2}{4 \, Bk}.$$

For the velocity, which corresponds to this maximum,

$$\text{II.} \qquad v = \frac{B}{kn^2}.$$

For the resistance acting upon the machine,

$$\text{III.} \qquad R = \frac{n^2 \, z^2 \, E^2}{4 \, B^2}.$$

Lastly, for the economic effect, $i. e.$ the duty or the mechanical
effect divided by the consumption of zinc in a given time,—

$$\text{IV.} \qquad O = \frac{E}{2k}.$$

These formulæ may be expressed in the terms : —

1st, The maximum of mechanical effect which may be ob-
tained from a machine, is proportional to the square of the
number of voltaic elements, multiplied by the square of the
electro-motive force, and divided by the total resistance of the
voltaic circuit.   There enters, moreover, into the formula, a
factor, which I have designated k, and which depends upon the
quality of the iron, the form and disposition of the rods, and
the distance between their extremities.   The result is, that with
reference to some other investigations, which I have made of
voltaic combinations, and under similar conditions, the use of
platinum and zinc, the resistance being the same, will produce an
effect two or three times greater than the use of copper, zinc.

2nd, Neither the number of the coils of the helix which covers
the rods, nor the diameter or the length of the rods themselves,
has any influence upon the maximum of the power.   It results,
therefore, that neither by adding to the length or diameter of
the rods, nor by employing a greater quantity of wire, can the
power be increased.   There is, however, this remarkable fact,
that the number of coils disappears from the formula, simply
because the force of the machine is in a direct ratio, and the
velocity is in an inverse ratio, to the square of this number.   It
is thus that the number of coils, the dimensions of the rods,
and the other constituent parts of an electro-magnetic machine,
should be considered simply as occupying the range of the
ordinary mechanisms which serve for the transmission or trans-
formation of the velocity, without increasing the available

power. So it would be possible to use, instead of the ordinary wheelwork, rods of greater or less length, or a greater or less quantity of wire, in order to establish between the force and the velocity, the relation which the applications to manufacturing processes may require.

3rd, The mean attraction of the magnetic rods, or the pressure which the machine can exert, is proportional to the square of the current. This pressure is indicated by the galvanometer, which in this manner performs the function of the manometer of steam engines.

4th, the economic effect, *i, e.* the duty or the available power, divided by the consumption of zinc, is a constant quantity, which is expressed most simply by the relation between the electro-motive force and the factor k, which has been previously noticed. I may here repeat, what I stated elsewhere, that by employing platinum instead of copper, the theoretical expenses may be reduced in the proportion of nearly 23 to 14.

5th, The consumption of zinc, which takes place while the machine is at rest, and does no work at all, is double that which takes place, while it is producing the maximum of power.

I consider that there will not be much difficulty in determining with sufficient precision the duty of one pound of zinc, by its transformation into the sulphate, in the same manner that in the steam-engine, the duty of one bushel of coal serves as a measure to estimate the effect of different combinations. The future use and application of electro-magnetic machines appears to me quite certain, especially as the mere trials and vague ideas which have hitherto prevailed in the construction of these machines, have now at length yielded to the precise and definite laws which are conformable to the general laws which nature is accustomed to observe with strictness, whenever the question of effects and their causes arises. In viewing on the one hand a chemical effect, and on the other a mechanical effect, the intermediate term scarcely presents itself at first. In the present case, it is magneto-electricity, the admirable discovery of Faraday, which we should consider as the regulating power, or, as it may be styled, the logic of electro-magnetic machines.

*Report of the committee, (Sir J. Herschel, Mr. Whewell,
Mr. Peacock, and Prof. Lloyd,) appointed to draw up plans of
scientific co-operation relating to the subject of Terrestrial
Magnetism.*

In consequence of the measures adopted, as detailed in the
last report of the committee (*Ath.* No. 620), a very extensive
system of magnetical corresponding observations has been
organized, embracing between thirty and forty stations in
various and remote parts of the globe, provided with mag-
netometers and every requisite instrument, and with observers
carefully selected, and competent to carry out, at most, if not
all the stations, a complete series of two-hourly observations,
day and night, during the whole period of their remaining in
activity, together with monthly term observations, at intervals
of two minutes and a half. Of these observatories, that at
Dublin, placed under the immediate superintendence of Pro-
fessor Lloyd, has been equipped and provided for by the
praiseworthy liberality and public spirit of the University of
that metropolis—those at Toronto, the Cape, St. Helena, and
Van Diemen's Land, as also the two itinerant observatories of
the Antarctic Expedition, by the British government—those of
Madras, Simla, Sincapore, and Aden, by the Hon. East India
Company ;—to which are to be added ten stations in European
and Asiatic Russia, and one at Pekin, established by Russia—
two by Austria, at Pragne and Milan—two by the Universities
of Philadelphia and Cambridge, in the United States—one by
the French government, at Algiers—one by the Prussian, at
Breslau—one by the Bavarian, at Munich—one by the Spanish,
at Cadiz—one by the Belgian, at Brussels—one by the Pasha
of Egypt, at Cairo—and one by the Rajah of Travancore, at
Trevandrum, in India. In addition to this list, it has recently
also been determined (at the instance of the Royal Society) by
the British government to provide for the performance of a
series of corresponding observations, both magnetic and me-
teorological, at the Royal Observatory at Greenwich, under the
able superintendence of the Astronomer Royal. At Hammer-
fest, also, in Norway, negotiations have been for some time
carrying on for establishing an observatory of a similar descrip-
tion, in which M. Hansteen has taken an especial interest. A
great number of magnetic and other instruments available for
this service, it appears, have been left at Kaafiord by M. Gay-
mard, acting for the "Commission Scientifique du Nord,"
under the directions of the French Ministry of the Marine—all
which instruments, through the efficient intervention of M.
Arago, it is understood, will be placed at the disposal of the
observer or observers who may be appointed to conduct the
observations. To complete the establishment, however, certain

instruments, as well as registry-books, &c. are still requisite.
The council of the Royal Society have undertaken to supply
these from the Wollaston Donation Fund.—As regards the
magnetic observatory at Breslau, under the direction of M.
Boguslawski, your committee have to report, that in order to
secure the establishment of that station, and to place it on an
equal footing with the rest, certain instruments, &c. required
to be provided, for which no funds existed or could be made
available on the spot, viz.—a bifilar and a vertical-force mag-
netometer, with the requisite reading telescopes, and a set of
registry-books.—As, owing to the actual circumstances of that
observatory, there appeared no prospect of these requisites
being otherwise supplied—as the station appeared to your Com-
mittee a desirable one, and as M. Boguslawski was willing and
desirous to lend his aid to this great combined operation, by
taking on himself the laborious duty of conducting the obser-
vations, your Committee conceived, that although possibly
transgressing, in some degree, the strict wording of their
powers, they were only acting up to their spirit in devoting a
portion (£185.) of the funds placed at their disposal to supply-
ing them, at the expense of the Association. Unwilling to
claim any privilege or establish any precedent for the smallest
deviation from the strict literal interpretation of a money grant,
your Committee suggest to the meeting the propriety of ratify-
ing, by an express act of recognition, their application of the
above mentioned sum. A letter from M. Boguslawski, dated
the 22nd of July, 1840, announces the safe arrival of the in-
struments and books in question, and the consequent complete
state of instrumental equipment of the Breslau observatory, ex-
pressing, at the same time, his sincere thanks for the assistance
accorded him.—By returns from the several stations authorized
by the British government, so far as yet received, it appears,
that the Observatories at the Cape and St. Helena might be
expected to be complete and ready for the reception of the
instruments in May. From Van Diemen's Land, no accounts
have yet been received. At Toronto, where the greatest delays
and difficulties were to be expected and have been experienced,
the observatory was so far advanced at the date of Mr. Riddell's
last communication, as to leave no doubt of its completion in
time for the regular observation of the August term. Mean-
while, in this, as at the other stations, all observations practicable
under the actual circumstances of each are made and regularly
forwarded ; and here your Committee would especially call
attention to the extremely remarkable phenomena exhibited at
Toronto on the 29th and 30th of May, when, by great and
good fortune, a most superb aurora appeared at the very time
of the term observations—(see table of the terms, Report of
Council of R.S., p. 38.) The phenomena of this aurora (which
was remarkable for the extent and frequency of the *pulsating*

waves alluded to in that part of the report above cited. (p. 47) relating to this subject are very minutely and scientifically described by Mr. Riddell. But what renders the occurrence presently interesting is the fact, that during the whole time of the visible appearance of this aurora on the night from the 29th to the 30th, as well as for some hours previous, while it might be presumed to be in progress, though effaced by daylight, all the three magnetical instruments were thrown into a state of continual and very extraordinary disturbance. In fact, at 6h. 25m. in the morning of the 29th, the disturbance in the magnetic declination during a single minute of time carried the needle over ten minutes of arc; and during the most brilliant part of the evening's display (from 3h. 25m. Gött. m. t. to 4h. 35m.,) the disturbances were such as to throw the scales of both the vertical and horizontal force magnetometors out of the field of view, and to produce a total change of declination, amounting to 1° 59'. It should also be remarked, that the greatest and most sudden disturbances were coincident with great bursts of the auroral streamers. The correspondence or want of correspondence of these deviations with the perturbations of the magnetic elements observed in Europe and elsewhere on the same day, cannot fail to prove of great interest. Should it fortunately have happened that Captain Ross has been able to observe that term at Kerguelen's Land, which is not very far from the antipodes of Toronto, an indication will be afforded whether or not the electric streams producing the aurora are to be regarded as diverging from one magnetic pole or region, and converging to another.—Your Committee cannot conclude this report, without congratulating the Association and the scientific world in general on the extensive interest inspired, and the vast range of observation consequently embraced by this operation, which, so far as any accounts have hitherto reached them, appears to be going on prosperously in all its parts, and to promise results fully answerable to every expectation of its promoters. Neither would they feel justified in their own eyes, were they to omit expressing their deep and grateful sense of the indefatigable personal exertions of Major Sabine throughout the whole of the progress, both in carrying on a most voluminous correspondence, in ordering, arranging, ond despatching instruments, and facilitating, by constant attention and activity, those innumerable details which are involved in a combination so extensive —a combination which, but for those exertions, your Committee are fully of opinion must have been greatly wanting in that unity of design and co-operation which now so eminently characterises it.

Signed, on the part of the Committee,

J. F. W. HERCHEL.

Prof. Forbes remarked on one point that appeared to him

important. The magnetic needle was known to be in a constant
state of motion, and it was heretofore supposed that these
motions were exactly identical as to time all over at least the
continent of Europe; but now it appeared, that a test of this
having presented itself on the occasion of the unusual disturb-
ance of the needle referred to, they were found not to be ex-
actly synchronal. He then made some observations on the
extraordinary auroral disturbances of the needle referred to.—
Major Sabine read the following letter from M. von. Boguslaw-
ski, director of the Magnetical Observatory at Breslau, received
since the meeting had commenced :

Breslau, September 7th, 1840.

MY DEAR SIR—I have the pleasure to inform you that
during the last magnetic term, viz. on the 28th and 29th of
August, I have made observations with the two magnetic instruments
provided by the British Association.   Notwithstanding the Mi-
chaelmas term of our University has begun, I have succeeded
in engaging and instructing a double number of observers, suffi-
cient to place them at the declination magnetometer in the
magnetic cabinet, as well as at the horizontal and at the vertical-
force magnetometers in the great room of the Observatory.   The
observations hitherto made can, however, only be considered as
observations of the magnetic variations, because there are several
masses of iron fixed in the buildings.   The prospect of obtaining
a separate magnetic observatory being still distant, I feel myself
highly indebted to Professor Lloyd for the assistance his paper
" On the Mutual Action of Permanent Magnets," &c., has afforded
me.   By these instructions, I have succeeded in effecting what at
first seemed to be impossible, namely, to place the declination
magnetometor, the bifilar instrument, and the vertical force magne-
tometer, *in the same room of the present magnetic cabinet*, and to
put them in equilibrium.   How this is to be done by three small
fixed subsidiary magnetic bars, I shall hereafter explain to Pro-
fessor Lloyd, and, if he agrees with me, all these instruments will
be placed in the magnetic cabinet at the next term.   However, I
shall use for a declination magnetometer the second magnetic bar
received with the horizontal-force magnetometer, instead of the
present bar of four pounds, in order to obtain small correction-
constants.   I shall then expect with patience the establishment of
a proper magnetic observatory, so as to begin to make absolute and
daily observations. * *   (Signed)   HENRY VON BOGUSLAWSKI.

Major Edward Sabine.

Major Sabine also presented, at the request of M. Kupffer,
director general of the magnetical observatories of Russia,
several copies of a report addressed by that gentleman to the
Imperial Academy of Sciences at St. Petersburgh, entitled,

" Sur les Observatoires magnétiques fondès par ordre des Gou-
vernemens d'Angleterre et de Russie sur plusieurs points de la
surface terrestre." In this report, the Russian observatories,
acting on the same system of observation, both magnetical and
meteorological, as those of England, are enumerated as follows :

Stations.	Directeurs.
St. Petersbourg	M. Kupffer directeur general.
Catherinebourg	M. Roschkoff
Barnaoul	M. Prange, 1er } Ingénieurs des mines.
Nertchinsk ...	M. Prange, 2eme
Kazan	M. Simopoff } Directeurs des observatoires
Nikolaïeff......	M. Knorre } astronomiques.
Tiflis	M. Philadelphine, Professor au Gymnase.
Sitka (Côte N.O. de l'Amérique.)	} MM. Homann et Fwanoff.
Helsingfors (Findland.)	} M. Nervander, Prof. extraor. à l'Université.
Pekin (Chine)	M. Gaschkevitsch } Membre de la mission ecclésiastique.

A la station de Pékin nous aurons, si non autant d'observations
que des autres stations, au moins les observations les plus im-
portantes.

In reference to the aurora which had been seen at Toronto,
in Upper Canada, on the 29th of May, and to the mangetic
perturbations by which its appearance had been accompanied,
the ASTRONOMER ROYAL stated, that the term day of the 29th
and 30th of May had also been kept at the Royal Observatory
at Greenwich, that an aurora was seen there also on the 29th
and that the disturbances of the declination magnetometer
exceeded in amount any which had been observed there on
previous occasions. Not having brought the observations with
him, he could not state whether their comparison with the curves
of the Toronto observatory, which Major Sabine had laid before
the Section, would manifest an accordance between the disturb-
ances at the two stations : a point of the highest interest as to
the nature and extent of these perturbations. He was happy
to inform the Section that her Majesty's Government had sanc-
tioned the establishment of a magnetic observatory at the Royal
Observatory at Greenwich. He had for some time had observa-
tions made under his superintendence, and had observed some
remarkable auroral disturbances of the needle, when the amount
of the deflection had, as well as he remembered, exceeded
0°.5. The coincidences of these disturbances had not been
exact; at Greenwich, as in America, they had been found to
occur earlier than those in places more to the east. One thing
in the report had a peculiar interest to him, now that he was
about to have a magnetic observatory under his own direction ;
he alluded to the mode of placing the magnets, so as to produce
the least mutual action. The three small correcting magnets

he considered to be a very useful hint; these with Professor Lloyd's investigation, he conceived left little to be desired on the subject. However, he thought, after all, it would be well to establish tables by actual observation, showing the effect produced on each magnet in all the positions ever assumed by each of the others; which might be accomplished at a small expence of time and trouble compared with its importance.

Dr. Lamont gave an account of the Magnetic Observatory of Munich, stating that the building had been undertaken in April this year, and that the regular series of observations, comprehending both the two-hourly daily observations and the term day observations, was commenced on the 1st of August. The magnetic observatory of Munich differs in two respects from other establishments of the same kind. In the first place, it is not a magnetical house, but a subterranean building, which is situated to the S.W. of the Royal Observatory, at a distance of about 120 feet, and connected with it by a subterranean passage. The depth of the magnetic observatory below the surface of the earth is 13 feet, thus affording the advantage of a temperature nearly equal at all times of the year, and rendering the corrections applied to magnetic observations in order to reduce them to a fixed temperature—corrections which are in general subject to considerable uncertainty—if not unnecessary, at least sufficiently small to be determined with the utmost degree of accuracy. In the second place, the instruments are of greater dimensions than those usually employed in magnetic observatories, and may be considered as sufficient in all respects for the most delicate investigations. The magnetic bars weigh 25lb. each; the theodolite has a circle of 2½ feet diameter, and an achromatic telescope of 3½ inches aperture. It may be remarked, that the horizontal-force instrument differs from the bifilar magnetometer, the power that holds the bar in a direction perpendicular to the magnetic meridian, being that of a spiral spring. Besides the instruments fixed, there are portable instruments for making experiments with bars of ½lb, 1lb, 4lb, 10lb, and 25lb.

Professor Forbes considered the plan of obtaining a uniform temperature excellent. The elimination of changes of temperature from the results he considered would be found highly desirable in other delicate observations as well as in magnetical; thus, in the late attempt to repeat Cavendish's experiments to determine the mean density of the earth, with all the exactness which the modern refinements in observing afforded, he believed Mr. Baily had been obliged to abandon them from the anomalies developed, chiefly, he believed, by changes of temperature.

Dr. Lamont gave a general statement of the system of Meteorological Observations carried on in Bavaria. The Royal Observatory of Munich constitutes the central establishment, and has the superintendence of all meteorological observations

made under public authority. There are meteorological observations at Ratisbon, Augsburg, and Hohen-Peissenberg, the latter being situated on the summit of a mountain 3,000 feet above the level of the sea. Besides, meteorological observations are registered partly by members of the Royal Meteorological Society, partly by persons appointed by government, at 260 towns and villages in Bavaria. The observations thus obtained, though not equally complete, some of them being registered only once, some twice, and but a comparatively small number three times a day. will be found extremely valuable for the purposes of meteorology: Hourly observations of the barometer and thermometer have been made at the Royal Observatory of Munich since May 1838, by means of accurate registering instruments, constructed on a new principle. Dr. Lamont, in mentioning this extensive system of observations, referred for the results and further particulars, to the annual publications of the Royal Observatory of Munich, and concluded by remarking that the great object of meteorology was to find the causes from which the changes in the atmosphere arise—to trace the propagation of these changes from one place to another, and the modifications they undergo on their way—to show what relation exists between the state of the atmosphere at different parts of the globe, and how the changes at one place depend upon or are connected with simultaneous or preceding changes at another. This, he said, can only be attained by combining observations made in different countries after a general and uniform system. In mentioning the extensive observations carried on in Bavaria, it was his intention to show how far a general system was likely to be supported in that part of Germany, and to express the hope that such a system will be introduced at no distant period, perhaps by the same Association by whose exertions a similar system of magnetic observations has now sosuccessfully been carried into effect.

To the Editor of the Annals of Electricity.

Sir,

If you should be of opinion that the following description of what I conceive to be a new variety of electro-magnet, would be acceptable to the readers of your Scientific Journal, you will, by inserting it in your next publication, oblige

Yours, very respectfully,

RICHARD ROBERTS.

Atlas Works,
Manchester, January 25th, 1841.

Towards the close of November last, whilst reflecting on the progress of electro-magnetism, it occurred to me that as the power of an electro-magnet depends in a considerable degree

on the extent of area of the face of the magnet in contact with the armature, provided the whole of that area be properly excited;—that a magnet having a series of grooves in its surface, into which the conducting wire shall be coiled; would, on being connected with a battery, be so excited.

I therefore, as soon as convenient, made a small magnet, with which, on the 16th of December, you were so kind as to put my hypothesis to the test of experiment, when the result seemed to prove the correctness of the view which I had taken.

Encouraged by the degree of success that attended my first experiment, I made a second magnet, of which the accompanying drawings and description will convey a tolerable correct idea.

Reference to the figures,—fig. 2, plate 1, is a view of the face of the magnet : fig. 3, is a side view taken at a : fig. 4, is a side view taken at b : fig, [5, is a side view of the armature : and fig. 6, is a side view of the armature taken at right angles to fig. 5.

The magnet is 2 and 7-16ths inches thick, and 6 and 5-8ths inches square, on its face, into which are planed (at equal distances from each eiher across its surface) four grooves, one and a quarter inches deep, and nearly three-eighths of an inch broad ; into these grooves was coiled, three-fold deep, a bundle of thirty-six copper-wires, No. 18's on the wire guage, wrapped with cotton tape, to prevent contact with the iron, as shown in figs. 2, 3, and 4, plate 1 (the wires having no insulation from each other).

The magnet, with the conducting wire, weighs 35lbs.

The armature is one and a half inches thick, and the same size as the magnet on the face : its weight is 23lbs.

The upper side of the iron which constitutes the magnet, is formed into an eye, or bow, shown at figs. 3 and 4, by which the whole is suspended.

The eye on the back of the armature is formed in like manner.

My first magnet and its armature were similar to those just described, except that the bow on the back of each was attached by four screws. The magnet was 1 and 7-16 inches thick, and 6½ inches square on its face, into which were planed 8 grooves one quarter of an inch broad, and seven-eighths of an inch deep, for the reception of the copper-conducting-wire, which was nearly a quarter of an inch in diameter, covered with cotton tape, and coiled three-fold deep. The magnet, with the conducting-wire, weighs 18¼lb. The armature is 11-16 inches thick, and the same size on the face as the magnet.

The battery employed with both magnets consisted of eight pairs, the jars of which were of cast-iron ; each side of the zinc, of each jar, presenting an area of about 50 square inches.

The load sustained by the small magnet in the experiment made on the 16th December, was 845lb., in a few days after, the experiment was repeated with a better insulated conductor, when the magnet sustained 901lb.*

The load sustained by the second magnet in the experiment made on the 14th of this month, was 2950lb., which I understand is more than has been sustained by any magnet on record, although to some of them the batteries were very powerful.

From these experiments it may be inferred that a magnet on the same principle, five feet square, and proportionately thick, would probably sustain one hundred tons.

Prize Volumes of the Annals of Electricity &c.

In order to stimulate and promote experimental inquiry, in the various departments of Electricity and Magnetism, the Editor proposes to offer prize volumes of the Annals, to those experimenters who may be most successful in the following subjects :—

1st. For a description of the most powerful, soft iron, or Electro-magnet, in proportion to the weight of the iron employed in its structure ; which is not to be less than 10lb. The voltaic battery employed will be at the option of the experimenter ; and is to be described by him, with the manner of using it in the experiments with the soft iron magnet.

2nd. For the invention of an electrical-machine, more powerful, in proportion to size, than the usual plate or cylindrical form. A full description of the apparatus, with a suitable drawing, will be required.

3rd. For an account of the most extensive and best conducted experiments of the electricity of the steam of boilers of high or low pressure engines; with all the particulars respecting the character of the water employed in each boiler ; and such other particulars as may appear interesting.

4 For the best mode of procuring Electro-type Plates, different from those published.

5 For the best paper on any branch of experimental research in Electricity or in Magnetism.

The prize for each of the above subjects will be Volume VI., of the " Annals of Electricity, Magnetism and Chemistry, &c." bound, and gold lettered in the first-rate style, with a suitable emblem and motto. To be presented to the successful candidates, or their agents, (in London, if required,) on the first day of August, 1841.

The communications on the above subjects are to be addressed to Mr. William Sturgeon, Royal Victoria Gallery of Practical Science, Manchester, on or before the 1st day of May, 1841.

* * The conductor, in both these experiments, was a copper rod of a proper diameter to fill the grooves in the magnet. With a bundle of wires for the conductor, and a larger battery, the magnet now carries 2657lbs.—EDIT.

LECTURE IV.

Besides the phenomena hitherto pointed out, the pith ball
electroscope is capable of displaying many others of a highly
interesting character, some of which I will now offer to your notice:
but for these purposes it will be better to have a longer brass wire
arm than that we have hitherto used; and, indeed, in some cases,
we shall require two or more pith ball electroscopes to assist in our
experiments. Let us now again excite our smooth glass tube, by
rubbing its surface with a piece of old black silk, observing as
before, that both the tube and the silk are dry and warm. Let
the now positive electric tube be held obliquely over the end of
the brass arm of the electroscope, as shown in the figure. The balls
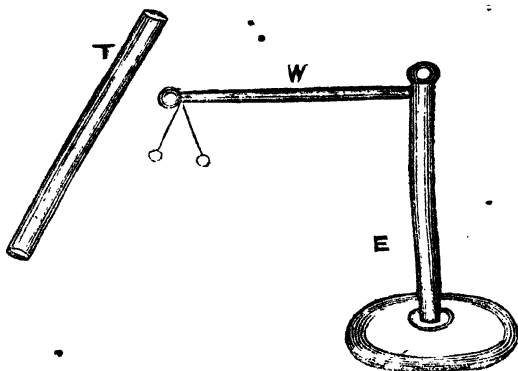
will diverge from one another as decidedly as in any of our former
experiments; but there are several facts now to be exhibited to
which I have not before alluded. Whilst the excited glass tube is
in the position shown in the figure, let another similarly excited
tube be held obliquely over the other end of the brass arm of the
electroscope. The balls, instead of diverging farther from one
another, as in our former experiments, will now come closer to-
gether; and by dexterous management of the two tubes, they may
be made to collapse and hang as close together as if under no electri-
cal influence whatever; but if instead of another excited glass tube,
or negatively excited stick of sealing wax be held over the opposite
end of the metallic arm, the pith balls will diverge farther from
one another than when they were under the electric influence of
the first glass tube alone.

These curious facts are very different to those exhibited in our
former experiments, although, to a certain extent, we have employed
the same means for their production. When we before presented
the positively excited glass tube, and the negatively excited sealing

wax, to the arm of the electroscope, we found that their electric
forces tended to destroy each others action, as was shown by the
collapsion of the pith balls, but in this case the forces of these two
differently electrized bodies are seen to operate in concert, and thus
assist each other in maintaining the divergency of the balls.   We
also observe, in this experiment, that when the electroscope is under
the influence of the electric forces of both the positively excited
glass tubes at one and the same time, those forces tend to neutralize
each others action, whereas by the former experiments two similar
electric forces assisted each other in keeping the pith balls asunder.

The mystery that may probably appear to envelope these ex-
perimental facts, will partly become dispelled, when we notice the
very different positions in which the excited bodies were held,
with respect to the metallic arm of the instrument, in the two sets
of experiments; although that observation alone will not satisfy
the whole of your curiosity.   You will, very naturally, want to
know something about the *cause* of these apparently inconsisten-
cies in the operations of the same species of electric force, and as
it is here that we enter the inner portals of electric science, we
must dwell a while on these matters, in order to explain them some-
what minutely: and it is here that we must call into requisition
the force of *electric repulsion*, and avail ourselves of its powers to
a greater extent than we have hitherto had need for.   It is in the
production of these phenomena also, that this formidable electric
force is most beautifully displayed, and conveys to the mind the
surest and most satisfactory indications of its existence.

When the excited glass tube is in an electro-positive condition,
the redundant fluid on its surface, by virtue of its repulsive force,
has a tendency to *displace* the fluid belonging to all surrounding
bodies within the sphere of its action, even the electric fluid con-
tained in the atmospheric air: and as the best conductors afford
the greatest facilities for the motion of the fluid which they contain;
that portion naturally belonging to the metallic arm of the electro-
scope became easily disturbed by the approach of the glass tube,
whose electric forces repelled the greater part of it to the farther
end of the metallic rod, where it accumulated and caused that end
of the rod to be *positively* electrical, at the expense of the other
end, which, in consequence, became defective, or *negatively* elec-
trical.   Hence you will understand, that in consequence of the dis-
placement of a portion of its electric fluid, by the approach of the
glass tube, the metallic arm of the electroscope was made to assume
oppositely electric conditions at its two extremities : *negative* at the
end nearest to the tube, and *positive* at the other.   When bodies
are thus electrized, or in this electric condition, they are said to be
electro-polar.   If, instead of being insulated by the glass support,
the metallic arm had been in metallic connection with the table,
the displacement of its fluid would have been more extensive, as
would have been indicated by a greater divergency of the pith
balls, for in that case it would not have been confined to the

metallic rod; for a portion of it would have found its way to the table, or, perhaps, to the floor, where it would have been diffused in the general mass amongst the surrounding bodies.
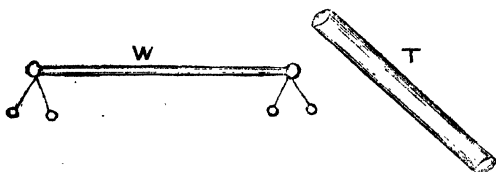
If we now approach the metallic arm of the electroscope by an excited stick of sealing-wax, in place of the glass tube, the electro-polarization of that part of the instrument will take place as decidedly as before; but we shall find, by the methods before described, that the divergent pith-balls are now in an electro-positive condition, which is contrary to their electrical state, when the glass tube was used: hence the electro-polarization of the metallic arm is now in the reverse order to that which it assumed when under the influence of the electric forces of the glass. To understand how this occurs it will be necessary to bear in mind that, by virtue of its repulsive force amongst its own particles, the electric fluid, like all other elastic fluids, moves with the greatest facility in the *direction of least resistance.* Now, as the sealing-wax has lost a portion of its fluid by this means of excitation, it has a disposition to recover a similar quantity from those vicinal bodies which are within the sphere of its attractive forces: so that when presented to the metallic arm of the electroscope the wax not only draws towards it a portion of the electric fluid in that end of the arm to which it approaches, but at the same time, causes the *electric resistance* to become much less in that direction than in any other: and therefore the fluid naturally belonging to the metal is constrained to accumulate in that end next to the wax, by the joint action of both the attractive and repulsive forces. Hence it is that the end of the metal arm next to the sealing-wax is electro-positive, and the other end electro-negative.

We are now prepared to explain the reason of the pith-balls diverging more when the farther end of the metallic arm of the electroscope was approached by an excited stick of sealing-wax, than whilst under the electric influence of the glass tube only. The negative wax, in that case, tended to draw the electric fluid of the metallic arm towards it, and consequently afforded a less degree of resistance in that direction, than was offered in any other: and by thus diminishing the electric resistance at that end of the rod, the electric influence of the glass tube became assisted in displacing the fluid belonging to the vicinal end of the metal, and pressing it towards the end nearest to the negative wax. Hence you will readily perceive, that the metallic connection with the table in the one experiment, and the approach of the negative sealing-wax in the other, are but two different modes of accomplishing the same thing, both serving to facilitate the departure of the electric fluid from that extremity of the metallic arm of the electroscope which was under the influence of the repulsive electric forces of the glass tube.

Now, when another excited glass tube was presented to the opposite extremity of the metallic arm, its electric forces tended to prevent the fluid coming in that direction; and when its powers

are equal to those of the first tube, they exactly balance each other, which was the cause of the pith-balls collapsing on its approach to the electroscope. To manage this experiment, however, requires some dexterity: for if the metallic arm of the electroscope be of considerable length, say ten or twelve inches, and the two tubes be highly electrified and brought near to its two extremities, they will polarize the metal in such a manner as to be *negative* at the extremities and positive in the centre. And if you were to employ two sticks of sealing-wax in a negative electric condition instead of the glass tubes, the metallic arm would become electro-positive at the two extremities, and electro-negative at the centre. I will presently show you a slight modification of the electroscope, by means of which these beautiful facts may very easily be shown.

We must now employ a brass rod W, about ten or twelve inches long, suspended by two dry and clean silk lines, or threads of sew-

ing silk. Or it may be supported by a thin glass stem at its centre. For a trial in private, the brass rod may be supported on a clean and warm champaign glass. Two pairs of pith balls must also be used, one at each end of the rod, as seen in the figure. If an excited glass tube T, be presented to this simple piece of apparatus, in the manner shown in the figure, the metal rod will become electro-polar upon the principles I have just before described. The pith-balls diverge at both ends of the rod, and by examining them either by an excited tube of smooth glass, or by a negatively excited stick of sealing-wax, it is found that those next to the polarizing tube T, are in an electro-negative condition, and the other pair in an electro-positive condition. If we were to hang a similar pair of pith-balls, to the centre of the brass rod, they would remain together, after a moment's slight agitation, which would be caused on the first approach of the glass tube T. Hence we learn that, there is a zone of the rod's surface, about the middle part, which, during the time that it is electro-polar under these circumstances, that is perfectly neutral, being neither positively nor negatively electric.

ERRATA.—Page 112, line 36, for "manifestations" read "manipulations."

Fig. 1.
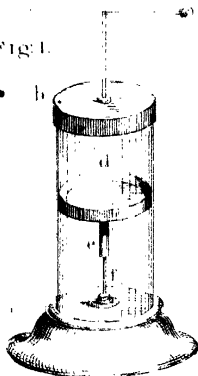
Fig. 2.
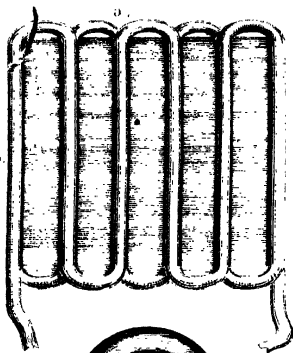
Fig. 4.
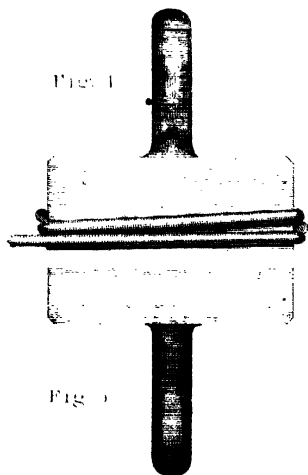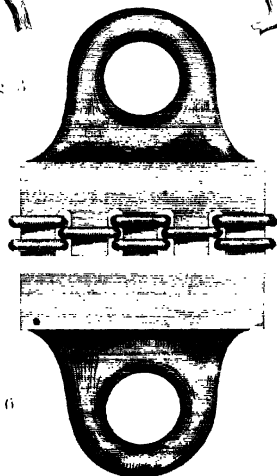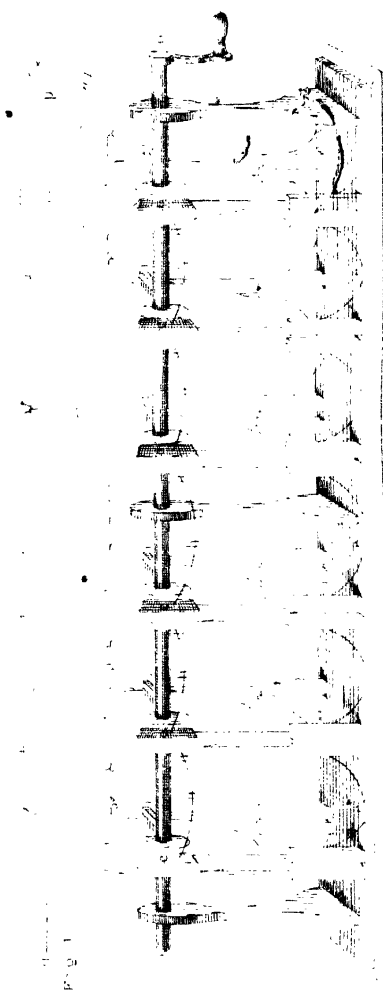
Fig. 5.

Fig. 5

Fig. 6

Fig. 1.

Fig. 2.

*An attempt to explain some of the Principal Phenomena of
Electricity, by Me as of an Elastic fluid.* By the Hon.
Henry Cavendish, F.R.S.

- *(Continued from page 152.)*

alteration in the pressure against AB, will be, that that part of
the fluid in A*ab*B, which we at first supposed solid and unable
to press against the plane, will now be at liberty to press
against it ; but as the density of the fluid, when its particles
are pressed close together, may be supposed many times
greater than when it is no denser than sufficient to saturate the
matter in the cylinder, and consequently the quantity of re-
dundant fluid in A*ab*B many times greater than that which is
required to saturate the matter in it, it follows that the pressure
against AB will be very little more than on the first supposition.

N.B. If any part of the cylinder is undercharged, the pres-
sure against AB is greater than the force with which the bodies
attract. If the electric repulsion is inversely as the square or
some higher power of the distance, it seems very unlikely that
any part of the cylinder should be undercharged : but if the
repulsion is inversely as some lower power than the square, it is
not improbable but some part of the cylinder may be under-
charged.

Lemma 7. Let AB, fig. 10, represent an infinitely thin flat circular plate, seen edgeways, so as to appear to the eye as a straight line; let c be the centre of the circle; and let DC, passing through c, be perpendicular to the plane of the plate; and let the plate be of uniform thickness, and consist of uniform matter, whose particles repel with a force inversely as the n power of the distance, n being greater than 1, and less than 3: the repulsion of the plate on a particle at D, is proportional to $\frac{DC}{DC^{n-1}} - \frac{DC}{DA^{n-1}}$ provided the thickness of the plate and size of the particle D is given.

For if cA is supposed to flow, the corresponding fluxion of the quantity of matter in the plate, is proportional to CA $\times$ c'A; and the corresponding fluxion of the repulsion of the plate on the particle D, in the direction DC, is proportional to $\frac{CA \times C'A}{DA^n} \times \frac{DC}{DA} = \frac{D'A \times DC}{DA^n}$; for D'A is to C'A :: CA : DA; the variable part of the fluent of which is $\frac{- DC}{n - 1 \times DA^{n-1}}$: whence the repulsion of the plate on the particle D, is proportional to $\frac{DC}{n - 1 \times DC^{n-1}} - \frac{DC}{n - 1 \times DA^{n-1}}$, or to $\frac{DC}{DC^{n-1}} - \frac{DC}{DA^{n-1}}$.

Corol. If DC^{n-1} is very small in respect of CA^{n-1}, the particle D is repelled with very nearly the same force as if the diameter of the plate was infinite.

Lemma. 8 Let L and l represent the two legs of a right angled triangle, and h the hypothenuse; if the shorter leg l is so much less than the other, that l^{n-1} is very small in respect of L^{n-1}, then $h^{3-n} - L^{3-n}$ will be very small in respect of l^{3-n}. For $h^{3-n} = (L^2 + l^2)^{\frac{3-n}{2}} = L^{3-n} \times (1 + \frac{l^2}{L^2})^{\frac{3-n}{2}} = L^{3-n} \times 1 + \frac{3-n \times l^2}{2L^2} \frac{3 - n \times n - 1 \times l^4}{8L^4}$, &c. therefore $h^{3-n} - L^{3-n} = \frac{3 - n \times l^2}{2L^{n-1}} - \frac{3 - n \times n + 1 \times l^4}{8L^{n+1}}$ &c. $= \frac{l^{3-n} \times 3 - n \times l^{n-1}}{2L^{n-1}} - \frac{l^{3-n} \times 3 - n \times n - 1 \times l^{n+1}}{8L^{n+1}}$ &c. which is very small in respect of l^{3-n}; as l^{n-1} is by the supposition very small in respect of L^{n-1}.

Lemma 9.—Let DC now represent the axis of a cylindric or prismatic column of uniform matter; and let the diameter of the column be so small, that the repulsion of the plate AB on it shall not be sensibly different from what it would be, if all the

matter in it was collected in the axis : the force with which the plate repels the column, is proportional to $DC^{3-n} + AC^{3-n} - DA^{3-n}$; supposing the thickness of the plate and base of the column to be given. For, if DO is supposed to flow, the corresponding fluxion of the repulsion is proportional to

$$\frac{D \cdot C}{DC^{n-2}} - \frac{DC + D \cdot C}{DA^{n-1}} = \frac{D \cdot C}{DC^{n-1}} - \frac{D \cdot A}{DA^{n-1}};$$ the fluent of which $\frac{AC^{3-n} + DC^{3-n} - DA^{3-n}}{3 - n}$, vanishes when DC vanishes.

Corol. 1. If the length of the column is so great, that AC^{n-1} is very small in respect of DC^{n-1}, the repulsion of the plate on it is very nearly the same as if the column was infinitely continued. For, by lemma 8, $AC^{3-n} + DC^{3-n} - DA^{3-n}$ differs very little in this case from AC^{3-n}; and if DC is infinite, it is exactly equal to it.

Corol. 2. It AC^{n-1} is very small in respect of DC^{n-1}, and the point E be taken in DC, such that EC^{n-1} shall be very small in respect of AC^{n-1}, the repulsion of the plate on the small part of the column EC, is to its repulsion on the whole column DC, very nearly as EC^{3-n} to AC^{3-n}.

Lemma 10. If we now suppose all the matter of the plate to be collected in the circumference of the circle, so as to form an infinitely slender uniform ring, its repulsion on the column DC will be less than when the matter is spread uniformly all over the plate in the ratio of $\dfrac{3-n \times AC^2}{2} \times (\dfrac{1}{AC^{n-1}} - \dfrac{1}{DA^{n-1}})$ to $DC^{3-n} + AC^{3-n} - DA^{3-n}$

For it was before said, that if the matter of the plate be spread uniformly, its repulsion on the column will be proportional to $DC^{3-n} + AC^{3-n} - DA^{3-n}$, or may be expressed by it; let now AC, the semidiameter of the plate, be increased by the infinitely small quantity $A \cdot C$; the quantity of matter in the plate will be increased by a quantity, which is to the whole, as $2A \cdot C$ to AC; and the repulsion of the plate on the column will be increased by $3 - n \times A \cdot C \times AC^{2-n} - A \cdot C \times \dfrac{AC}{DA} \times 3 - n$

$\times DA^{2-n} = 3 - n \times A \cdot C \times AC \times (\dfrac{1}{AC^{n-1}} - \dfrac{1}{DA^{n-1}})$: therefore if a quantity of matter, which is to the whole quantity in the plate, as $2A \cdot C$ to AC, be collected in the circumference, its repulsion on the column DC, will be to that of the whole plate, as $3 - n \times A \cdot C \times AC \times (\dfrac{1}{AC^{n-1}} - \dfrac{1}{DA^{n-1}})$ to $DC^{3-n} + AC^{3-n}$ $- DA^{3-n}$; and consequently the repulsion of the plate, when all the matter is collected in its circumference, is to its repul-

sion when the matter is spread uniformly, as $\dfrac{3 - n \times \text{AC}^2}{2} \times$

$\left(\dfrac{1}{\text{AC}^{n-1}} - \dfrac{1}{\text{DA}^{n-1}}\right)$ to $\text{DC}^{3-n} + \text{AC}^{3-n} - \text{DA}^{3-n}$.

Corol. 1. If the length of the column is so great, that AC^{n-1} is very small in respect of DC^{n-1}, the repulsion of the plate when all the matter is collected in the circumference, is to its repulsion when the matter is spread uniformly, very nearly as $\dfrac{2 - n \times \text{AC}^{3-n}}{2}$ to AC^{3-n}, or as $3 - n$ to 2.

Corol 2. If EC^{n-1} is very small in respect of AC^{n-1}, the repulsion of the plate on the short column EC, when all the matter in the plate is collected in its circumference, is to its repulsion when the matter is spread uniformly, very nearly as $\dfrac{3 - n \times n - 1 \times \text{EC}^2}{4\text{AC}^{n-1}}$ to EC^{3-n}, or as $3 - n \times n - 1 \times \text{EC}^{n-1}$ to 4AC^{n-1}; and is therefore very small in comparison of what it is when the matter is spread uniformly.

For, by the same kind of process as was used in lemma 8, it appears, that if EC^2 is very small in respect of AC^2, then $\text{AC}^2 \times \left(\dfrac{1}{\text{AC}^{n-1}} - \dfrac{1}{\text{EA}^{n-1}}\right)$ differs very little from $\dfrac{n - 1 \times \text{EC}^2}{2\text{EA}^{n-1}}$, or from $\dfrac{n - 1 \times \text{EC}^2}{2\text{AC}^{n-1}}$; and if EC^{n-1} is very small in respect of AC^{n-1}, then EC^2 is a fortiori very small in respect of AC^2.

Corol. 3. Suppose now that the matter of the plate is denser near the circumference than near the middle, and that the density at and near the middle is to the mean density, or the density which it would every where be of if the matter was spread uniformly, as δ to 1, then the repulsion of the plate on EC will be less than if the matter was spread uniformly, in a ratio approaching much nearer to that of δ to 1, than to that of equality.

Corol. 4. Let every thing be as in the last corollary, and let π be taken to 1, as the force with which the plate actually repels the column DC, (DC^{n-1} being very great in respect of AC^{n-1}) is to the force with which it would repel it, if the matter was spread uniformly; the repulsion of the plate on EC will be to its repulsion on DC, in a ratio between that of $\text{EC}^{3-n} \times \delta$ to $\text{AC}^{3-n} \times \pi$, and that of EC^{3-n} to $\text{AC}^{3-n} \times \pi$, but will approach much nearer to the former ratio than to the latter.

Lemma 11. In the line DC produced, take CF equal to CA: if all the matter of the plate AB is collected in the circumference, its repulsion on the column CD, infinitely continued, is equal to the repulsion of the same quantity of matter collected in the

point F, on the same column.   For the repulsion of the plate
on the column in the direction CD, is the same whether the
matter of it be collected in the whole circumference, or in the
point A.   Suppose it therefore to be collected in A ; and let an
equal quantity of matter be collected in F ; take FG constantly
equal to AD ; and let AD and FG flow ; the fluxion of CD is to
the fluxion of FG, as AD to CD ; and the repulsion of A on the
point D, in the direction CD, is to the repulsion of F on G, as
CD to AD ; therefore the fluxion of the repulsion of A on the
column CD, in the direction CD, is equal to the fluxion of the
repulsion of F on CG ; and when AD equals AC, the repulsion of
both A and F, on their respective columns, vanishes ; and
therefore the repulsion of A on the whole column CD, equals
that of F on CG ; and when CD and CG are both infinitely ex-
tended, they may be considered as the same column.

Prop. 17. Let two similar bodies, of different sizes, and
consisting of different sorts of matter, be both overcharged, or
both undercharged, but in different degrees ; and let the
redundance or deficience of fluid in each be very small, in
respect of the whole quantity of fluid in them : it is impossible
for the fluid to be disposed accurately in a similar manner in
both of them ;* as it has been shown that there will be a space,
close to the surface, which will either be as full of fluid as it can
hold, or will be entirely deprived of fluid; but it will be disposed
as nearly in a similar manner in both, as is possible.   To explain
this, let BDE and bde, fig. 12, be the two similar bodies ; and
let the space comprehended between the surfaces BDE and FGH
(or the space BF as he calls it for shortness) be that part of
BDE, which is either as full of fluid as it can hold, or entirely
deprived of it : draw the surface *fgh,* such that the space *bf,*
shall be to the space BF, as the quantity of redundant or defi-
cient fluid in *bde,* to that in BDE, and that the thickness of the
space *bf* shall every where bear the same proportion to the cor-
responding thickness of BF : then will the space *bf* be either as
full of fluid as it can hold, or entirely deprived of it ; and the
fluid within the space *fgh* will be disposed very nearly simi-
larly to that in the space FGH.

For it is plain, that if the fluid could be disposed accurately in a
similar manner in both bodies, the fluid would be in equilibrio
in one body, if it was in the other ; therefore draw the surface
βδ, such that the thickness of the space *βf,* shall be every
where to the corresponding thickness of BF, as the diameter of

* By the fluid being disposed in a similar manner in both bodies, Mr.
C. means that the quantity of redundant or deficient fluid, in any small
part of one body, is to that in the corresponding small part of the other, as
the whole quantity of redundant or deficient fluid in one body, to that in
the other.   By the quantity of deficient fluid in a body, he means the
quantity of fluid wanting to saturate it.   Notwithstanding the impropriety
of this expression, he begs leave to make use of it, as it will consequently
save a great deal of circumlocution.

bde to the diameter of BDE; and let the redundant fluid or matter in *bf* be spread uniformly over the space ₃ₓ*f*; then if the fluid in the space *fgh* is disposed exactly similar to that in FGH, it will be in equilibrio; as the fluid will then be disposed exactly similarly in the spaces ℬₐ and BDE; but as, by the supposition, the thickness of the space ₓ*f* is very small in respect of the diameter of *bde*, the fluid or matter in the space *bf* will exert very nearly the same force on the rest of the fluid, whether it is spread over the space ₓ*f*, or whether it is collected in *bf*.

Prop. 18. Let two bodies, B and *b*, be connected to each other by a canal of any kind, and be either over or undercharged: it is plain that the quantity of redundant or deficient fluid in B, would bear exactly the same proportion to that in *b*, whatever sort of matter B consisted of, if it was possible for the redundant or deficient fluid in any body, to be disposed accurately in the same manner, whatever sort of matter it consisted of. For suppose B to consist of any sort of matter; and let the fluid in the canal and two bodies be in equilibrio; let now B be made to consist of some other sort of matter, which requires a different quantity of fluid to saturate it; but let the quantity and disposition of the redundant or deficient fluid in it remain the same as before: it is plain that the fluid will still be in equilibrio; as the attraction or repulsion of any body depends only on the quantity and disposition of the redundant and deficient fluid in it. Therefore by the preceding proposition, the quantity of redundant or deficient fluid in B, will actually bear very nearly the same proportion to that in *b*, whatever sort of matter B consists of; provided the quantity of redundant or deficient fluid in it is very small in respect of the whole.

Prop. 19. Let two bodies B and *b*, fig. 11, be connected together by a very slender canal ADd*a*, either straight or crooked: let the canal be every where of the same breadth and thickness; so that all sections of this canal made by planes perpendicular to the direction of the canal in that part, shall be equal and similar: let the canal be composed of uniform matter; and let the electric fluid in it be supposed incompressible, and of such density as exactly to saturate the matter in it; and let it nevertheless be able to move readily along the canal; and let each particle of fluid in the canal be attracted and repelled by the matter and fluid in the canal and in the bodies B and *b*, just in the same manner that it would be if it was not incompressible;* and let the bodies B and *b* be either over or undercharged. Then the force with which the whole quantity of fluid in the canal is impelled from A towards D, in the direction of the axis

* This supposition of the fluid in the canal being incompressible, is not mentioned as a thing which can ever take place in nature, but is merely imaginary; the reason for making of which will be given hereafter.

of the canal, by the united attractions and repulsions of the two
bodies, must be nothing : as otherwise the fluid in the canal
could not be at rest: observing that by the force with which
the whole quantity of fluid is impelled in the direction of the
axis of the canal, he means the sum of the forces, with which
the fluid in each part of the canal is impelled in the direction of
the axis of the canal in that place, from A towards D ; and ob-
serving also, that an impulse in the contrary direction, from D
towards A, must be considered as negative.

For as the canal is exactly saturated with fluid, the fluid in it
is attracted or repelled only by the redundant matter or fluid
in the two bodies. Suppose now that the fluid in any section of
the canal, as Ee, is impelled with any given force in the direction
of the canal at that place, the section Dd would, in consequence
of it, be impelled with exactly the same force in the direction of
the canal at D, if the fluid between Ee and Dd was not at all
attracted or repelled by the two bodies ; and consequently the
section Dd is impelled in the direction of the canal, with the sum
of the forces, with which the fluid in each part of the canal is
impelled, by the attraction or repulsion of the two bodies in the
direction of the axis in that part ; and consequently, unless this
sum was nothing, the fluid in Dd could not be at rest.

Corol. Therefore the force with which the fluid in the canal
is impelled one way in the direction of the axis, by the body B,
must be equal to that with which it is impelled by b in the
contrary direction.

Prop. 20. Let two similar bodies B and b, fig. 13, be con-
nected by the very slender cylindric or prismatic canal Aa,
filled with incompressible fluid, in the same manner as describ-
ed in the preceding proposition : let the bodies be overcharged;
but let the quantity of redundant fluid in each bear so small a
proportion to the whole, that the fluid may be considered as
disposed in a similar manner in both ; let the bodies also be
similarly situated in respect of the canal Aa ; and let them be
placed at an infinite distance from each other, or at so great a
one, that the repulsion of either body on the fluid in the canal,
shall not be sensibly less than if they were at an infinite dis-
tance : then, if the electric attraction and repulsion is inversely
as the n power of the distance, n being greater than 1, and less
than 3, the quantity of redundant fluid in the two bodies will
be to each other, as the $n - 1$ power of their corresponding
diameters A$_F$ and af.

For if the quantity of redundant fluid in the two bodies is in
this proportion, the repulsion of one body on the fluid in the
canal, will be equal to that of the other body on it, in the con-
trary direction ; and consequently the fluid will have no ten-
dency to flow from one body to the other, as may thus be

proved. Take the points D and E very near to each other, and take da to DA, and ea to EA, as af to AF ; the repulsion of the body B on a particle at D, will be to the repulsion of b on a particle at d, as $\dfrac{1}{AF}$ to $\dfrac{1}{af}$; for, as the fluid is disposed similarly in both bodies, the quantity of fluid in any small part of B, is to the quantity in the corresponding part of b, as AF^{n-1} to af^{n-1} ; and consequently the repulsion of that small part of D, on D, is to the repulsion of the corresponding part of b on d, as $\dfrac{AF^{n-1}}{AF^n}$, or $\dfrac{1}{AF}$ to $\dfrac{1}{af}$. But the quantity of fluid in the small part DE of the canal, is to that in de, as DE to de, or as AF to af; therefore the repulsion of B on the fluid in DE, is equal to that of b on the fluid in de: therefore, taking ag to AA, as af to AF, the repulsion of b on the fluid in ag, is equal to that of B on the fluid in AA ; but the repulsion of b on ag may be considered as the same as its repulsion on AA ; for, by the supposition, the repulsion of B on AA may be considered as the same as if it was continued infinitely ; and therefore the repulsion of b on ag may be considered as the same as if it was continued infinitely.

N. B. If n was not greater than 1, it would be impossible for the length of AA to be so great, that the repulsion of B on it might be considered as the same as if it was continued infinitely ; which was the reason for requiring n to be greater than 1.

Corol. By just the same method of reasoning it appears, that if the bodies are undercharged, the quantity of deficient fluid in b will be to that in B, as af^{n-1} to AF^{n-1}.

Prop. 21. Let a thin flat plate be connected to any other body, as in the preceding proposition, by a canal of incompressible fluid, perpendicular to the plane of the plate; and let that body be overcharged ; then the quantity of redundant fluid in the plate will bear very nearly the same proportion to that in the other body, whatever the thickness of the plate may be, provided its thickness is very small in proportion to its breadth, or smallest diameter. For there can be no doubt, but under that restriction, the fluid will be disposed very nearly in the same manner in the plate, whatever its thickness may be ; and therefore its repulsion on the fluid in the canal will be very nearly the same, whatever its thickness may be.

Prop. 22. Let AB and DF, fig. 14, represent two equal and parallel circular plates, whose centres are C and E ; let the plates be placed so, that a right line joining their centres shall be perpendicular to the plates ; let the thickness of the plates be very small in respect of their distance CE ; let the plate AB communicate with the body H, and the plate DF with the body L, by the canals CG and EM of incompressible fluid, such as are

described in prop. 19 ; let these canals meet their respective
plates in their centres c and E, and be perpendicular to the
plane of the plates ; and let their length be so great, that the
repulsion of the plates on the fluid in them may be considered
as the same as if they were continued infinitely ; let the body
H be overcharged, and let L be saturated. It is plain, from
prop. 12, that DF will be undercharged, and AB will be more
overcharged than it would otherwise be. Suppose now, that
the redundant fluid in AB is disposed in the same manner as the
deficient fluid is in DF ; let P be to 1, as the force with which
the plate AB would repel the fluid in CE, if the canal ME was
continued to C, is to the force with which it would repel the
fluid in CM ; and let the force with which AB repels the fluid in
CG, be to the force with which it would repel it, if the redun-
dant fluid in it was spread uniformly, as r to 1 ; and let the
force with which the body H repels the fluid in CG, be the same
with which a quantity of redundant fluid, which we will call B,
spread uniformly over AB, would repel it in the contrary
direction. Then will the redundant fluid in AB be equal to

$$\frac{B}{2P_r - \frac{1}{P^2 r}}$$; and therefore, if P is very small, will be very

nearly equal to $\frac{B}{2P_r}$; and the deficient fluid in DF will be to
the redundant fluid in AB, as 1 — P to 1 ; and therefore, if P is
very small, will be very nearly equal to the redundant fluid in
AB.

   For it is plain, that the force with which AB repels the fluid
n EM, must be equal to that with which DF attracts it ; for
otherwise some fluid would run out of DF into L, or out of L
into DF : for the same reason, the excess of the repulsion of AB
on the fluid in CG, above the attraction of FD on it, must be
equal to the force with which a quantity of redundant fluid
equal to B, spread uniformly over AB, would repel it, or it

must be equal to that with which a quantity equal to $\frac{B}{r}$, spread

in the manner in which the redundant fluid is actually spread
in AB, would repel it. By the supposition, the force with
which AB repels the fluid in EM, is to the force with which it
would repel the fluid in CM, supposing EM to be continued to
C, as 1 — P to 1 ; but the force with which any quantity of
fluid in AB would repel the fluid in CM, is the same with which
an equal quantity similarly disposed in DF, would repel the
fluid in EM ; therefore the force with which the redundant fluid
in AB repels the fluid in EM, is to that with which an equal
quantity similarly disposed in DF, would repel it, as 1 — P to
1 : therefore, if the redundant fluid in AB be called A, the defi-
cient fluid in DF must be A × 1 — P : for the same reason ,the

force with which DF attracts the fluid in CG, is to that with which AB repels it, as $A \times 1 - P \times 1 - P$, or $A \times (1 - P)^2$, to A ; therefore, the excess of the force with which AB repels CG, above that with which DF attracts it, is equal to that with which a quantity of redundant fluid equal to $A - A \times (1 - P)^2$, or $A \times (2P - P^2)$, spread over AB, in the manner in which the redundant fluid in it is actually spread, would repel it : therefore, $A \times (2P - P^2)$ must be equal to $\frac{B}{\pi}$, or A must be equal to $\frac{B}{2P_\pi - P^2_\pi}$.

Corol. 1. If the density of the redundant fluid near the middle of the plate AB, is less than the mean density, or the density which it would every where be of, if it was spread uniformly, in the ratio of b to 1 ; and if the distance of the two plates is so small, that EC^{n-1} is very small in respect of AC^{n-1}, and that EC^{3-n} is very small in respect of AC^{3-n}. the quantity of redundant fluid in AB will be greater than $\frac{B}{2} \times \left(\frac{AC}{EC}\right)^{3-n}$, and less than $\frac{B}{2_b} \times \left(\frac{AC}{EC}\right)^{3-n}$, but will approach much nearer to the latter value than the former. For, in this case, P^τ is, by lemma 10, corol. 4, is less than $\left(\frac{EC}{AC}\right)^{3-n}$, and greater than $\left(\frac{EC}{AC}\right)^{3-n} \times b$, but approaches much nearer to the latter value than the former ; and if EC^{3-n} is very small in respect of AC^{3-n}, P is very small.

Remarks. If DF was not undercharged, it is certain that AB would be considerably more overcharged near the circumference of the circle than near the centre : for if the fluid was spread uniformly, a particle placed any where at a distance from the centre, as at N, would be repelled with considerably more force towards the circumference, than it would towards the centre. If the plates are very near together, and consequently DF nearly as much undercharged as AB is overcharged, AB will still be more overcharged near the circumference than near the centre, but the difference will not be near so great as in the former case : for, let NR be many times greater than CE, and NS less than CE ; and take Er and Es equal to CR and CS ; there can be no doubt, he thinks, but that the deficient fluid in DF will be lodged nearly in the same manner as the redundant fluid in AB ; and therefore the repulsion of the redundant fluid at R, on a particle at N, will be very nearly balanced by the attraction of the redundant matter at r, for R is not much nearer to N than r is ; but the repulsion of s will not be near balanced by that of s ; for the distance of s from N is much less than that of s. Let now a small circle, whose diameter is sT, be drawn round

the centre N, on the plane of the plate; as the density of the fluid is greater at T than at s, the repulsion of the redundant fluid within the small circle tends to impel the point N towards c; but as there is a much greater quantity of fluid between N and B, than between N and A, the repulsion of the fluid without the small circle tends to balance that; but the effect of the fluid within the small circle is not much less than it would be, if DF was not undercharged: whereas much the greater part of the effect of that part of the plate on the outside of the circle, is taken off by the effect of the corresponding part of DF : consequently the difference of density between T and s will not be near so great, as if DF was not undercharged. Hence he imagines, that if the two plates are very nearly together, the density of the redundant fluid near the centre will not be much less than the mean density, or s will not be much less than 1 ; moreover, the less the distance of the plates, the nearer will s approach to 1.

Corol. 2. Let now the body H consist of a circular plate, of the same size as AB, placed so, that the canal CG shall pass through its centre, and be perpendicular to its plane ; by the supposition, the force with which H repels the fluid in the canal CG, is the same with which a quantity of fluid, equal to B, spread uniformly over AB, would repel it in the contrary direction : therefore, if the fluid in the plate H was spread uniformly, the quantity of redundant fluid in it would be B; and if it was all collected in the circumference, would be $\dfrac{2B}{3-n}$; and therefore the real quantity will be greater than B, and less than $\dfrac{2B}{3-n}$.

Corol. 3. Therefore, if we suppose s to be equal to 1, the quantity of redundant fluid in AB will exceed that in the plate H, in a greater ratio than that of $\left(\dfrac{AC}{CE}\right)^{3-n} \times \dfrac{3-n}{4}$ to 1, and less than that of $\left(\dfrac{AC}{CE}\right)^{3-n} \times \frac{1}{2}$ to 1 : and from the preceding remarks it appears, that the real quantity of redundant fluid in AB can hardly be much greater than it would be if s was equal to 1.

Corol. 4. Hence, if the electric attraction and repulsion is inversely as the square of the distance, the redundant fluid in AB, supposing s to be equal to 1, will exceed that in the plate H, in a greater ratio than that of AC to 4CE, and less than that of AC to 2CE.

Corol. 5. Let now the body H consist of a globe, whose dia-

meter equals AB; the globe being situated in such a manner, that the canal cα, if continued, would pass through its centre; and let the electric attraction and repulsion be inversely as the square of the distance, the quantity of redundant fluid in the globe will be 2B; for the fluid will be spread uniformly over the surface of the globe, and its repulsion on the canal will be the same as if it was all collected in the centre of the sphere, and will therefore be the same with which an equal quantity, disposed in the circumference of AB, would repel it in the contrary direction, or with which half that quantity, or B, would repel it, if spread uniformly over the plate.

Corol. 6. Therefore, if *b* was equal to 1, the redundant fluid in AB would exceed that in the globe, in the ratio of AC to 4CE; and therefore it will in reality exceed that in the globe, in a rather greater ratio than that of AC to 4CE; but if the plates are very near together, it will approach very near to that ratio, and the nearer the plates are, the nearer it will approach to it.

Corol. 7. Whether the electric repulsion is inversely as the square of the distance or not, if the body H is as much undercharged, as it was before overcharged, AB will be as much undercharged as it was before overcharged, and DE as much overcharged as it was before undercharged.

Corol. 8. If the size and distance of the plates be altered, the quantity of redundant or deficient fluid in the body H remaining the same, it appears, by comparing this proposition with the 20th and 21st propositions, that the quantity of redundant and deficient fluid in AB, will be as AC^{n-1}

$\times \left(\frac{AC}{EC}\right)^{3-n}$, or as $\frac{AC^2}{EC^{3-n}}$, supposing the value of *b* to remain the same.

Prop. 23. Let AE, fig. 15, be a cylindric canal, infinitely continued beyond E; and let AF be a bent canal, meeting the other at A, and infinitely continued beyond F: let the section of this canal, in all parts of it, be equal to that of the cylindric canal, and let both canals be filled with uniform fluid of the same density: then the force with which a particle of fluid P, placed any where at pleasure, repels the whole quantity of fluid in AF, in the direction of the canal, is the same with which it repels the fluid in the canal AE, in the direction AE.— On the centre P, draw two circular arches BD and *bd*, infinitely near to each other, cutting AE in B and *β*, and AF in D and *δ*; and draw the radii P*b* and P*d*. As PB = PD, the force with which P repels a particle at B, in the direction B*β*, is to that with which it repels an equal particle at D, in the direction D*δ*, as $\frac{B b}{B \beta}$ to $\frac{D d}{D \delta}$, or as $\frac{1}{B \beta}$ to $\frac{1}{D \delta}$; and therefore the force with which it repels the whole fluid in B*β*, in the direction B*β*, is the

same with which it repels the whole fluid in D_δ, in the direction $D\delta$, that is in the direction of the canal; and therefore the force with which it repels the whole fluid in AE, in the direction of AE, is the same with which it repels the whole fluid in AF, in the direction of the canal.

Corol. If the bent canal ADF, instead of being infinitely continued, meets the cylindric canal in E, as in fig. 16, the repulsion of P on the fluid in the bent canal ADE, in the direction of the canal, will still be equal to its repulsion on that in the cylindric canal AE, in the direction of AE.

Prop. 24. If two bodies, for instance the plate AB, and the body H, of prop. 22, communicate with each other, by a canal filled with incompressible fluid, and are either over or undercharged; the quantity of redundant fluid in them will bear the same proportion to each other, whether the canal by which they communicate is straight or crooked, or into whatever part of the bodies the canal is inserted, or in whatever manner the two bodies are situated in respect of each other; provided that their distance is infinite, or so great that the repulsion of each body on the fluid in the canal shall not be sensibly less than if it was infinite.

Let the parallelograms AB and DF, fig. 17, represent the two plates, and H and L the bodies communicating with them: let now H be removed to h; and let it communicate with AB, by the bent canal gc; the quantity of fluid in the plates and bodies remaining the same as before; and let us, for the sake of ease in the demonstration, suppose the canal gc to be every where of the same thickness as the canal GC; though the proposition will evidently hold equally good, whether it is or not; then the fluid will still be in equilibrio. For let us first suppose the canal gc to be continued through the substance of the plate AB, to c, along the line crc; the part crc being of the same thickness as the rest of the canal, and the fluid in it of the same density; by the preceding proposition, the repulsion or attraction of each particle of fluid or matter in the plates AB and DF, on the fluid in the whole canal $crcg$, in the direction of that canal, is equal to its repulsion or attraction on the fluid in the canal CG, in the direction CG; and therefore the whole repulsion or attraction of the two plates on the canal $crcg$, is equal to their repulsion or attraction on CG: but as the fluid in the plate AB is in equilibrio, each particle of fluid in the part crc of the canal, is impelled by the plates, with as much force in one direction as the other; and consequently the plates impel the fluid in the canal cg, with as much force as they do that in the whole canal $crcg$, that is, with the same force that they impel the fluid in CG. In like manner the body h impels the fluid in cg, with the same force that H does the fluid in CG; and consequently h impels the fluid in cg, one way in the direction of the canal, with

the same force that the two plates impel it the contrary way ; and therefore the fluid in *cg* has no tendency to flow from one body to the other.

Corol. By the same method of reasoning, with the help of the corollary to the 23d proposition, it appears that if AB and H each communicate with a third body, by canals of incompressible fluid, and a communication is made between AB and H by another canal of incompressible fluid, the fluid will have no tendency to flow from one to the other through this canal ; supposing that the fluid was in equilibrio before this communication was made. In like manner, if AB and H communicate with each other, or each communicate with a third body, by canals of real fluid, instead of the imaginary canals of incompressible fluid used in these propositions, and a communication is also made between them by a canal of incompressible fluid, the fluid can have no tendency to flow from one to the other. The truth of the latter part of this corollary will appear by supposing an imaginary canal of incompressible fluid to be continued through the whole length of the real one.

Prop. 25. Let now a communication be made between the two plates AB and DF, by the canal NRS of incompressible fluid, of any length ; and let the body H and the plate AB be overcharged. It is plain that the fluid will flow through that canal from AB to DF. Now the whole force with which the fluid in the canal is impelled along it, by the joint action of the two plates, is the same with which the whole quantity of fluid in the canal CG or *cg* is impelled by them ; supposing the canal NRS to be every where of the same breadth and thickness as CG or *cg*. For suppose that the canal NRS, instead of communicating with the plate DF, is bent back just before it touches it, and continued infinitely along the line *ss* ; the force with which the two plates impel the fluid in *ss*, is the same with which they impel that in EL, supposing *ss* to be of the same breadth and thickness as EL ; and is therefore nothing ; therefore the force with which they impel the fluid in NRS, is the same with which they impel that in NRS*s* ; which is the same with which they impel that in CG.   •

Prop. 26. Let now *xyz* be a body of an infinite size, containing just fluid enough to saturate it ; and let a communication be made between *h* and *xyz*, by the canal *hy* of incompressible fluid, of the same breadth and thickness as *gc* or CC ; the fluid will flow through it from *h* to *xyz* ; and the force with which the fluid in that canal is impelled along it, is equal to that with which the fluid in NRS is impelled by the two plates.

If the canal *hy* is of so great a length, that the repulsion of *h* on it is the same as if it was continued infinitely, then the thing is evident ; but if it is not, let the canal *hy*, instead of com-

municating with xyz, so that the fluid can flow out of the canal into xyz, be continued infinitely through its substance, along the line yv: now it must be observed that a small part of the body xyz, namely, that which is turned towards h, will by the action of h on it, be rendered undercharged; but all the rest of the body will be saturated; for the fluid driven out of the undercharged part will not make the remainder, which is supposed to be of an infinite size, sensibly overcharged: now the force with which the fluid in the infinite canal hyv, is impelled by the body h and the undercharged part of xyz, is the same with which the fluid in gc is impelled by them; but as the fluid in all parts of xyz is in equilibrio, a particle in any part of yv cannot be impelled in any direction; and therefore the fluid in hy is impelled with as much force as that in hyv; and therefore the fluid in hy is impelled with as much force as that in gc; and is therefore impelled with as much force as the fluid in NRS is impelled by the two plates.

It perhaps may be asked, whether this method of demonstration would not equally tend to prove that the fluid in hy was impelled with the same force as that in NRS, though xyz did not contain just fluid enough to saturate it. He answers not; for this demonstration depends on the canal yv being continued, within the body xyz, to an infinite distance beyond any over or undercharged part; which could not be if xyz contained either more or less fluid than that.

Prop. 27. Let two bodies B and b, fig. 13, be joined by a cylindric or prismatic canal Aa, filled with real fluid; and not by an imaginary canal of incompressible fluid as in the 20th proposition; and let the fluid in it be in equilibrio: the force with which the whole or any given part of the fluid in the canal, is impelled in the direction of its axis, by the united repulsions and attractions of the redundant fluid or matter in the two bodies and the canal, must be nothing; or the force with which it is impelled one way in the direction of the axis of the canal, must be equal to that with which it is impelled the other way. For as the canal is supposed cylindric or prismatic, no particle of fluid in it can be prevented from moving in the direction of its axis, by the sides of the canal; and therefore the force with which each particle is impelled either way, in the direction of the axis, by the united attractions and repulsions of the two bodies and the canal, must be nothing, otherwise it could not be at rest; and therefore the force with which the whole, or any given part of the fluid in the canal, is impelled in the direction of the axis, must be nothing.

Corol. 1. If the fluid in the canal is disposed in such manner, that the repulsion or attraction of the redundant fluid or matter in it, on the whole or any given part of the fluid in the canal, has no tendency to impel it either way in the direction of the

axis; then the force with which that whole or given part is impelled by the two bodies, must be nothing; or the force with which it is impelled one way in the direction of the axis, by the body B, must be equal to that with which it is impelled in the contrary direction by the other body; but not if the fluid in the canal is disposed in a different manner.

Corol. 2. If the bodies, and consequently the canal, is over-charged; then, in whatever manner the fluid in the canal is disposed, the force with which the whole quantity of redundant fluid in the canal is repelled by the body B, in the direction Aa, must be equal to that with which it is repelled by B, in the contrary direction. For the force with which the redundant fluid is impelled in the direction Aa, by its own repulsion, is nothing; for the repulsion of the particles of any body on each other, have no tendency to make the whole body move in any direction.

Remarks. When Mr. C. first thought of the 20th and 22d propositions, he imagined that when two bodies were connected by a cylindric canal of real fluid, the repulsion of one body on the whole quantity of fluid in the canal, in one direction, would be equal to that of the other body on it, in the contrary direction, in whatever manner the fluid was disposed in the canal; and that therefore those propositions would have held good very nearly, though the bodies were joined by cylindric canals of real fluid; provided the bodies were so little over or undercharged, that the quantity of redundant or deficient fluid in the canal should be very small in respect of the quantity required to saturate it; and consequently that the fluid in it should be very nearly of the same density in all parts. But from the foregoing proposition it appears that he was mistaken, and that the repulsion of one body on the fluid in the canal, is not equal to that of the other body on it, unless the fluid in the canal is disposed in a particular manner: besides that, when two bodies are both joined by a real canal, the attraction or repulsion of the redundant matter or fluid in the canal, has some tendency to alter the disposition of the fluid in the two bodies; and in the 22d proposition, the canal co exerts also some attraction or repulsion on the canal EM, on all which accounts the demonstration of those propositions is defective, when the bodies are joined by real canals. He has good reason however to think, that those propositions actually hold good, very nearly, when the bodies are joined by real canals; and that, whether the canals are straight or crooked, or in whatever direction the bodies are situated in respect of each other: though he is by no means able to prove that they do: he there-fore chose still to retain those propositions, but to demonstrate them on this ideal supposition, in which they are certainly true,

in hopes that some more skilful mathematician may be able to show whether they really hold good or not.

What principally makes him think that this is the case, is, that as far as he can judge from some experiments he has made, the quantity of fluid in different bodies agrees very well with those propositions, on a supposition that the electric repulsion is inversely as the square of the distance. It should also seem from those experiments, that the quantity of redundant or deficient fluid in two bodies, bore very nearly the same proportion to each other, whatever is the shape of the canal by which they are joined, or in whatever direction they are situated in respect to each other.

Though the above proposition should be found not to hold good, when the bodies are joined by real canals, still it is evident, that in the 22d proposition, if the plates AB and DF are very near together, the quantity of redundant fluid in the plate AB will be many times greater than that in the body H, supposing H to consist of a circular plate of the same size, as AB, and DF will be nearly as much undercharged as AB is overcharged.

Sir Isaac Newton supposes that air consists of particles which repel each other with a force inversely as the distance: but it appears plainly from the foregoing pages, that if the repulsion of the particles was in this ratio and extended indefinitely to all distances, they would compose a fluid extremely different from common air. If the repulsion of the particles was inversely as the distance, but extended only to a given very small distance from their centres, they would compose a fluid of the same kind as air, in respect of elasticity, except that its density would not be in proportion to its compression; if the distance to which the repulsion extends, though very small, is yet many times greater than the distance of the particles from each other, it might be shown, that the density would be nearly as the square root of the compression. If the repulsion of the parts extend indefinitely, and was inversely as some higher power of the distance than the cube, the density of the fluid would be as some power of the compression less than three-fifths. The only law of repulsion, Mr. C. can think of, which will agree with experiment, is one which seems not very likely; namely, that the particles repel each other with a force inversely as the distance; but that, whether the density of the fluid is great or small, the repulsion extends only to the nearest particles; or, what comes to the same thing, that the distance to which the repulsion extends, is very small, and also is not fixed, but varies in proportion to the distance of the particles.

PART II.—*Containing a Comparison of the foregoing Theory with Experiment.*

§ 1. It appears from **experiment**, that some bodies suffer the electric fluid to pass with great readiness between their pores; while others will not suffer it to do so without great difficulty; and some hardly suffer it to do so at all. The first sort of bodies are called conductors, the others non-conductors. What this difference in bodies is owing to, Mr. C. does not pretend to explain. It is evident that the electric fluid in non-conductors may be considered as moveable, or answering to the definition given of that term immediately before prop. 1. As to the fluid contained in non-conducting substances, though it does not absolutely answer to the definition of immoveable, as it is not absolutely confined from moving, but only does so with great difficulty; yet it may in most cases be considered as such without sensible error. Air does in some measure permit the electric fluid to pass through it; though, if it is dry, it lets it pass but very slowly, and not without difficulty; it is therefore to be called a non-conductor.

It appears that conductors would readily suffer the fluid to run in and out of them, were it not for the air which surrounds them: for if the end of a conductor is inserted into a vacuum, the fluid runs in and out of it with perfect readiness; but when it is surrounded on all sides by the air, as no fluid can run out of it without running into the air, the fluid will not do so without difficulty. If any body is surrounded on all sides by the air, or other non-conducting substances, it is said to be insulated: if on the other hand it any where communicates with any conducting body, it is said to be not insulated. When he says that a body communicates with the ground, or any other body, he would be understood to mean that it does so by some conducting substance.

Though the terms positively and negatively electrified are much used, yet the precise sense in which they are to be understood, seems not well ascertained; namely, whether they are to be understood in the same sense in which he has used the words over or undercharged, or whether, when any number of bodies, insulated and communicating with each other by conducting substances, are electrified by means of excited glass, they are all to be called positively electrified (supposing, according to the usual opinion, that excited glass contains more than its natural quantity of electricity); even though some of them, by the approach of a stronger electrified body, are made undercharged. He uses the words in the latter sense; but as it will be proper to ascertain the sense in which he uses them more accurately, he gives the following definition. In order to judge whether any body, as A, is positively or nega-

tively electrified: suppose another body B, of any given shape and size, to be placed at an infinite distance from it, and from any other over or undercharged body; and let B contain the same quantity of electric fluid, as if it communicated with A by a canal of incompressible fluid: then if B is overcharged, he calls A positively electrified; and if it is undercharged, he calls A negatively electrified; and the greater the degree in which B is over or undercharged, the greater is the degree in which A is positively or negatively electrified.

It appears from the corol. to the 24th proposition, that if several bodies are insulated, and connected together by conducting substances, and one of these bodies is positively or negatively electrified, all the other bodies must be electrified in the same degree: for suppose a given body B to be placed at an infinite distance from any over or undercharged body, and to contain the same quantity of fluid as if it communicated with one of these bodies by a canal of incompressible fluid; all the rest of those bodies must, by that corol., contain the same quantity of fluid as if they communicated with B by canals of incompressible fluid; but yet it is possible that some of those bodies may be overcharged, and others undercharged: for suppose the bodies to be positively electrified, and let an overcharged body D be brought near one of them, that body will become undercharged, provided D is sufficiently overcharged, and yet by the definition it will still be positively electrified in the same degree as before.

Moreover, if several bodies are insulated and connected together by conducting substances, and one of these bodies is electrified by excited glass, there can be no doubt but they will all be positively electrified; for if there is no other over or undercharged body placed near any of these bodies, the thing is evident; and though some of these bodies may, by the approach of a sufficiently overcharged body, be rendered undercharged: yet he does not see how it is possible to prevent a body placed at an infinite distance, and communicating with them by a canal of incompressible fluid, from being overcharged. In like manner if one of these bodies is electrified by excited sealing wax, they will all be negatively electrified.

It is impossible for any body communicating with the ground to be either positively or negatively electrified; for the earth, taking the whole together, contains just fluid enough to saturate it, and consists in general of conducting substances; and consequently though it is possible for small parts of the surface of the earth to be rendered over or undercharged, by the approach of electrified clouds, or other causes; yet the bulk of the earth, and especially the interior parts, must be saturated with elec-

tricity. Therefore assume any part of the earth which is itself saturated, and is at a great distance from any over or undercharged part; any body communicating with the ground, contains as much electricity as if it communicated with this part by a canal of incompressible fluid, and therefore is not at all electrified.

If any body a, insulated and saturated with electricity, is placed at a great distance from any over or undercharged body, it is plain that it cannot be electrified ; but if an overcharged body is brought near it, it will be positively electrified ; for supposing a to communicate with any body b, at an infinite distance, by a canal of incompressible fluid, it is plain that unless b is overcharged, the fluid in the canal could not be in equilibrio, but would run from a to b. For the same reason a body insulated and saturated with fluid, will be negatively electrified if placed near an undercharged body.

§ 2. The phenomena of the attraction and repulsion of electrified bodies seem to agree exactly with the theory ; as will appear by considering the following cases. *Case* 1. Let two bodies, a and b, both conductors of electricity, and both placed at a great distance from any other electrified bodies, be brought near each other. Let a be insulated, and contain just fluid enough to saturate it ; and let b be positively electrified. They will attract each other ; for as b is positively electrified, and at a great distance from any overcharged body, it will be overcharged ; therefore, on approaching a and b to each other, some fluid will be driven from that part of a which is nearest to b to the farther part : but when the fluid in a was spread uniformly, the repulsion of b on the fluid in a was equal to its attraction on the matter in it ; therefore when some fluid is removed from those parts where the repulsion of b is strongest, to those where it is weaker, b will repel the fluid in a with less force than it attracts the matter ; and consequently the bodies will attract each other.

Case 2. If we now suppose that the fluid is at liberty to escape from out of a, if it has any disposition to do so, the quantity of fluid in it before the approach of b being still sufficient to saturate it ; that is, if a is not insulated and not electrified, b being still positively electrified, they will attract with more force than before : for in this case, not only some fluid will be driven from that part of a which is nearest to b to the opposite part, but also some fluid will be driven out of a. It must be observed, that if the repulsion of b on a particle at e, fig. 19, the farthest part of a, is very small in respect of its repulsion on an equal particle placed at d, the nearest part of a, the two bodies will attract with very nearly the same force, whether a is insulated or not ; but if the repulsion of b on a particle at e, is very near as great as on one at d, they will at-

tract with very little force if A is insulated. For instance, let a small overcharged ball be brought near one end of a long conductor not electrified ; they will attract with very near the same force, whether the conductor be insulated or not; but if the conductor be overcharged, and brought near a small unelectrified ball, they will not attract with near so much force, if the ball is insulated, as if it is not.

Case 3. If we now suppose that A is negatively electrified, and not insulated, it is plain that they will attract with more force than in the last case ; as A will be still more undercharged in this case than in the last.

N.B. In these three cases, we have not as yet taken notice of the effect which the body A will have in altering the quantity and disposition of the fluid in B ; but in reality this will make the bodies attract each other with more force than they would otherwise do ; for in each of these cases the body A attracts the fluid in B ; which will cause some fluid to flow from the farther parts of B to the nearer, and will also cause some fluid to flow into it, if it is not insulated, and will consequently cause B to act upon A with more force than it would otherwise do.

Case 4. Let us now suppose that B is negatively electrified ; and let A be insulated, and contain just fluid enough to saturate it ; they will attract each other ; for B will be undercharged ; it will therefore attract the fluid in A, and will cause some fluid to flow from the farthest part of A, where it is attracted with less force, to the nearest part, where it is attracted with more force ; so that B will attract the fluid in A with more force than it repels the matter.

Case 5 and 6. If A is now supposed to be not insulated and not electrified, B being still negatively electrified ; it is plain that they will attract with more force than in the last case: and if A is positively electrified, they will attract with still more force.

In these last three cases also, the effect which A has in altering the quantity and disposition of the fluid in B, tends to increase the force with which the two bodies attract.

Case 7. It is plain that a non-conducting body saturated with fluid, is not at all attracted or repelled by an over or undercharged body, until, by the action of the electrified body on it, it has either acquired some additional fluid from the air, or had some driven out of it, or till some fluid is driven from one part of the body to the other.

Case 8. Let us now suppose that the two bodies A and B are both positively electrified in the same degree. It is plain, that were it not for the action of one body on the other, they would both be overcharged, and would repel each other. But it may perhaps be said, that one of them as A may, by the action of the

other on it, be either rendered undercharged on the whole, or at least may be rendered undercharged in that part nearest to B ; and that the attraction of this undercharged part on a particle of the fluid in B, may be greater than the repulsion of the more distant overcharged part : so that on the whole the body A may attract a particle of fluid in B. If so, it must be affirmed that the body B repels the fluid in A ; for otherwise, that part of A which is nearest to B could not be rendered undercharged. Therefore, to obviate this objection, let the bodies be joined by the straight canal DC of incompressible fluid (fig. 19). The body B will repel the fluid in all parts of this canal ; for as A is supposed to attract the fluid in B, B will not only be more overcharged than it would otherwise be, but it will also be more overcharged in that part nearest to A, than in the opposite part. Moreover, as the near undercharged part of A is supposed to attract a particle of fluid in B, with more force than the more distant overcharged part repels it ; it must, a fortiori, attract a particle in the canal with more force than the other repels it ; therefore the body A must attract the fluid in the canal ; and consequently some fluid must flow from B to A, which is impossible ; for as A and B are both electrified in the same degree, they contain the same quantity of fluid as if they both communicated with a third body at an infinite distance, by canals of incompressible fluid ; and therefore by the corol. to prop. 24, if a communication is made between them by a canal of incompressible fluid, the fluid would have no disposition to flow from one to the other.

Case 9. But if one of the bodies, as A, is positively electrified, in a less degree than B, then it is possible for the bodies to attract each other ; for in this case the force with which B repels the fluid in A may be so great, as to make the body A either entirely undercharged, or at least to make the nearest part of it so much undercharged, that A shall on the whole attract a particle of fluid in B. It may be worth remarking, with regard to this case, that when two bodies, both electrified positively but unequally, attract each other, you may by removing them to a greater distance from each other, cause them to repel ; for as the stronger electrified body repels the fluid in the weaker with less force when removed to a greater distance, it will not be able to drive so much fluid out of it, or from the nearer to the farther part, as when placed at a less distance.

Case 10 *and* 11. By the same reasoning it appears, that if the two bodies are both negatively electrified in the same degree, they must repel each other : but if they are both negatively electrified in different degrees, it is possible for them to attract each other.

All these cases are exactly conformable to experiment.

Case 12. Let two cork balls be suspended by conducting threads, from the same positively electrified body, in such manner, that if they did not repel, they would hang close together: they will both be equally electrified, and will repel each other: let now an overcharged body, more strongly electrified than them, be brought under them; they will become less overcharged, and will separate less than before: on bringing the body still nearer, they will become not at all overcharged, and will not separate at all: and on bringing the body still nearer, they will become undercharged, and will separate again.

Case 13. Let all the air of a room be overcharged; and let two cork balls be suspended close to each other by conducting threads communicating with the wall. By prop. 15, it is highly probable that the balls will be undercharged; and therefore they should repel each other.

These last two cases are experiments of Mr. Canton's, and are described in Phil. Trans. 1753, p. 350, where are other experiments of the same kind, all readily explicable by the foregoing theory.

I have now, says Mr. C., considered all the principal or fundamental cases of electric attractions and repulsions which I can think of; all of which appear to agree perfectly with the theory.

§ 3. On the cases in which bodies receive electricity from or part with it to the air.

Lemma 1. Let the body A. fig. 6, either stand near some over or undercharged body, or at a distance from any. It seems highly probable, that if any part of its surface, as MN, is overcharged, the fluid will endeavour to run out through that part, provided the air adjacent to it is not overcharged.

For let G be any point in that surface, and P a point within the body, extremely near to it; it is plain that a particle of fluid at P, must be repelled with as much force in one direction as another (otherwise it could not be at rest) unless all the fluid between P and G is pressed close together, in which case it may be repelled with more force towards G, than it is in the contrary direction: now a particle at G is repelled in the direction PG, i. e. from P to G, by all the redundant fluid between P and G; and a particle at P is repelled by the same fluid in the contrary direction; so that as the particle at P is repelled with not less force in the direction PG than in the contrary, Mr. C. does not see how a particle at G can help being repelled with more force in that direction than the contrary, unless the air on the outside of the surface MN was more overcharged than the space between P and G.

In like manner, if any part of the surface *is* undercharged, the fluid will have a tendency to run in at that part from the

air. The truth of this is somewhat confirmed by the 3d problem ; as in all the cases of that problem, the fluid was shown to have a tendency to run out of the spaces AD and EH, at any surface which was overcharged, and to run in at any which was undercharged.

Corol. 1. If any body at a distance from other over or undercharged bodies, be positively electrified, the fluid will gradually run out of it from all parts of its surface into the adjoining air ; as it is plain that all parts of the surface of that body will be overcharged : and if the body is negatively electrified, the fluid will gradually run into it at all parts of its surface from the adjoining air.

Corol 2. Let the body A, fig. 6, insulated, and containing just fluid enough to saturate it, be brought near the overcharged body B ; that part of the surface of A which is turned towards B will, by prop. 2, be rendered undercharged, and will therefore imbibe electricity from the air ; and at the opposite surface RS, the fluid will run out of the body into the air.

Corol. 3. If we now suppose that A is not insulated, but communicates with the ground, and consequently that it contained just fluid enough to saturate it before the approach of B, it is plain that the surface MN will be more undercharged than before ; and therefore the fluid will run in there with more force than before ; but it can hardly have any disposition to run out at the opposite surface RS ; for if the canal by which A communicates with the ground is placed opposite to B, as in fig. 5, then the fluid will run out through that canal, till it has no longer any tendency to run out at RS ; and by the remarks at the end of prop. 27, it seems probable that the fluid in A will be nearly in the same quantity, and disposed nearly in the same manner, into whatever part of A the canal is inserted, by which it communicates with the ground.

Corol. 4. If B is undercharged, the case will be reverse ; that is, it will run out where it before ran in, and will run in where it before ran out.

As far as I can judge, these corollaries seem conformable to experiment: thus far is certain, that bodies at a distance from other electrified bodies receive electricity from the air, if negatively electrified, and part with some to it if positively electrified : and a body not electrified, and not insulated, receives electricity from the air if brought near an overcharged body, and loses some when brought near an undercharged body : and a body insulated and containing its natural quantity of fluid, in some cases receives, and in others loses electricity, when brought near an over or undercharged body.

§ 4. The well-known effects of points in causing a quick discharge of electricity seem to agree very well with this theory.

It appears from the 20th proposition, that if two similar bodies of different sizes are placed at a very great distance from each other, and connected by a slender canal, and overcharged, the force with which a particle of fluid, placed close to corresponding parts of their surface, is repelled from them, is inversely as the corresponding diameters of the bodies. If the distance of the two bodies is small, there is not so much difference in the force with which the particle is repelled by the two bodies ; but still, if the diameters of the two bodies are very different, the particle will be repelled with much more force from the smaller body than from the larger. It is true indeed, that a particle placed at a certain distance from the smaller body, will be repelled with less force than if it be placed at the same distance from the greater body ; but this distance is, he believes, in most cases pretty considerable; if the bodies are spherical, and the repulsion inversely as the square of the distance, a particle placed at any distance from the surface of the smaller body, less than a mean proportional between the radii of the two bodies, will be repelled from it with more force, than if it be placed at the same distance from the larger body.

Mr. C. thinks, therefore, that we may be well assured, that if two similar bodies are connected together by a slender canal, and are overcharged, the fluid must escape faster from a smaller body than from an equal surface of the larger ; but as the surface of the larger body is greatest, he does not know which body ought to lose most electricity in the same time ; and indeed it seems impossible to determine positively from this theory which should, as it depends in great measure on the manner in which the air opposes the entrance of the electric fluid into it. Perhaps in some degrees of electrification the smaller body may lose most, and in others the larger.

Let now ACB, fig. 18, be a conical point, standing on any body DAB, c being the vertex of the cone ; and let DAB be overcharged : Mr. C. imagines that a particle of fluid placed close to the surface of the cone, anywhere between b and c, must be repelled with at least as much, if not more force, than it would, if the part AabB of the cone was taken away, and the part acb connected to DAB by a slender canal; and consequently, from what has been said before, it seems reasonable to suppose that the waste of electricity from the end of the cone must be very great in proportion to its surface ; though it does not appear from this reasoning, whether the waste of electricity from the whole cone, should be greater or less than from a cylinder of the same base and altitude. All that has been here said relating to the flowing out of electricity from overcharged bodies, holds equally true with regard to the flowing in of electricity into undercharged bodies.

But a circumstance which, he believes, contributes as much

as anything to the quick discharge of electricity from points, is the swift current of air caused by them, and taking notice of by Mr. Wilson and Dr. Priestley (vide Priestley, p. 117 and 591); and which is produced in this manner. If a globular body ABD is overcharged, the air close to it, all round its surface, is rendered overcharged, by the electric fluid, which flows into it from the body; it will therefore be repelled by the hody; but as the air all round the body is repelled with the same force, it is in equilibrio, and has no tendency to fly off from it. If now the conical point ACB be made to stand out from the globe, as the fluid will escape much faster in proportion to the surface from the end of the point, than from the rest of the body, the air close to it will be much more overcharged than that close to the rest of the body; it will therefore be repelled with much more force; and consequently a current of air will flow along the sides of the cone, from B towards C; by which means there is a continual supply of fresh air, not much overcharged, brought in contact with the point; whereas otherwise the air adjoining to it would be so much overcharged, that the electricity would have but little disposition to flow from the point into it.

The same current of air is produced in a less degree, without the help of the point, if the body, instead of being globular, is oblong or flat, or has knobs on it, or otherwise formed in such manner as to make the electricity escape faster from some parts of it than the rest.

In like manner, if the body ABD be undercharged, the air adjoining to it will also be undercharged, and will therefore be repelled by it; but as the air close to the end of the point will be more undercharged than that close to the rest of the body, it will be repelled with much more force; which will cause exactly the same current of air, flowing the same way, as if the body was overcharged; and consequently the velocity with which the electric fluid flows into the body, will be very much increased. Mr. C. believes indeed that it may be laid down as a constant rule, that the faster the electric fluid escapes from any body when overcharged, the faster will it run into that body when undercharged.

Points are not the only bodies which cause a quick discharge of electricity; in particular, it escapes very fast from the ends of long slender cylinders; and a swift current of air is caused to flow from the middle of the cylinder towards the end: this will easily appear by considering, that the redundant fluid is collected in much greater quantity near the ends of the cylinders, than near the middle. The same thing may be said, but he believes in a less degree, of the edges of thin plates.

What has just been said concerning the current of air, serves to explain the reason of the revolving motion of Dr. Hamilton's

and Mr. Kinnersley's bent pointed wires, vide Phil Trans. vol. 51, p. 905, and vol. 53, p. 86 ; also Priestley, p. 429 : for the same repulsion which impels the air from the thick part of the wire towards the point, tends to impel the wire in the contrary direction.

It is well known, that if a body в is positively electrified, and another body A, communicating with the ground, be then brought near it, the electric fluid will escape faster from в, at that part of it which is turned towards A, than before.   This is plainly conformable to theory ; for as A is thus rendered under-charg d, в will in its turn be made more overcharged, in that part of it which is turned towards A, than it was before.   But it is also well known, that the fluid will escape faster from в, if A be pointed, than if it be blunt ; though в will be less over-charged in this case than in the other ; for the broader the sur-face of A, which is turned towards в, the more effect will it have in increasing the overcharge of в.   The cause of this phenome-non is as follows :

If A is pointed, and the pointed end turned towards в, the air close to the point will be very much undercharged, and there-fore will be strongly repelled by A, and attracted by в, which will cause a swift current of air to flow from it towards в, by which means a constant supply of undercharged air will be brought in contact with в, which will accelerate the discharge of electricity from it in a very great degree : and moreover, the more pointed A is, the swifter will be this current.   If, on the other hand, that end of A which is turned towards в, is so blunt, that the electricity is not disposed to run into A faster than it is to run out of в, the air adjoining to в may be as much over-charged as that adjoining to A is undercharged ; and therefore may, by the joint repulsion of в and attraction of A, be impelled from в to A, with as much or more force than the air adjoining to A is impelled in the contrary direction ; so that what little current of air there is may flow in the contrary direction.

It is easy applying what has been here said to be the case in which в is negatively electrified.

§ 5. In the paper of Mr. Canton's, quoted in the 2nd section, and in a paper of Dr. Franklin's, (Phil. Trans., 1755, p. 300, and Franklin's letters, p. 155) are some remarkable experi-ments, showing that when an overcharged body is brought near another body, some fluid is driven to the farther end of this body, and also some driven out of it, if it is not insulated. The experiments are all strictly conformable to the 11th, 12th, and 13th propositions, but it is needless to point out the agreement, as the explanation given by the authors does it sufficiently.

§ 6. *On the Leyden Vial.*—The shock produced by the Leyden vial, seems owing only to the great quantity of redundant fluid

collected on its positive side, and the great deficiency on its ne-
gative side ; so that if a conductor was prepared of so great a
size, as to be able to receive as much additional fluid by the
same degree of electrification, as the positive side of a Leyden
vial, and was positively electrified in the same degree as the
vial, he does not doubt but what as great a shock would be pro-
duced by making a communication between this conductor and
the ground, as between the two surfaces of the Leyden vial,
supposing both communications to be made by canals of the
same length and same kind.

It appears plainly from the experiments which have been
made on this subject, that the electric fluid is not able to pass
through the glass ; but yet it seems as if it was able to pene-
trate without much difficulty to a certain small depth, perhaps
he might say an imperceptible depth, with the glass ; as Dr.
Franklin's analysis of the Leyden vial shows that its electricity
is contained chiefly in the glass itself, and that the coating is
not greatly over or undercharged.

It is well known that glass is not the only substance which
can be charged in the manner of the Leyden vial ; but that the
same effect may be produced by any other body, which will
not suffer the electricity to pass through it.

  * Hence the phenomena of the vial seem easily explicable by
means of the 22nd proposition.　For let ACGM, fig. 20, represent
a flat piece of glass, or any other substance which will not suf-
fer the electric fluid to pass through it, seen edgewise ; and let
Bbdd D, and EefF, or Bd and Ef, as he calls them for shortness, be
two plates of conducting matter of the same size, placed in
contact with the glass opposite to each other ; and let Bd be
positively electrified ; and let Ef communicate with the ground ;
and let the fluid be supposed either able to enter a little way
into the glass, but not to pass through it, or unable to enter it
at all ; and if it is able to enter a little way into it, let bbd, or
or bb, as he calls it, represent that part of the glass into which
the fluid can enter from the plate Bd, and eg that which the fluid
from Ef can enter.　By the abovementioned proposition, if bc,
the thickness of the glass, is very small in respect of bd, the
diameter of the plates, the quantity of redundant fluid forced
into the space Bd, or Bb, that is, into the plate Bd, if the fluid
is unable to penetrate at all into the glass, or into the plate Bd,
and the space bb together, if the fluid is able to penetrate into
the glass, will be many times greater than what would be forced

  * The following explication is strictly applicable only to that sort of
of Leyden vial, which consists of a flat plate of glass or other matter.　It
is evident however, that the result must be nearly of the same kind, though
the glass is made into the shape of a bottle as usual, or into any other form ;
but he proposes to consider those sort of Leyden vials more particularly in
a future paper.—*Orig.*

into it by the same degree of electrification if it had been placed by itself; and the quantity of fluid driven out of E?, will be nearly equal to the redundant fluid in B^b.

If a communication be now made between B^b and E?, by the canal NRS, the redundant fluid will run from B^b to E? ; and if in its way it passes through the body of any animal, it will, by the rapidity of its motion, produce in it that sensation called a shock.

It appears from the 26th proposition, that if a body of any size was electrified in the same degree as the plate Bd, and a communication was made between that body and the ground, by a canal of the same length, breadth, and thickness, as NRS ; that then the fluid in that canal would be impelled with the same force as that in NRS, supposing the fluid in both canals to be incompressible ; and consequently, as the quantity of fluid to be moved, and the resistance to its motion, are the same in both canals, the fluid should move with the same rapidity in both : and he sees no reason to think that the case will be different, if the communication is made by canals of real fluid.

Therefore what was said in the beginning of this section, namely, that as great a shock would be produced by making a communication between the conductor and the ground, as between the two sides of the Leyden vial, by canals of the same length and same kind, seems a necessary consequence of this theory ; as the quantity of fluid which passes through the canal, is, by the supposition, the same in both; and there is the greatest reason to think, that the rapidity with which it passes will be nearly, if not quite the same, in both.   Mr. C. hopes soon to be able to say whether this agrees with experiment as well as theory.

It may be worth observing, that the longer the canal NRS is, by which the communication is made, the less will be the rapidity with which the fluid moves along it; for the longer the the canal is, the greater is the resistance to the motion of the fluid in it ; whereas the force with which the whole quantity of fluid in it is impelled, is the same, whatever be the length of the canal.   Accordingly, it is found in melting small wires, by directing a shock through them, that the longer the wire the greater charge it requires to melt it.

As the fluid in B is attracted with great force by the redundant matter in E?, it is plain that if the fluid is able to penetrate at all into the glass, great part of the redundant fluid will be lodged in b_b ; and in like manner there will be a great deficiency of fluid in c_b.   But in order to form some estimate of the proportion of the redundant fluid, which will be lodged in b_b, let the communication between Ef and the ground be taken away, as well as that by which Bd is electrified ; and let so

much fluid be taken from B^3, as to make the redundant fluid in
it equal to the deficient fluid in Ef. If we suppose that all the
redundant fluid is collected in b^3, and all the deficient in ef, so
as to leave Bd and Ef saturated; then, if the electric repulsion
is inversely as the square of the distance, a particle of fluid
placed any where in the plane bd, except near the extremities b
and d, will be attracted with very near as much force by the
redundant matter in ef as it is repelled by the redundant fluid in
b^3; but if the repulsion is inversely as some higher power than
the square, it will be repelled with much more force by b^3, than
it is attracted by ef, provided the depth b^3 is very small in re-
spect of the thickness of the glass; and if the repulsion is
inversely as some lower power than the square, it will be attrac-
ted with much more force by ef, than it is repelled by b^3.
Hence it follows, that if the depth to which the fluid can pene-
trate, is very small in respect of the thickness of the glass, but
yet is such that the quantity of fluid naturally contained in b^3,
or ef, is considerably more than the redundant fluid in B^3; then,
if the repulsion is inversely as the square of the distance, almost
all the redundant fluid will be collected in b^3, leaving the plate
Bd not very much overcharged; and in like manner Ef will be
not very much undercharged : if the repulsion is inversely as
some higher power than the square, Bd will be very much over-
charged, and Ef very much undercharged ; and if the repulsion
is inversely as some lower power than the square, Bd will be
very much undercharged, and Ef very much overcharged.

Suppose now the plate Bd to be separated from the plate of
glass, still keeping it parallel to it, and opposite to the same part
of it that it before was applied to ; and let the repulsion of the
particles be inversely as some higher power of the distance than
the square. When the plate is in contact with the glass, the
repulsion of the redundant fluid in that plate, on a particle in
the plane bd, id est, the inner surface of the plate, must be equal
to the excess of the repulsion of the redundant fluid in b^3 on it,
above the attraction of Ef on it ; therefore, when the plate Bd
is removed ever so small a distance from the glass, the repulsion
of the redundant fluid in the plate, on a particle in the inner
surface of that plate, will be greater than the excess of the re-
pulsion of b^3 on it, above the attraction of Ef ; for the repulsion
of b^3 will be much more diminished by the removal, than the
attraction of Ef : consequently some fluid will fly from the plate
to the glass, in the form of sparks : so that the plate will not be
so much overcharged when removed from the glass, as it was
when in contact with it. Mr. C. imagines however, that it
would still be considerably overcharged.

If one part of the plate is separated from the glass before the
rest, as must necessarily be the case if it consists of bending
materials, he guesses it would be at least as much, if not more,

overcharged, when separated, as if it is separated all at once.
In like manner, it should seem that the plate Ef will be consi-
derably undercharged, when separated from the glass, but not
so much so as when in contact with it.   From the same kind of
reasoning he concludes, that if the repulsion is inversely as
some lower power of the distance than the square, the plate Dd
will be considerably undercharged, and Ef considerably over-
charged, when separated from the glass, but not in so great a
degree as when they are in contact with it.

§ 7. There is an experiment of Mr. Wilke and Æpinus, re-
lated by Dr. Priestley, p. 258, called by them, electrifying a plate
of air ; it consisted in placing two large boards of wood, covered
with tin plates, parallel to each other, and at some inches asun-
der.   If a communication was made between one of these and
the ground, and the other was positively electrified, the former
was undercharged ; the boards strongly attracted each other ;
and, on making a communication between them, a shock was
felt like that of the Leyden vial.

Mr. C. is uncertain whether, in this experiment, the air con-
tained between the two boards is very much overcharged on
one side, and very much undercharged on the other, as is the
case with the plate of glass in the Leyden vial ; or whether the
case is, that the redundant or deficient fluid is lodged only in
the two boards, and that the air between them serves only to
prevent the electricity from running from one board to the
other ; but whichever of these is the case, the experiment is
equally conformable to the theory.

It must be observed, that a particle of fluid, placed between
the two plates, is drawn towards the undercharged plate, with a
force exceeding that with which it would be repelled from the
overcharged plate, if it was electrified with the same force, the
other plate being taken away, nearly in the ratio of twice the
quantity of redundant fluid actually contained in the plate, to
that which it would contain if electrified with the same force by
itself ; so that, unless the plate is very weakly electrified, or
their distance is very considerable, the fluid will be apt to fly
from one to the other, in the form of sparks.

§ 8. Whenever any conducting body, as A, communicating
with the ground, is brought sufficiently near an overcharged
body B, the electric fluid is apt to fly through the air from B to
A, in the form of a spark : the way by which this is brought
about seems to be this.   The fluid placed anywhere between
the two bodies, is repelled from B towards A, and will conse-
quently move slowly through the air from one to the other :
now it seems as if this motion increased the elasticity of the air,
and made it rarer : this will enable the fluid to flow in a swifter
current, which will still further increase the elasticity of the air,

till at last it is so much rarefied, as to form very little opposition to the motion of the electric fluid, on which it flies in an uninterrupted mass from one body to the other.

In the same manner may the electric fluid pass from one body to another, in the form of a spark, if the first body communicates with the ground, and the other body is negatively electrified, or in any other case in which one body is strongly disposed to part with its electricity to the air, and the other is strongly disposed to receive it.

In like manner, when the electric fluid is made to pass through water, in the form of a spark, as in Signor Beccaria's* and Mr. Lane's† experiments, Mr. C. imagines that the water, by the rapid motion of the electric fluid through it, is turned into an elastic fluid, and so much rarefied as to make very little opposition to its motion; and when stones are burst or thrown out from buildings struck by lightning, in all probability that effect is caused by the moisture in the stone, or some of the stone itself, being turned into an elastic fluid.

It appears plainly, from the sudden rising of the water, in Mr. Kinnersley's electrical air thermometer,‡ than when the electric fluid passes through the air, in the form of a spark, the air in its passage is either very much rarefied, or entirely displaced: and the bursting of the glass vessels, in Beccaria's and Lane's experiments, shows that the same thing happens with regard to the water, when the electric fluid passes through it in the form of a spark. Now, Mr. C. saw no means by which the displacing of the air or water can be brought about, but by supposing its elasticity to be increased, by the motion of the electric fluid through it, unless you suppose it to be actually pushed aside, by the force with which the electric fluid endeavours to issue from the overcharged body: but he can by no means think that the force with which the fluid endeavours to issue, in the ordinary cases in which electric sparks are produced, is sufficient to overcome the pressure of the atmosphere, much less than it is sufficient to burst the glass vessels in Beccaria's and Lane's experiments.

The truth of this is confirmed by prop. 16. For, let an undercharged body be brought near to, and opposite to the end of a long cylindrical body, communicating with the ground; by that proposition the pressure of the electric fluid against the base of the cylinder, is scarcely greater than the force with which the two bodies attract each other, provided that no part of the cylinder is undercharged; which is very unlikely to be the case,

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* Elettricismo artificiale e naturale, p. 110.   Priestley, p. 209.

† Phil. Trans. 1767, p. 451.

‡ Phil. Trans. 1763, p. 81.   Priestley, p. 216.

if the electric repulsion is inversely as the square of the dis-
tance, as he has great reason to believe it is; and consequently,
if the spark was produced by the air being pushed aside, by
the force with which the fluid endeavours to issue from the
cylinder, no sparks should be produced, unless the electricity
was so strong, that the force with which the bodies attracted
each other was as great as the pressure of the atmosphere against
the base of the cylinder ; whereas it is well known, that a spark
may be produced, when the force, with which the bodies attract,
is very trifling in respect of that.

We may frequently observe, in discharging a Leyden vial,
that if the two knobs are approached together very slowly, a
hissing noise will be perceived before the spark ; which shows,
that the fluid begins to flow ·from· one knob to the other,
before it passes in the form of a spark ; and therefore serves to
confirm the truth of the opinion, that the spark is brought about
in the gradual manner here described.

ROYAL VICTORIA GALLERY OF PRACTICAL
SCIENCE, MANCHESTER.

Conversazione held on the Thursday, January 17th, 1841.

W. Fairbairn, Esq., in the Chair.

The Third Lecture on the Nutrition of Plants. By J. A. RAN-
SOME, Esq., Lecturer on Surgery, &c., at the Royal School
of Medicine, &c., Pine-street ; Secretary to the Literary and
Philosophical Society, Manchester.

Mr. Ransome opened the proceedings, by observing that on two
former occasions he had had an opportunity of bringing before
the meeting the new views entertained by Liebeg in relation to
the process of vegetable nutrition ; and, having fully developed
these views, there still remained an opportunity of making a
practical application of the subject. On the present occasion he
wished for an audience exclusively of gentlemen, in order that
he might the more readily enter into details, which he could
hardly submit for discussion in a mixed assembly. In the an-
nouncement of the subject for the evening, he had used a new
term—the art of culture—which he considered an episode to the
general course of lectures, which he was bound to deliver to
them. The art of culture implied that some violence, gentle or
otherwise, was done to assist natural processes—that artificial
means were employed to produce certain effects. The agricul-

turist availed himself of the guidance of nature, to produce certain effects, which nature herself could not effect, were his interference wanting. The main end of vegetable existence seemed to be the reproduction of means by which the species might be maintained. A few seeds, maturated by the parent plant, falling into a suitable soil, would have answered this object, but this was not sufficient. It had been wisely ordered that where a higher class of beings depended on vegetables, for life and support, the reproduction should be made as abundant as possible. It was intended that the increase should be amply sufficient for the maintenance of the superior orders of creation. That abundance had been, up to the present time, materially increased by artificial means, and it was still doubtful whether it had as yet reached its maximum. The object of Professor Liebeg, was to show how, by the application of certain principles, the production of a still greater abundance might be obtained. It was an object which every good citizen must have at heart ; for it had been well said that a man deserved well of his country who could make two ears of wheat grow where there was only one before. (Applause.) He (Mr. R.) considered himself merely as the expositor of Professor Liebeg, for his own knowledge of agriculture was slight, and his authority of little weight ; but he would endeavour, as much as possible, to deliver the substance of Professor Liebeg's book extemporaneously, as he (Mr. R.) knew it would be more interesting to the audience than reading from a book. They had seen, on former occasions, that vegetables were compounded of four principal elements, carbon, oxygen, hydrogen, and nitrogen ; and in addition to these there were certain traces of inorganic constituents which were found to follow the exact law of definite proportions, such as potash, soda, lime, magnesia, &c. Having pointed out the existence of carbon, oxygen, hydrogen, and nitrogen in plant matter, Mr. Ransome showed that the carbonic acid was furnished to the plant through the medium of the air and water, and the leaves, and absorbed into the spongy part of the roots. The hydrogen and oxygen were derived from the rains furnished by dew or irrigation. The atmospheric air contained about 79 per cent. of nitrogen, but it was not probable that this was assimilated, but that it was furnished by the ammonia in the atmosphere or in the soil. The inorganic products, such as potash, soda, lime, magnesia, &c. he had shown on a former occasion to have been derived from the disintegration of the rocks of which the soil was composed. If they took a virgin soil, such as the first colonists of America were so fortunate as to discover, they would find that the seeds of plants sown in it would flourish according to their natural habitudes—that is, each would select a soil suitable to itself ; if sown in a wrong situation it would fail, but where it fell into ground suitable to it it would flourish. In South America they were able to ac-

complish a task which the English agriculturists could not ; they
grew for upwards of 100 years successive crops of tobacco plants,
but after 100 years the soil ceased to yield this produce ; it was
exhausted—of what ? not of carbon, that was provided from
the air ; water might be wanting in some localities, but where
rain was not, there was abundance of dew. Ammonia also
pervaded the atmosphere ; it was shewn in that room that it
could be brought down by showers of rain. The exhaustion of
the soil might in part depend on its being robbed of the nitrogen,
or the ammonia that furnished it. The abstraction of the nitro-
gen would impoverish the soil. The impoverishment of the
soil might also be attributed to the abstraction of the inorganic
matter. The question then was how the exhaustion could be
prevented—how the carbon, hydrogen, oxygen, and nitrogen,
with the inorganic constituents, such as soda, lime, potash, mag-
nesia, &c. could be preserved ? It had been shewn that plants
would grow in the air without soil, yet these were exceptions :
most plants required soil, and in that soil there was a quantity
of vegetable mould to which the name *humus* had been given
by vegetable physiologists. The *humus* that exists in the soil,
when brought into a state called coal of *humus*, was, in fact, a
sponge which absorbed the oxygen, and formed carbonic acid,
and then gave out the latter to the plant. We might expect to
find in every good soil this vegetable mould or *humus*, and this
ingredient increases after each crop. It would be considered
bad husbandry for a man to grow two crops of wheat in two
successive years ; why ? Some supposed the soil would be ex-
hausted. But *Decandolle* and *Macaire*, who experimented on
this subject, came to the conclusion, that although the plant
takes from the soil whatever is presented to it in a state of
solution, it also excretes back to it those matters not useful for
its own assimilation. They also found that it would ensure the
destruction of a plant, to put it into the water where another
plant of the same kind had grown and left its excrementitious
matter, showing that the excrement of one plant is the poison of
another, of the same description. But plants of another species
would flourish if put into this excrementitious matter, which
also showed that the excrement of one plant contained the nu-
triment of another. From these experiments *Decandolle* and
Macaire were quite justified in coming to the conclusion that
something was thrown off by one crop which would be injurious
to another in the following year ; but if that matter could be ex-
posed to the action of oxygen as it exists in the air or water—
particularly if the earth were turned up for tillage, so as to ex-
pose a fresh surface, this excrementitious matter would under-
go decompostion, and be reduced to a state of coal of *humus* or
vegetable mould. This *humus* had another property, namely,
that of assuming the form of humic acid, in which state it formed
slightly soluble compounds with potash, lime, &c. This was

the dogma of the old physiologists, that humic acid united with lime, &c., became soluble and was taken up by the spongioles of the roots. Now it was found that the vegetable earth which contained this humic acid was injurious to the growth of plants, and that the earth which is mixed with it is unfit to be used until it is so thoroughly washed as to have lost the power of colouring the water.—From these experiments it appeared that cultivation did not diminish the quantity of carbonaceous matter in the soil, for each crop left behind it more than the preceding one exhausted—exhaustion therefore could not proceed from the diminution of carbon. The atmosphere contained a sufficient quantity of ammonia from which the plant could attract its nutriment, in a state of nature. Many of the wild plants did not require ammonia, but wheat and other plants used as nourishment in this country required a large proportion of it. Wheat possessed more nutritious qualities, in proportion to the nitrogen it contained than other plants. The supply of nitrogen, therefore, should be regarded as an artificial means of increasing the produce of the soil within certain limits. The next point of inquiry would be to ascertain from what source the loss of potash, magnesia, lime, &c., each succeeding crop takes from the soil, might be restored. It was obvious to all that, if in a crop so many parts of earthy and alkaline matter were abstracted from the soil, so much less would be left for the succeeding crop. They had then to look by what means this loss might be supplied. He would first begin with nitrogen. It was found that the destination of plants, besides the reproduction, was generally for the stomachs of animals. In fact, all animals derived the elements of their growth from plants. The animal takes this vegetable food into his stomach, part of it is assimilated for the growth of his body, and part is voided as excrement, the law of the case being, that whatever was solid and insoluble passed off in excrement, whatever was soluble passed in the liquid, and whatever was volatile passed of in exhalations, either from the lungs or skin. Nitrogen was a volatile body, and, in its compound state, ammonia was highly volatile, but there was a beautiful provision by which the nitrogen in combination was kept solid for the purpose of agriculture. The excretion which contained the largest proportion of nitrogen was human urine. According to an analysis of Berzelius, 1000 parts of human urine contained :—

Urea	30.10
Free lactic acid, *lactate of ammonia,* and animal matter, not separable from them......	17.14
Uric acid	1.00
Mucus of the bladder	0.32
Sulphate of potash	3.71
Sulphate of soda	3.16

Phosphate of soda	2.94
Phosphate of ammonia	1.65 •
Chloride of sodium	4.45
Muriate of ammonia.......... • ......•........	1.50
Phosphates of magnesia and lime	1.00
Siliceous earth	0.03
Water.......................	933.00
	1000.00

Those marked in italics were all the elements in the urine that contained nitrogen. If the urea were allowed to purify spontaneously—that is, to pass into that state in which it is used as manure, all the urea in combinaton with lactic acid would be converted into lactate of ammonia, and that which was free into volatile carbonate of ammonia. In a necessary, if the muriatic acid were exposed, white fumes would indicate the combination of the ammonia and the acid; for when the urea undergoes this change, it is converted into carbonate of ammonia and free ammonia, both volatile products. Liebeg says, " If a basin filled with concentrated muriatic acid is placed in a common necessary, so that its surface is in free communication with the vapours which rise from below, it becomes filled after a few days with crystals of muriate. The ammonia, the presence of which the organs of smell amply testify, combines with the muriatic acid, and loses entirely its volatility, and thick clouds, or fumes of the salt newly formed, hang over the basin." In dung reservoirs, well constructed and protected from evaporation, this carbonate of ammonia was retained in the state of solution, and when the urine was spread over the land, a part of the ammonia would escape with the water which evaporated, but another portion would be absorbed by the soil, if it contained either alumina, or iron, or gypsum. But, in general, only the muriate, phosphate, and lactate of ammonia remained in the ground. It was these alone, therefore, which enabled the soil to exercise a direct influence on plants during the progress of their growth, and a very small proportion of them escaped absorption by the roots. Thus we had one element, and by and by he would allude to the neglect of the agriculturists of this country in not taking better care of what was so very valuable, but which they were apt to consider not worth keeping. In Flanders and China this was justly considered the best manure; but our farmers, by their neglect, benefitted their neighbours as much as themselves, by not preserving the ammoniacal gas from escaping. Among the inorganic elements contained in urine, were sulphate of potash, soda, chloride of sodium, phosphate of magnesia, alum, and siliceous earth. Might they not see in this, at once, the stock which supplied the waste produced in the soil by the growth of plants, and that these elements, taken from the soil into the body, were found of no use, and returned back to the soil again. In the solid excre-

ments of animals—in the cow, the sheep, the horse, and man him-self, was found a greater or less proportion of azote ; perhaps in the cow there was the least proportion, in the horse and sheep not much more, and in the body of man the largest proportion of all. In the cow, the proportion would be one-half per cent. ; in the horse, one per cent. ; and in man, from the peculiar nature of his food, it amounted to much more ; and in the pig there was most of all.   The horse, cow, and sheep, devoured the grass as it grew in the field ; they took in the azote it contained, and assimi-lated what was necessary for themselves, and gave off the surplus in various ways.   The cow gave it off in her milk or urine, but more especially in the milk, which was highly nutritious.   It passed off from the surface as well as in the excrement of man, in whom the proportion varied from one half to five per cent. But even here there was a difference between the man who lived in the country, and the man who resided in town.   The former lived chiefly on farinaceous food, being unable to procure animal food, and therefore there was a smaller proportion in his excre-ments than in those of the town-fed man, who consumed a greater proportion of animal food.   The town-fed man took in a greater quantity of azote than his body required, which was afterwards given out in solid excrement and urine.   Of all the manures de-rived from towns, the excrement of man was considered the most nourishing.   But there were other sources from which the inor-ganic elements of plants could be derived.   If, for example, we wanted to supply the land with a sufficiency of phosphates ; we had the choice of ashes of oak, beech, pine, fir, or Norway pine. In the ashes of oak there were only the traces of phosphate, in beech there was twenty per cent of the phosphate of soda, pine and fir had from nine to fifteen per cent., but the Norway pine had only 1.8 of phosphoric acid.   With every 100lbs. of the lixi-viated ashes of the beech spread over the soil, we could furnish as much phosphate as 460lbs. of fresh human excrement could yield. According to the analysis of De Saussure, 100 parts of the ashes of the grain of wheat contained 32 parts of soluble, and 44.5 of insoluble phosphates, in all 76.5 parts.   The ashes of wheat straw contanied 11.5 per cent. of the same salts ; hence, with every 100lbs. of the ashes of the beech, a field might be supplied with phosphoric acid sufficient for the production of 3820lbs. of straw, and of from 15,000lbs. to 18,000lbs. of corn.   It was also found that when an animal died, and all his secretions ceased, the bones were still useful as manure, and they had this advantage, that they could be kept almost any number of years.   Bones contained phosphate of lime and phosphate of magnesia ; they also contained gelatine, and albumen according to others, and this was rich in ni-trogen.   Eight pounds of bones contained as much of the inor-ganic elements as 1000lbs. of hay or wheat straw, or 4000lbs. of the grain of wheat or oats.   The advantages of bone manure were pretty well appreciated in this country, and in Flanders and China.

With respect to the excrements of cows, black cattle, and sheep, these contained phosphate of lime, common salt, and silicate of potash. The nature of the excrements were found to vary with the elements which the animal had taken in, and always consisted of the elements which the body found unfit for its own purposes. Human fœces, or excrements according to an exact analysis furnished by Berzelius, contained, besides three-fourths of their weight in water, nitrogen in very variable quantity, namely, in the minimum $1\frac{1}{4}$, in the maximum five per cent. In all cases, however, they were richer in this element than the excrements of most other animals. Berzelius obtained by the incineration of 100 parts of dried excrements, 15 parts of ashes, which were principally composed of the phosphates of lime and magnesia, both of which enter into the composition of the husk of wheat. With respect to vegetable feeders, such as the horse, cow, and sheep, the excrements of the two latter restored to the land the silicate of potash, and salts of phosphoric acid, which was removed from it in the shape of corn, roots, or grain, and the excrements of the horse gave back to the soil phosphate of magnesia, and silicate of patash; and the straw which they used as a litter, restored a further quantity of silica of potash and phosphates, which, if the straw were putrified, would be exactly in the same condition in which they were before being assimilated. It was evident, therefore, that a farm containing a certain number of human beings, and a certain portion of which was allotted for the grazing of cattle and the growing of corn, would be very little impoverished if the excrements of the human beings and cattle were carefully distributed over it every year. In the case of a number of young children being born and reared on the farm, the alteration in the land would still be very little; for, supposing the children continued there till they reached maturity, and consequently assimilated a great proportion of the inorganic matter, let them die and be given back to the ground, and then it would contain as much as it did at first. This shewed the necessity of using over again the excrements as manure for the purpose of reproduction; and it also shewed the fallacy of much that had been said about excessive population and a deficiency of food; for the supply of food seemed to be always in proportion to the increase of population, (within certain limits,) and the means of returning to the soil the necessary elements.

The Chairman.—In this case do you suppose that the inhabitants consume all the produce?

Mr. Ransome.— Yes.

The Chairman.—If the population is redundant, do you suppose a consequent impoverishment of the soil?

Mr. Ransome said he had taken a hypothetical case—a farm of a given extent, supporting a certain number of animals during a cycle of years.

The Chairman.—If I understand you right the animals and the men would consume that produce.

Mr. Ransome.—Yes. Sometimes, however, an animal would be satisfied with eating a certain quantity, and sometimes he would eat more than was required for his sustenance; a fault not confined to brutes—man also often takes a little too much. In the excrements of animals some of the surplus passed through unchanged, and Liebig remarks that " we cover our fields every year with the seed of weeds, which, from their nature and form pass undigested along with the excrements through animals without being deprived of their power of germination ; and yet it is considered surprising that where they have once flourished they cannot again be expelled by all our endeavours: we think it very astonishing, while we really sow them ourselves every year. A famous botanist attached to the Dutch embassy to China could scarcely find a single plant on the corn fields of the Chinese, except the corn itself." By thus spreading over our corn fields the excrements of animals, we impregnate the soil with the seeds of weeds, which interrupt the growth of the crops we are seeking to cultivate. In Flanders and China animal excrements are thought little of, while human excrements are highly prized ; and urine, a manure which in this country is perhaps most neglected, is with them considered best of all. When exposed for a length of time to the air, urea undergoes decomposition, uric acid and lactate of ammonia undergo decomposition, and free ammonia escapes into the air. What is left behind is collected for the purpose of manuring the ground ; but it has lost the principal element which should have been saved—namely, ammonia, which, passing into the air, benefits the neighbouring farmer as much as him who collects it. This gives rise to the question, can it not be economised? In China we know they take the greatest care of it. Mr. Ransome then referred to what he had said on a former occasion respecting the influence of gypsum and muriate of lime, which, in certain circumstances, converted carbonate of ammonia into sulphate and muriate of ammonia. He also showed, on that occasion, that some earths had an affinity for ammonia. Professor Liebig proposed that wherever urine was kept for the purpose of manure, that it should be mixed, before it underwent decomposition, with coarsely powdered gypsum, or sulphate of lime. By this means the ammonia, instead of escaping into air, became fixed, and could be removed to the soil. This kind of manure was exceedingly useful in breaking up the masses of which the earth was composed, subdividing it and otherwise improving its mechanical texture. Having seen from what source nitrogen, phosphates of soda and lime, &c., might be derived, and also how they might be diminished, he would next come to the question, in what proportions were they to be added to the soil? If a

proportion greater than nature pointed out were used, the plants would be over-stimulated, and the produce would be diminished. It was necessary, therefore, to observe a medium in the use of these means; for, supposing that the presence of nitrogen gave the plant a power of assimilating more rapidly the carbon, the oxygen, and the hydrogen, for whatever these three existed they were generally in combination with the vesicle or membrane that contained the nitrogen, yet the quality of some plants would undergo a change on account of the extra proportion of nitrogen. Wheat, for instance, would deteriorate in quality, and potatoes also. The latter would contain less starch, be larger, and have more cells; they would be waxy, and in Lancashire he could hardly expect such a change would be considered advantageous. The suggestion of Professor Liebig respecting the use of the bone manure was important. He said, that instead of allowing the bones, simply after pulverization, to be sprinkled over the fields, they should be mixed with half their weight of sulphuric acid, and after remaining in contact for some time, to be diluted with 800 or 1,000 parts of water, and then sprinkled over the soil. This would render the ground far more productive than if a course powder were sprinkled over its surface. The sandy soils of the South American coast were rendered fertile by a process not unlike that produced by the use of the urine of men and animals. They were manured by a substance called guano, consisting of urate of ammonia and other ammoniacal salts found in the islands of the Pacific, and abundant crops were produced. The idea was taken from the fertility produced by the fæces of carnivorous birds, which were analogous to those of serpents. The next point to which he would draw their attention was, that if they supplied the land with nitrogen, and at the same time omitted to supply it with potash, soda, magnesia, alumina, or other inorganic substances, they would do it more harm than good. This had been practically decided in the vineyards, in the neighbourhood of the Rhine, where some persons were in the habit of manuring their vines with the cuttings of horn, and bone powder, by which means they forced the growth of the vines, and increased the quantity of their produce; but as they neglected to supply their vines with inorganic elements which were necessary for the plants, they completely ruined their vines. There was one poor man in that place, whose sole dependence was on the produce of his vineyard, and as he was not able to manure his vines after the expensive manner of his neighbours, with an ingenuity which did him credit, he discovered a method of renewing his vines, which were almost worn out, and which were the only support of his old age. He observed, that wherever the cuttings of the vine were buried, the grass sprung up most luxuriantly; and he reasoned thus:—

" If the grass springs up luxuriantly on account of these cuttings, why may not the vines? Accordingly he applied the cuttings round the roots of the vines—inorganic elements being thus supplied to the plants, his vineyard from being the poorest soon became the richest in the neighbourhood.   In what part of the plant were these inorganic elements found? In a spreading tree they were in the largest proportion in the leaves, next in the branches, and least of all in the trunk.   At the autumnal fall of the leaf they were once more returned to the soil, where they were again taken in at the spongy parts of the roots, and thus the foliage and growth of the tree were renewed from year to year.   These were points bearing on practical culture, but there were one or two curiosities connected with it which he would be wrong to omit.   On a former occasion he observed that one plant might contain potash, and another of the same kind soda, which shewed that there was a principle of substitution in existence.   This was proved in the case of a certain maratime plant, *salso alkali*, which, when growing on the shore contained soda as its alkaline principle, but if sown on land where there was no soda, it would produce seeds containing one half soda and one half potash : but the plants from these seeds would produce nothing but potash, shewing how one plant might replace another, where nothing else could be had. In some samples of the Jesuit's bark it was found that the proportion of quinine was very trifling, but whenever this was the case, the deficiency was supplied by lime.   On the other hand with respect to acid, there was plenty of opium that had none of its peculiar acid, the meconic, but the deficiency was replaced by sulphuric acid ; so that it might be sometimes necessary to deprive the soil of some of its inorganic elements, in order that a plant having deficiency of one acid or alkali, might be compensated by the supply of another of its own secretion.   In looking forward, therefore, to a crop, the first question would be, what does the soil contain? and the next, what will the crop, which may be expected from the soil, contain?   And if it were found that no relation or correspondence existed between the crop and the soil, success could not be expected ; and thus the interference of the art of culture was called into existence.

I have thus, gentlemen, (said Mr. Ransome,) endeavoured, as well as I could from memory, to bring before you some of the facts recorded in Professor Liebig's book, and I hope the seeds which have been thus cast on the earth, may bring forth fruit in such abundance, as will convince the most ignorant and the most prejudiced that science can do something for agriculture, and that Professor Liebig, in sending forth this book, may be considered as the friend and benefactor of his species. (Applause.)

The Chairman said, that as the subject was one of vast interest to the country, he would be happy to hear any observations

which any of the gentlemen present, some of whom were agriculturists, might choose to offer.

Mr. Read : I understood you to say, that Professor Liebig had given the amount of potash derived from the decomposition of the rocks which produce it?

Mr. Ransome : He takes a Hessian acre, which contains 40,000 square feet, and says if that undergoes decomposition to the depth of 20 inches, a soil of fell spar would contain 1,152,000lbs. clink stone, from 20,000 to 40,000lbs. basalt, from 47,500 to 75,000lbs. clay slate, from 100,000 to 200,000lbs. loam, from 87,000 to 300,000lbs. potash, is present in all clays : according to Fuchs it is contained even in marl. It has been found in all the argillaceous earth in which it has been sought. He shows that the inorganic element, potash, may be derived from the decomposition of rocks: An expert agriculturist could tell the certain constituents necessary to produce a crop.

A gentleman said he was reminded by the observations of Mr. Ransome of what he had frequently seen in Cheshire, where the farmers had their dunghills elevated in such a way, that the fluid parts ran off and escaped, leaving them only the solid and inferior parts.

The Chairman said that the farmers in Switzerland were very particular with regard to the preservation of their urine. They had pits to the depth of 84 feet below the surface of the ground, where, surrounded by a sort of wicker-work, the urine was preserved until it was mixed with loam, as Mr. Ransome described.

Mr. Simpson knew a gentleman who considered it profitable to keep a pack of hounds, for by the wash from their kennel he obtained twelve loads of hay, where only three grew before.

Mr. Moore said, he was sure the agriculturalists of the neighbourhood would consider themselves much obliged by the kind and able manner in which Mr. Ransome had introduced Professor Liebig's book to their notice. His long connection with the Manchester agricultural society had afforded him (Mr. Moore) frequent opportunities of intercourse with intelligent farmers, who had often acknowledged the want of such a work ; indeed hitherto, little had been known as to the comparative value of manures beyond their chemical action. For instance, farmers had been well aware that, to make sandy or loose land retain a proper supply of water for vegetation, the addition of marl was necessary ; and sand had been usefully applied to stiff clayey soils to render them less tenaceous of water, and to enable the roots of plants to spread themselves and range about more freely in search of food, whatever that food might be. Farmers also know, that upon poor or exhausted soils, they could get crops of potatoes, corn, and clover, by the addition of stable dung, either used by itself, or mixed with the

night soil, which Manchester furnishes to a very great extent; and upon newly reclaimed moss land, they could grow potatoes, turnips, or cabbage, with the same kinds of manure; but, before they attempted to produce wheat, oats, or barley, it was necessary to apply marl to enable the corn to support itself, there being no proper matter in the peat earth to furnish the flinty outside of the straw. Gardeners as well as farmers noticing the luxuriance with which hyacinths and other bulbous rooted plants flourish in water, have been naturally led to believe that water and the soluble matter it contained must have formed the most important food of such plants; and the same persons observing many epiphytes would flower beautifully, although entirely disengaged from other plants, if they were suspended in an atmosphere containing a proper degree of moisture, could not help believing that this tribe of plants at least was supported by a supply of food from the air itself. The great value of Liebig's book appeared to be this—that, when put into more familiar language, it will enable farmers to account for their failures as well as for their success under different management, and explain many interesting facts with which they have long been familiar in practice. Mr. Moore also observed, that as it was now ascertained that plants require access to the air by their roots, as well as by their leaves, the necessity and advantage of keeping land well drained and free from stagnant water was clearly demonstrated. To chew that the farmers near Manchester were not unmindful of the advantages which that town afforded them, Mr. Moore said he had, about twelve years back, procured an account of the manure removed from the town on the Cheshire road, and found that more than twelve hundred carts passed weekly through the Cornbrook bar alone, laden with dung. Taking two-thirds of this manure to be applied to the growth of potatoes, which several intelligent farmers agreed with him in supposing would be the case, and reckoning upon a fair average crop, he found that more than three hundred thousand loads of this most useful vegetable was thus procured, beside the corn and clover which, in the usual rotation of crops, would follow. In this way, when a judicious system of agriculture is pursued in the neighbourhood, the inhabitants of large towns are regularly relieved from unhealthy accumulations of offensive matter, and, by the wonderful arrangements of an Almighty Providence, are continually receiving back, in an endless variety of new and beautiful forms, a bountiful supply of those simple elements which constitute the most nutritive and essential food of man.

Mr. Day said that when Mr. Ransome spoke of urea as a manure, it reminded him of several fertile meadows in the neighbourhood of Edinburgh, whose production he found, on idquiry, to arise from their being overflowed several times a year by the common sewers of the city. This was a very extensive experiment, proving the fact mentioned by Mr. Ransome. If some provision were made in Manchester for the

preservation of urea, it would be of great benefit to the surrounding neighbourhood.

The Chairman was surprised that in Edinburgh, where there were such excellent farmers—East Lothian particularly—there should be a waste of such material. He remembered that some time ago a joint-stock company was formed in London to pump up all the main sewers, and send their contents off by the railways to the surrounding districts. If something of the kind had been done, it would have proved useful to society at large.

Dr. Black said their was an immense reservoir in the vicinity of Paris for the reception of urine.

Mr Ransome said that while this was a powerful manure, according to Liebig it would be still more so, if it were neutralized by some cheap mineral acid.

Mr. Hopkins said that in Rome, not only did they throw all the sweepings of the street into the Tiber, but the refuse of public slaughter-houses; such as the blood and animal excrements were also thrown in, and no one ever saw a single load of manure taken from Rome to the adjoining neighbourhood. It was to be hoped that such views as were developed that night would reach even Rome, and teach the people of that country the necessity of preserving this valnable manure. He trusted that through the exertions of Mr. Ransome, Professor Liebig's book would soon be epitomized, and put into the hands of the farmers in a popular and familiar style of language, so that they might be able to enter into the views of the author, and add their experience to the scientific knowledge which he had brought to bear upon this subject. Farmers occasionally meet with facts, which they observe and act upon, but, being unable to account for them, these facts generally remained insolated and barren. When in Italy in 1814, he (Mr H.) observed large trenches dug, and filled with sticks, in which the vines were planted. On enquiring' he was told that the sticks were employed, because the roots of the vine would penetrate more easily through them. Their idea was to diminish the mechanical resistance, but from what Mr. Ransome had stated, it was evident that they had planted their vines in the very best stratum that could be procured. They knew, by experience, the value of the sticks, but could not tell in what respect they were valuable. But as soon as science was united with the accidental experience of the practical man, the results would be of the most beneficial character.

The Rev. W. Hutchinson said soda was not only useful as a manure, but also valuable in destroying snails, &c., which injured the plant.

The Chairman observed that there was one important fact which should never be forgotten,—namely, draining and the

use of subsoil ploughs, which would let the air down to the roots of the plants, and increase their vegetation. He very much regretted that little attention was paid to culture in this country. If farmers paid sufficient attention to ploughing and draining, they would have double the usual produce. He trusted, that by a better system of culture than was hitherto adopted in this country, where agriculture was considered only as a secondary object, farmers would be able, not merely to grow twice, but three times the amount of produce now obtained from the surface of Great Britain and Ireland.

Mr. Read : Mr. Ransome mentioned, that in South America the inhabitants made the sandy soils capable of producing abundant crops, by using an admixture of the fœces of carnivorous birds and serpents.

Mr. Read said, that as we were informed in scripture that in time of scarcity the fourth part of a cale of doves' dung sold for five pieces of silver, he wished to know if there was any comparative estimate of the prolific capability of that species of manure traced by Liebig ?

Mr. Ransome answered in the negative.

Mr. Swindells said, that farmers had ascertained that bones were better manure after than before they were boiled. If they were applied to the soil without being boiled, they would not produce any effect for two or three years, because the gelatine should be dissolved before the ammonia could act on the phosphate of lime ; but when the gelatine became dissolved, the bones became a prolific manure.

Mr. Ransome then read the following experiments and observations on the action of charcoal on vegetation, by Edward Lucas.

" In a division of a low hothouse in the botanical garden at Munich, a bed was set apart for young tropical plants, but instead of being filled with tan, as is usually the case, it was filled with the powder of charcoal, (a material which could be easily procured,) the large pieces of charcoal having been previously separated by means of a sieve. The heat was conducted by means of a tube of white iron into a hollow space in this bed, and distributed a gentle warmth, sufficient to have caused tan to enter into a state of fermentation. The plants placed in this bed of charcoal quickly vegetated, and acquired a healthy appearance. Now, as always is the case in such beds, the roots of many of the plants penetrated through the holes in the bottom of the pots, and spread themselves out ; but these plants evidently surpassed in vigour and general luxuriance plants grown in the common way, for example, in tan. Several of them, of which I shall only specify the beautiful Thunbergia alata, and the

genus Peireskia, throve quite astonishingly; the blossoms of the former were so rich, that all who saw it affirmed they had never before seen such a specimen. It produced also a number of seeds without any artificial aid, while in most cases it is neces- sary to apply the pollen by the hand. The Peireskia grew so vigorously, that the P. aculeata produced shoots several ells in in length, and the P. grandifolia acquired leaves of a foot in length. These facts, as well as the quick germination of the seeds which had been scattered spontaneously, and the abun- dant appearance of young Filices, naturally attracted my atten- tion, and I was gradually led to a series of experiments, the results of which may not be uninteresting; for, besides being of practical use in the cultivation of most plants, they demon- strate also facts of importance to physiology. The first experi- ment which naturally suggested itself was, to mix a certain proportion of charcoal with the earth in which the different plants grew, and to increase its quantity according as the advantage of the method was perceived. An addition of two- thirds of charcoal, for example, for vegetable mould, appeared to answer excellently for the Gesneira and Gloxinia, and also for the tropical Aroideæ with tuberous roots. The two first soon excited the attention of connoisseurs, by the great beauty of all their parts and their general appearance. They surpassed very quickly those cultivated in the common way, both in the thickness of their stems and dark colour of their leaves; their blossoms were beautiful, and their vegetation lasted much longer than usual: so much so, that in the middle of November, when other plants of the same kinds were dead, these were quite fresh, and partly in bloom. Aroidræ took root very rapidly, and their leaves surpassed much in size the leaves of those not so treated; the species, which are reared as ornamental trees on account of the beautiful colouring of their leaves, (I mean such as the Caladium bricolor, Pictium, Pæcile, &c.,) were particu- larly remarked for the liveliness of their tints; and it happened here also, that the period of their vegetation was unusually long. A cactus, planted in a mixture of charcoal and earth, throve progressively, and attained double its former size in the space of a few weeks. The use of the charcoal was very advantage- ous, with several of the Bromeliacæ and Siliaceæ, with the Citrus and Begonia also, and even with the Palmæ. The same advantage was found in the case of almost all those plants for which sand is used in order to keep the earth porous, when charcoal was mixed with the soil instead of sand; the vegeta- tion was always rendered stronger and more vigorous. "At the same time that these experiments were performed with mix- tures of charcoal with different soils, the charcoal was also used free from any addition, and in this case the best results were obtained. Cuts of plants from different genera took root in it well and quickly. I mention only the Euphorbia Fastnoso and Eulgens which took root in ten days; Pandaus utilus in three

months, P. amaryllifolius, Chamœdorea élatior, in four weeks, Pipernigean, Begonia, Ficus, Cacropia, Chicocea, Buddleja, Hatrea, Phyllanthus, Capparis, Laurus, Stifftsa, Jacquinia, Mimosa, Cactus, in from eight to ten days, and several others, amounting to forty species, including Ilex and many others. Leaves, and pieces of leaves,* and even dedimenti or petioles, took root in part budded in pure charcoal. Amongst others we may mention the fioliola of several of the Cycadeœ as having taken root, as also did parts of the leaves of the Begoniee Selsairice, and Tacaranda brasiliensis, leaves of Euphorbia fastnosa, Oxelis Barrilieri, Ficus, Cyclamen, Polyanthus, Mesembrianthemum ; also, pieces of a leaf of the Agave American, tufts of Pinus, &c., and all without the aid of a previously formed bud.

 "Pure charcoal acts excellently as a means of curing unhealthy plants. A *Dorianthes exceloa*, for example, which had been drooping for three years, was rendered completely healthy in a very short time by this means. An orange-tree, which had the very common disease in which the leaves became yellow ; acquired within four weeks its healthy green colour, when the upper surface of the earth was removed from the pot in which it was contained, and a ring of charcoal of an inch in thickness strewed in its place around the periphery of the pot. The same was the case with the *Gardemia*.

"I should be led too far, were I to state all the results of the experiments which I have made with charcoal. The object of this paper is merely to show the general effect exercised by this substance on vegetation, but the reader who takes particular interest in the subject, will find more extensive observations in the *Allgemeine deutsche Carteuzeitung*, of Otto and Dirtrich in Berlin.

"The charcoal employed in these experiments was the dust like powder of charcoal from firs and pines, such as is used in the forges of blacksmiths, and may be easily procured in any quantity. It was found to have most effect when allowed to lie during the winter exposed to the action of the air. In order to ascertain the effects of different kinds of charcoal, experiments were also made upon that obtained from the hard woods and peat, and also upon animal charcoal, although I foresaw the probability that none of them would answer so well as that of pinewood, both on account of its porosity and the ease with which it is decomposed. It is superfluous to remark that in treating plants in the manner here described they must be plentifully supplied with water, since the air, having such free access, penetrates, and dries the roots, so that unless this precaution is taken, the failure of all such experiments is unavoidable.

"The action of charcoal consists primarily in its preserving

the parts of the plants with which it is in contact, whether they be roots, branches, leaves, or pieces of leaves, unchanged in their vital power for a long space of time, so that the plant obtains time to develope the organs which are necessary for its further support and propagation. There can scarcely be a doubt, also, that the charcoal undergoes decomposition, for after being used five to six years it becomes a coaly earth, and if this is the case it must yield carbon, or carbonic oxide, abundantly to the plants growing in it, and thus afford the principal substance necessary for the nutrition of vegetables. In what other manner, indeed, can we explain the deep green colour and great luxuriance of the leaves and every part of the plants, which can be obtained in no other kind of soil, according to the opinion of men well qualified to judge. It exercises, likewise, a favourable influence by decomposing and absorbing the matters absorbed [query, excreted] by the roots, so as to keep the soil free from the putrefying substances which are often the cause of the death of the spongiolæ. Its porosity, as well as the power which it possesses of absorbing water with rapidity, and, after its saturation, of allowing all other water to sink through it, are causes also of its favourable effects. These experiments show what a close affinity the component parts of charcoal have to all plants, for every experiment was crowned with success, although plants belonging to a great many different families were subjected to trial."

LETTER from WILLIAM STURGEON to the Editor of the "*Nautical Magazine.*"

In your number of the "*Nautical Magazine*" for the present month, at the foot of page 110, you have inserted the following note, "Doubtless Mr. Sturgeon's remarks on Dr. Faraday were quite uncalled for." I do not mean to say that *your* remark, in that note, was quite uncalled for, because I think it possible that you entertain an idea that there are some misstatements in my strictures on Dr. Faraday's papers; and that you are prepared, and may probably have a wish, to point them out. To such a proceeding I can certainly have no objection, nor can any one more cheerfully acknowledge any error in his scientific writings, than I should do, were they clearly and satisfactorily pointed out, either by you, or by any other gentleman. But as I have, already, been led into a very unpleasant and fruitless discussion, with Mr. Harris, on a question

of great national importance, it is my intention to avoid, as
much as possible, again entering into discussion with such desul-
tqreous and unprepared reasoners : and, therefore, should you
be inclined to enter the lists in favor of those papers of Dr.
Faraday which I have called in question, I must request that
you will draw no inferences but from those facts with which
you are perfectly acquainted from your own experience, and
that you strictly adhere to the subject matter which you under-
take to discuss, without having recourse to other authority, and
without allusion to any other subject or circumstance whatever.
Upon these conditions, and these alone, can I be induced to de-
vote much time to reply to any remarks you may think proper
to place before the readers of your magazine.

As to the desultory clamour in which Mr. Harris continues
to indulge, in the "*Nautical Magazine*,"* it being of the same
stamp as that to which he has constantly resorted from the com-
mencement of his defence of his lightning conductors, is not
likely to have much weight on the minds of those who are qua-
lified to judge of the matter : therefore, I must decline making
any remark on the *very scientific and gentlemanly* effusions
which appear in your last number.   My opinion, and the rea-
sons for forming that opinion, of Mr. Harris's marine lightning
conductor, are very clearly stated in the fourth volume of the
"Annals of Electricity, &c.," and in that volume and in the
succeeding one, are the whole of my letters, in reply to the
various attempts, that have since been made, to defend the pro-
priety of those conductors being generally introduced to the
Royal Navy.

<div align="center">

I have the honor to be,

Sir,

Your very obedt. Servt.

WILLIAM STURGEON.

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Royal Victoria Gallery of Practical Science,
    Manchester, Feb. 22nd, 1841.

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* The paper alluded to, appears in the February No. of the "*Nautical
Magazine*," in which Mr. Harris employs his usual pell mell style of rea-
soning.   The theme of this production, is an attempt to evade the effects
of my remarks on his unaccountable statement in page 53, vol. 5, of these
"Annals," viz. "It is only in the *absence* of continuous conductors we find
such magnetic effects, and even then their occurrence is comparatively rare."
   I do not intend to make any further remarks, on this point, than those
already before my readers; but as I am aware of the interest with which
original papers, containing data on controverted topics, are read, I have
procured the following document from the Philosophical Transactions of
the Royal Society, in which it will be found that Sir Humphrey Davy dis-
covered that electric discharges, *through good conductors*, are productive of
powerful magnetic action on steel; even though situated at a considerable
distance from them, a fact now well known by every Electro-magnest.
<div align="right">W. S.</div>

On the Magnetic Phenomena produced by Electricity ; in a Letter from SIR H. DAVY, BART., *to* W. H. Wollaston, M. D. P. R. S.

Read Nov. 16, 1820.

MY DEAR SIR,

The similarity of the laws of electrical and magnetic attraction has often impressed philosophers : and many years ago, in the progress of the discoveries made with the voltaic pile, some enquirers (particularly M. Ritter,)* attempted to establish the existence of an identity or intimate relation between these two powers ; but their views being generally obscure, or their experiments inaccurate, they were neglected ; the chemical and electrical phenomena exhibited by the wonderful combination of Volta, at that time almost entirely absorbed the attention of scientific men,; and the discovery of the fact of the true connexion between electricity and magnetism, seems to have been reserved for M. Œrsted, and for the present year. This discovery, from its importance and unexpected nature, cannot fail to awaken a strong interest in the scientific world ;

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* M. Ritter asserted that a needle composed of silver and zinc arranged itself in the magnetic meridian, and was slightly attracted and repelled by the poles of a magnet; and that a metallic wire, after being exposed in the voltaic circuit, took a direction N. E. and S. E. His ideas are so obscure, that it is often difficult to understand them : but he seems to have had some vague notion that electrical combinations, when not exhibiting their electrical tension, were in a magnetic state, and that there was a kind of electromagnetic meridian depending upon the electricity of the earth. (See Annales de Chimie, tome 64, p. 80.) Since this letter has been written, Dr. Marcet has been so good as to send me from Genoa some pages of Aldini on Galvanism, and of Izarn's Manual of Galvanism, published at Paris more than sixteen years ago. M. Mojon, senior, of Genoa, is quoted in these pages, as having rendered a steel needle magnetic, by placing it in a voltaic circuit for a great length of time. This, however, seems to have been dependent merely upon its place in the magnetic meridian, or upon an accidental curvature of it : But M. Romagnesi, of Trente, is stated to have discovered that the pile of Volta caused a deviation of the needle. The details are not given, but if the general statement be correct, the author could not have observed the same fact as M. Œrsted, but merely supposed that the needle had its magnetic poles altered after being placed in the voltaic circuit as a part of the electrical combination.

and it opens a new field of enquiry, into which many experi-
menters will undoubtedly enter: and where there are so many
objects of research obvious, it is scarcely possible that similar
facts should not be observed by different persons.   The pro-
gress of science is, however, always promoted by a speedy
publication of experiments; hence, though it is probable that
the phenomena which I have observed may have been disco-
vered before, or at the same time in other parts of Europe, yet
I shall not hesitate to communicate them to you, and through
you to the Royal Society.

   I found in repeating the experiments of M. Œrsted with a
voltaic apparatus of one hundred pairs of plates, of four inches,
that the south pole of a common magnetic needle (suspended
in the usual way) placed under the communicating wire of the
platinum, (the positive end of the apparatus being on the right
hand,) was strongly attracted by the wire, and remained in con-
tact with it, so as entirely to alter the direction of the needle,
and to overcome the magnetism of the earth.   This I could
only explain by supposing that the wire itself became magnetic
during the passage of the electricity through it, and direct ex-
periments, which I immediately made, proved that this was the
case.   I threw some iron filings on a paper, and brought them
near the communicating wire, when immediately they were at-
tracted by the wire, and adhered to it in considerable quantities,
forming a mass round it ten or twelve times the thickness of
the wire: on breaking the communication, they instantly fell
off, proving that the magnetic effect depended entirely on the
passage of the electricity through the wire.   I tried the same
experiment on different parts of the wire, which was seven or
eight feet in length, and about the twentieth of an inch diame-
ter, and found the iron filings were every where attracted, and
the magnetic needle affected in every part of the circuit.

   It was very easy to imagine that such magnetic effects could
not be exhibited by the electrified wire, without being capable
of permanent communication to steel.   I fastened several steel
needles, in different directions, by fine silver wire to a wire of
the same metal, of about the thirtieth of an inch in thickness,
and eleven inches long, some parallel, others transverse, above
and below in different directions; and placed them in the elec-
trical circuit of a battery of thirty pairs of plates of nine inches
by five, and tried their magnetism by means of iron filings:
they were all magnetic: those which were parallel to the wire
attracted filings in the same way as the wire itself; but those in
transverse directions exhibited each two poles, which being
examined by the test of delicate magnets, it was found that all
the needles that were placed under the wire (the positive end of
the battery being east) had their north poles on the south side
of the wire, and their south poles on the north side; and that

those placed over, had their south poles turned to the south, and their north poles turned to their north ; and this was the case whatever was the inclination of the needles to the horizon. On breaking the connection, all the steel needles that were on the wire in a transverse direction retained their magnetism, which was as powerful as ever, whilst those which were parellel to the sïiver wire appeared to lose it at the same time as the wire itself.

I attached small longitudinal portions of wires of platinum silver, tin, iron, and steel, in transverse directions, to a wire of platinum that was placed in the circuit of the same battery. The steel and the iron wire immediately required poles in the same manner as in the last experiment : the other wires seemed to have no effect, except in acting merely as parts of the electrical circuit : the steel retained its magnetism as powerfully after the circuit was broken as before ; the iron wire immediately lost a part of its polarity, and in a very short time the whole of it.

. The battery was placed in different directions as to the poles of the earth, but the effect was uniformly the same. All needles placed transversely under the communicating wires, the positive end being on the right hand, had their north poles turned towards the face of the operator, and those above the wire their south poles : and on turning the wire round to the other side of the battery, it being in a longitudinal direction, and marking the side of the wire, the same side was always found to possess the same magnetism ; so that in all arrangements of needles transversely round the wire, all the needles above had north and south poles opposite to those below, and those arranged vertically on one side, opposite to those arranged vertically on the other side.

I found that contact of the steel needles was not necessary, and that the effect was produced instantaneously by the mere juxta-position of the needle in a transverse direction, and that through very thick plates of glass ; and a needle that had been placed in a transverse direction to the wire merely for an instant, was found as powerful a magnet as one that had been long in communication with it.

I placed some silver wire of 1-20th of an inch, and some of 1-50th, in different parts of the voltaic circuit when it was completed, and shook some steel filings on a glass plate above them; the steel filings arranged themselves in right lines, always at right angles to the axis of the wire ; the effect was observed, though feebly, at the distance of a quarter of an inch above the thin wire, and the arrangement in lines was nearly to the same length on each side of the wire.

I ascertained by several experiments, that the effect was proportional to the quantity of electricity passing through a given

space, without any relation to the metal transmitting it ; thus the finer the wires the stronger their magnetism.

A zinc plate of a foot long and six inches wide, arranged with a copper plate on each side, was connected, by a very fine wire of platinum, according to your method ; and the plates were plunged an inch deep in dilute nitric acid. The wire did not sensibly attract fine steel filings. When they were plunged two inches, the effect was sensible ; and it increased with the quantity of immersion. Two arrangements of this kind acted more powerfully than one ; but when the two were combined so as to make the zinc and copper plates but parts of one combination, the effect was very much greater. This was shown still more distinctly in the following experiment :—Sixty zinc plates with double copper plates were arranged in alternate order, and the quantity of iron filings which a wire of a determinate thickness took up observed : the wire remaining the same, they were arranged so as to make a series of thirty ; the magnetic effect appeared more than twice as great ; that is, the wire raised more than double the quantity of iron filings.

The magnetism produced by voltaic electricity, seems (the wire transmitting it remaining the same) exactly in the same ratio as the heat ; and however great the heat of the wire, its magnetic powers were not impaired. This was distinctly shown in transmitting the electricity of twelve batteries of ten plates each of zinc, with double copper arranged as three, through fine platinum wire, which when so intensely ignited as to be near the point of fusion, exhibited the strongest magnetic effects, and attracted large quantities of iron filings, and even small steel needles from a considerable distance. •

As the discharge of a considerable quantity of electricity through a wire seemed necessary to produce magnetism, it appeared probable, that a wire electrified by the common machine would not occasion a sensible effect ; and this I found was the case on placing very small needles across a fine wire connected with a prime conductor of a powerful machine and the earth : but as a momentary exposure in a powerful electrical circuit was sufficient to give permanent polarity to steel, it appeared equally obvious, that needles placed transversely to a wire at the time that the electricity of a common Leyden battery was discharged through it, ought to become magnetic : and this I found was actually the case, and according to precisely the same laws as in the voltaic circuit: the needle *under* the wire, the positive conductor being on the right hand, offering its north pole to the face of the operator, and the needle *above*, exhibiting the opposite polarity. So powerful was the magnetism produced by the discharge of an electrical battery of 17 square feet highly charged through a silver wire of 1-20th of an inch, that it ren-

dered a bar of steel, two inches long, and from 1-20th to 1-10th in thickness, so magnetic, as to enable them to attract small pieces of steel wire or needles: and the effect was communicated to a distance of five inches above or below or laterally from the wire, through water or thick plates of glass, or metal electrically insulated.*

The facility with which experiments were made with the common Leyden battery, enabled me to ascertain several circumstances which were easy to imagine, such as that a tube filled with sulphuric acid of 1-4th of an inch in diameter, did not transmit sufficient electricity to render steel magnetic; that a needle placed transverse to an explosion through air, was less magnetized than when the electricity was passed through wire,† the steel bars exhibited no polarity (at least at their extremities) when the discharge was made through them as part of the circuit, or when they were placed parallel to the discharging wire: that two bars of steel fastened together, and having the discharging wire placed through their common centre of gravity, showed little or no sign of magnetism after the discharge till they were separated, when they exhibited their north and south poles opposite to each other, according to the law of position.

These experiments distinctly showed, that magnetism was produced wherever concentrated electricity was passed through space; but the precise circumstance, or law of its production, was not obvious from them. When a magnet is made to act on steel filings, these filings arrange themselves in curves round the poles, but diverge in right lines: and in their adherence to each other, form right lines, appearing as spicula. In the attraction of the filings round the wire in a voltaic circuit, on the contrary, they form one coherent mass, which would probably be perfectly cylindrical were it not for the influence of gravity. In first considering the subject, it appeared to me that there must be as many double poles as there could be imagined points of contact round the wire; but when I found that N. and S. poles of a needle uniformly attracted by the same quarters of the wire, it appeared to me that there must be four principal poles corresponding to these four quarters. You, however, pointed out to me that there was nothing definite in the poles, and mentioned your idea, that the phenomena might be explained, by supposing a kind of revolution of magnetism round the axis of the wire, depending for its direction upon the position of the negative and positive sides of the electrical apparatus.

To gain some light upon this matter, and to ascertain correctly the relations of the north and south poles of steel magnetized by electricity to the positive and negative state, I placed short steel needles round a circle made on pasteboard, of about

two inches and a half in diameter, bringing them near each
other, though not in contact, and fastening them to the paste-
board by thread, so that they formed the sides of a hexagon in-
cribed within the circle. A wire was fixed in the centre of this
circle, so that the circle was parallel to the horizon, and an
electric shock was passed through the wire, its upper part being
connected with the positive side of a battery and its lower part
with the negative.   After the shock all the wires were found
magnetic, and each had two poles ; the south pole being op-
posite to the north pole of the wire next to it, and vice versa :
and when the north pole of a needle was touched with a wire,
and that wire moved round the circle to the south pole of the
same needle, its motion was opposite to that of the apparent
motion of the sun.

A similar experiment was tried with six needles arranged in
the same manner : with only this difference, that the wire posi-
tively electrified was below.   In this case the results were
precisely the same, except that the poles were reversed : and
any body, moved in the circle from the north to the south pole
of the same needle, had its direction from east to west.

A number of needles were arranged as polygons in different
circles round the same piece of card board, and made magnetic
by electricity : and it was found that in all of them, whatever
was the direction of the paste-board, whether horizontal or
perpendicular, or inclined to the horizon, and whatever was the
direction of the wire with respect to the magnetic meridian, the
same law prevailed ; for instance, when the positive wire was
east, and a body was moved round the circle from the north to
the south poles of the wire : its motion (beginning with the lower
part of the circle) was from north to south, or with the upper
part from south to north : and when the needles were arranged
round a cylinder of paste-board so as to cross the wire, and a
pencil mark drawn in the direction of the poles, it formed a
spiral.

It was perfectly evident from these experiments, that as many
polar arrangements may be formed as chords can be drawn in
circles surrounding the wire : and so far these phenomena agree
with your idea of revolving magnetism : but I shall quit this
subject, which I hope you will yourself elucidate for the in-
formation of the society, to mention some other circumstances
and facts belonging to the enquiry.

Supposing powerful electricity to pass through two, three,
four, or more wires, forming part of the same circuit parallel
to each other in the same plane, or in different planes, it could
hardly be doubted that each wire, and the space around it,
would become magnetic in the same manner as a single wire,
though in a less degree ; and this I found was actually the case.

When four wires of fine platinum were made to complete a powerful voltaic circuit, each wire exhibited its magnetism in the same manner, and steel filings on the sides of the wires opposite attracted each other. •

As the filings on the opposite sides of the wires attracted each other in consequence of their being in opposite magnetic states, it was evident, that if the similar sides could be brought in contact, steel filings upon them would repel each other. This was very easily tried with two voltaic batteries arranged parallel to each other, so that the positive end of one was opposite the negative end of the other; steel filings upon two wires of platinum joining the extremities strongly repelled each other. When the batteries were arranged in the *same* order, i. e. positive opposite to positive, they attracted each other; and wires of platinum (without filings) and fine steel wire (still more strongly) exhibited similar phenomena of attraction and repulsion under the same circumstances.

As bodies magnetized put a needle in motion, it was natural to infer that a magnet would put bodies magnetized by electricity in motion: and this I found was the case. Some pieces of wire of platinum, silver, and copper, were placed separately upon two knife edges of platinum connected with two ends of a powerful voltaic battery, and a magnet presented to them: they were all made to roll along the knife edges, being attracted when the north pole of the magnet was presented, the positive side of the battery being on the right hand, and repelled when it was on the left hand: and vice versa, changing the pole of the magnet. Some folds of gold leaf were placed across the same apparatus, and the north pole of a powerful magnet held opposite to them: the folds approached the magnet, but did not adhere to it. On the south pole being presented, they receded from it.

I will not indulge myself by entering far into the theoretical part of this subject; but a number of curious speculations cannot fail to present themselves to every philosophical mind, in consequence of the facts developed: such as whether the magnetism of the earth may not be owing to its electricity, and the variation of the needle to the alterations of the electrical currents of the earth in consequence of its motions, internal chemical changes, or its relations to solar heat; and whether the luminous effects of the auroras at the poles are not shown, by these new facts, to depend on electricity. This is evident, that if strong electrical currents be supposed to follow the apparent course of the sun, the magnetism of the earth ought to be such as it is found to be.

But I will quit conjectures, to point out a simple mode of

making powerful magnets, by fixing bars of steel across, or circular pieces of steel fitted for making horse shoe magnets, round the electrical conductors of buildings in elevated and exposed situations.*

The experiments detailed in these pages were made with the apparatus belonging to the Royal and London Institutions ; and I was assisted in many of them by Mr. Pepys, Mr. Allen, and Mr. Stodart, and in all of them by Mr. Farraday.†

I am, my dear Sir,

Very sincerely yours,

HUMPHREY DAVY.

Lower Grosvenor Street, Nov. 12, 1820.

* There are many facts recorded in the Philosophical Transactions which prove the magnetizing powers of lighting : one in particular, where a stroke of lightning passing through a box of knives, rendered most of them powerful magnets.—See Phil. Trans No. 147, p, 520, and No. 437, p. 57.

† All the experiments detailed in this paper, except those mentioned page 15, (experiments on the needles, page 7 of this manuscript) were made in the course of October, 1820 : the last arose in consequence of a conversation with Dr. Wollaston, and were made in the beginning of November. I find by *the Annales de Chimie et de Physique,* for September, which arrived in London November 24th, that M. ARAGO has anticipated me in the discovery of the attractive and magnetizing powers of the wires in the voltaic circuit : but the phenomena presented by the action of common electricity' (which as yet, I believe, have been observed by no other person,) induce me me still to submit my paper to the council of the Royal Society. Before any notice arrived of the researches of the French philosophers, I had tried, with Messrs. Allen and Pepys, an experiment, which M. ARAGO likewise thought of,—whether the air of flame of the voltaic battery would be effected by the magnet: but from the imperfection of our apparatus, the results were not decisive. I hope soon to repeat it under new circumstances.

I have made various experiments, with the hope of effecting electrified wires by the magnetism of the earth, and of producing chemical changes by magnetism : but without any successful results.

Since I have perused M. AMPERE's on the electro-magnetic phenomena, I have passed the electrical shock along a spiral wire twisted round a glass tube, containing a bar of steel, and I found that the bar was rendered powerfully magnetic by the process.

Description of a Novel Form of Electro Magnet. By JOSEPH
RADFORD, Esq., in a Letter to Mr. Sturgeon.

Waterloo Foundry, Offices, David-st.,
Manchester, Feb. 19th, 1841.

DEAR SIR,

Herewith I send you the new electro magnet which
I commenced experimenting upon early in October last, *and beg
your acceptance of it.*

The drawings in plate IV. will explain its character very
plainly.—

No. 1 being the elevation.
No. 2 the vertical section.
No. 3 the convoluted face.

It is nine inches in diameter, and the sketches being drawn
to scale the other dimensions can be ascertained easily.

The magnet weighs, exclusive of the copper coil, and the
keeper, sixteen pounds and two ounces.

The coil is a bundle of twenty-three small copper wires, and
weighs two pounds two ounces.

The coil is covered with cotton tape.

The keeper weighs 14lb. 14oz. and three-quarters.

The depth of the convoluted grove or recess, is 3-8ths of an
inch, and 1-⅛th of an inch wide.

The width or breadth of the metal between the grooves as
shown in the section, is half an inch.

The thickness of the magnet is one inch, at the outside edge,
and about 3-8th in the centre.

Before it was finished an experiment was made on the 3rd of
December, 1840, and it sustained a load of 2141 pounds, with
eight of your battery jars.

The greatest weight was lifted on the 17th of December last,
by a battery of yours, with twelve jars, and the weight sustained
was 2500 pounds avoirdupois.

If you think this worth a place in your annals, I shall feel
obliged by your insertion of it.

Yours, faithfully,
JOSEPH RADFORD.

DEAR SIR,

By your kindness in presenting to me this piece of apparatus, which, amongst many other favours, I take a pleasure in availing myself of this opportunity to acknowledge, you have put me in possession of the most curious and extraordinary magnet that has ever yet appeared on the pages of science, or ever been placed in the cabinet of philosophy.

The convoluted figure of its face is not only a great curiosity, but a perfect novelty in magnetics ; and the unusual arrangement of its poles, both of which are on the *same* convoluted strip of iron, (which forms the face) one pole occupying the whole length on one edge, and the other the whole length of the opposite edge: together with its great powers, render it a piece of apparatus of extraordinary character and great interest : and will, no doubt, command a great deal of attention amongst electro magnetists.

I am, dear Sir,

Yours, truly,

W. STURGEON.

Royal Victoria Gallery of Practical Science,
    Manchester, February 20*th,* 1841.

To Joseph Radford, Esq.

Observations on the Electrical Phenomena of High-Pressure Steam, as recently exhibited by two Engine Boilers in the vicinity of Newcastle ; with memoranda of an unsuccessful experiment made with a view to the investigation of Electricity from Steam. By W. H. WEEKES, Esq., Surgeon, Lecturer on Philosophical and Operative Chemistry, &c.

The attention of the scientific world, in conjunction with that of intelligent men generally, having recently been drawn to several reports of very extraordinary electrical phenomena proceeding from two steam engine boilers, situate in the vicinity of Newcastle, I have been induced to peruse such papers as have been published on the subject, and subsequently to undertake several experiments with the hope of arriving at some illustration of the matter. Though none of these were successful in reproducing the electrical phe-

nomena exhibited by the steam boilers, an outline of my principal attempt may, nevertheless, be worth recording.

The papers chiefly entitled to notice, in relation to this interesting fact, are three which have appeared, originally in the " Philosophical Magazine." First—" On the electricity of a jet of steam issuing from a boiler." By H. G. Armstrong, Esq.* Second— " Experiments on the electricity of high-pressure steam." By H. L. Pattison, Esq., F. G. S.+ The presumption of Mr. Armstrong, to use his own words, " is exceedingly strong, that the phenomenon is in some way occasioned by the peculiar nature of the water from which the steam is produced." He mentions an " incrustation of a month's growth, deposited by the water from the mine in the boilers in which it is used," a specimen of which incrustation Professor Faraday says " contains ·traces of a soluble muriate and sulphate, but consists almost entirely of sulphate of lime, with a little oxide of iron and insoluble clayed matter, carried in probably by the water. There is hardly a trace of carbonate of lime in it." I am quite incapable of conceiving that the powerful electrical effects exhibited by the jet of steam in question could in any way be dependent on a local cause, and especially so that they should arise from the presence of the above named material in the water forming the incrustation.

Mr. Pattinson, whose paper is an exceedingly interesting one, details a number of experiments performed by him, precisely similar in their results to those ordinarily obtained from the employment of good frictional machines; such as charging jars and passing shocks through a chain of from twelve to twenty persons, perforating a card, firing combustible substances, &c., and he, too, concludes that " it is hardly possible to suppose that there is any local peculiarity about these boilers, or the place where they are situated, to occasion the highly electrical condition of the steam produced in them; and yet it is difficult," he further remarks, " to suppose the fact of high-pressure steam being electrical, a general one." Mr. Pattinson considers that " the conditions, therefore, under which steam becomes electrical require to be investigated, and it is not unlikely that the investigation will lead to important results." Under feelings perfectly in accordance with such views my own experiments were entered upon.

I happen to have in my possession a very strong tin vessel which has commonly been employed for purposes of distillation; it is of a rectangular form and of the following dimensions. Length fourteen inches; width ten inches; average depth beneath the neck seven inches. From the centre of the top of

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* In two letters to Professor Faraday.

+ See p. p. 37 and 42 of this volume of the Annals. Also Vol. v. p. p. 452 and 456.—EDIT.

this vessel a conical tin beak or neck leads off three feet in length, and terminates in an opening 3-4ths of an inch diameter. This Still having been charged to one inch in depth over its bottom by pouring in three quarts of water, a surface of 140 superficial inches were consequently exposed to the heat arising from an ordinary open furnace. The opening of the tin beak being now partially closed by means of a good cork through which was inverted a glass tube ten inches in length, and rather less than a quarter of an inch bore, the heat was gradually increased until an atmosphere of steam became generated under considerable pressure within the cavity of the Still above the surface of water, and rushed furiously through the orifice of the glass tube with a continuous loud hissing sound. I then took a T formed conductor, belonging to a large electrical machine, well insulated upon a stout glass pillar, and presenting a surface of about 360 superficial inches. Into a hole in the single end of this conductor was inserted a stout wire projecting from the back of a polished concave tin reflector, one foot in diameter, commonly used in experiments on the radiation of heat. Against the focus of this reflector the action of the steam jet was directed with an increasing force during twenty minutes, while a communication, by means of a copper wire, also carefully insulated, was kept up between the conductor and a delicate gold-leaf electroscope, in the window-board of the laboratory, at about fifteen feet distance, in order that the condensation of any steam from the boiler should not affect or interrupt the action of the instrument. These arrangements I considered were as favourably disposed as could well be under my limited circumstances, in regard to the generation of high-pressure steam, yet, notwithstanding the most minute attention was devoted to the experiment throughout, the electroscope did not exhibit the slightest divergence.

To whatever cause the electrical phenomena proceeding from the steam boilers may ultimately be referred, the disengagement of this principle, during the conversion of water into steam and the converse, has been long since recognized by philosophers. Almost every electrician is, doubtless, familiar with the beautiful experiments of Cavallo, performed during the last century, by means of that exquisitely delicate instrument of his invention, denominated the "Multiplier," now by no means so much known or used as it deserves to be, and by means of which he shewed that two drops of water evaporated from a burning coal, placed upon an insulated tin-plate, caused a divergence in the gold leaves of his electroscope, more than sufficient to show that the tin-plate had been electrified negatively by the evaporation of the two drops of water. A powerful and yet delicate arrangement of machinery for the purposes of atmospheric electricity, similar in its operation to the celebrated Broomfield Apparatus, and which I have been in the almost daily habit of experimenting with during many months

past, invariably gives signs of positive electricity when the morn-
ing evaporation commences in fine weather, and precisely the same
phenomena recur when the dews of evening are beginning to be
deposited. An opinion has, I believe, been advanced to the effect
that these minor evolutions of electricity are to be referred merely
to *evaporation.* In a proximate sense, it is true, this admits of no
objection, but what are we the wiser for such a conclusion in re-
gard to ultimate principles? Had evaporation *alone* been neces-
sary to the production of such electric phenomena upon a small
scale, we should insure their appearance in an increasing ratio by
the magnitude of our experiments ; and the powerful jet of steam
from my tin still ought not only to have caused free divergence in
the leaves of an electroscope, but to have yielded strong sparks, on
the presentation of a knuckle to the large insulated conductor
used on the occasion. There are then, I think, by fair inference,
other essential conditions involved in the subject of our enquiry ;
these are yet worth searching for, and in the mean time (as regards
the phenomena of the engine boilers) I should be loth to decide
that they were wholly independent of the effects of *high pressure.*
I am bound to acknowledge that I had no immediate means of
ascertaining with precision the amount of pressure under which
my steam jet issued from the generator, and though confident that
it must have been very considerable, I can easily conceive that it
might yet have been vastly short of the requisite degree of *com-
pression* upon which the production of electric phenomena are, in
a measure at least, dependent.

Sandwich, January 20th, 1841.

BRITISH ASSOCIATION PROCEEDINGS.

On the Theory of Electricity, by C. J. KENNEDY, Esq.

When an electrical current is passing through an imperfectly
conducting medium, such as atmospheric air, the electrical par-
ticles being retarded, must be accumulated in the track which
is traversed by that current. This fact supplies the means of
determining whether there are two electric fluids, or only one.
If there were two fluids, the particles of each would be accu-
mulated in the line of discharge. If the velocity of the two
currents were uniform and equal, each section of the track tra-
versed would necessarily contain as many of the vitreous as of
the resinous electrical particles. Hence when such a double
electrical current was passing between two similar metallic wires,
a light body suspended midway between the wires should re-
main unmoved, supposing the electrical intensity of the two
wires to be equal ; for it would be urged with equal force in
opposite directions. But if there is only one electric fluid, that

electric fluid being retarded, and its particles accumulated in
the line of transit, the air situated in that line must become po-
sitively electrified in the central section ; and throughout the
line the positive state must predominate.   Hence, a light body
placed midway between the two wires ought to be urged to-
wards the negative wire. Probably the proof alleged to establish
the reality of a direct impulsive power in the particles of elec-
tricity may be entirely illusive.   But, independently of any
direct impulsion by the electric particles, the light body should
be urged towards the negative wire.   Probably the proof alleg-
ed to establish the reality of a direct impulsive power in the
particles of electricity may be entirely illusive.   But, indepen-
dently of any direct impulsion by the electric particles, the
light body should be urged towards the negative wire.   For
the current of air emitted from the positive wire being superior
in length and intensity to that emitted from the negative wire,
must be urged onwards with superior force under the influence
of the attraction and repulsion of the two opposite wires. The
positive wire repels, and the negative wire attracts every posi-
tively electrified aerial particle ; while, on the other hand, the
positive wire attracts and the negative wire repels every nega-
tively electrified aerial particle.   But as the positively electrified
aerial particles exceed the negatively electrified aerial particles
in number and intensity, the united force of the former particles
must be superior to the united force of the latter.   Hence the
aerial electrified current proceeding from the positive wire must,
on the theory of a single fluid, be superior in force to the elec-
trified aerial current proceeding from the negative wire.   It is
so ; and this fact decides the point at issue.   A light broad-
vaned wheel W (fig. 1st), delicately suspended, carefully ba-
lanced, and placed exactly midway between the two wires P
and N, moves from P, the positive wire, towards N, the negative
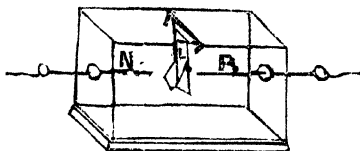wire, when an electric current is transmitted through the wires.

Fig. 1.   N   W   P
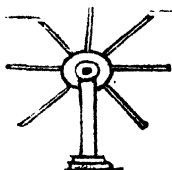
The same sort of result is obtained by means of the Cratos-
cope, a very simple and effective instrument, which is represented

in figure 2. This instrument

consists of an oblong box, of which the sides, ends, and cover are of varnished glass. Two similar metallic wires, P and N, pass through holes drilled in the ends, and are moveable, so as to admit of being accurately adjusted at any required distance from L, a slip of gold leaf suspended from a thin metallic flap attached to a wire which passes across the middle of the glass box, where an opening is left in the cover. Over this opening a plate of glass is laid. The slip of gold leaf is thus completely protected from being agitated by motions in the neighbouring air. It obeys the slightest impulse of the electrified aërial currents which are emitted from the wires P, N, when these are electrified. This is effected by connecting the rings of the wires P N with two equal wires about four feet long ; the one of which is inserted into a hole in the end of the positive, and the other into a hole in the end of the negative prime conductor of the electric machine. The wires P N are then accurately adjusted to equidistance from the leaf L. Previous to the principal experiment, it is ascertained—by a simple and satisfactory experimental test—that the attractive powers of the wires P N are precisely equal to each other. The electric machine is then worked, when it is found that the gold leaf instantly moves *from* the wire P towards the wire N. This shows the power of the electrified aërial current thrown off by the positive to be superior to that of the electrified current thrown off by the negative wire. On reversing the connections, the direction of the movement in the leaf still is *from* the positive towards the negative wire. This experiment contradicts the theory of two fluids, and establishes the theory of a single fluid. It is an *experimentum crucis ;* it decides the point in dispute. No hypothetical force has been introduced into the preceding explanation— nothing has been employed but the known electrical powers. If the vitreous electric fluid is supposed to be less retarded in traversing air than the resinous fluid, the result will be altered, but will be still more hostile to the theory of Du Fay ; for then the resinous electricity ought to predominate in the aërial interval, and the light body should be urged towards the positive wire, not from it, as is the fact. There are other *experimenta crucis,* all concurring to establish the theory of a single electric fluid. Mr. Porret and M. De la Rive found that a line of water lying between the opposite wires of the voltaic battery was urged from the positive or vitreous towards the negative or resinous wire. When a strong saline solution possessing considerable conducting power was employed, in place of the water, the same result was not obtained. The reason

is obvious, The electricity was little retarded, and therefore was
not accumulated to any sensible amount in the line of transit be-
tween the oppositely electrified wires. The aqueous line did not
become sensibly electrified, and therefore could not be urged from
the positive or vitreous wire by the repulsion of that wire, and the
attraction of the opposite wire. The results completely harmonize
with those obtained when the electric transit is made through an
interval of atmospheric air. When an insulated card, C, is pierced
by the electric

discharge passing between two equidistant and equally electri-
fied knobs, P N, it is perforated at a single spot, where a hole
is formed, having around it two burrs, one on each side of the
card. This proves that at the instant of perforation the parti-
cles of the card are disrupted by a divulsive force acting in
both directions. On there being but one perforation made,
nothing need be founded. The passage of one electric fluid, or
of two, through the perforated spot, might alike explain the
occurrence of a single perforation, provided that the simulta-
neous passage of *equal* quantities of the two opposite electrici-
ties through one single spot of the card, were capable of origi-
nating *there* a disruptive force among the particles of the card.
But the simultaneous addition of equal quantities of the two
electricities to the spot which they traverse, must leave that spot
still in the neutral state. Why then should the particles of the
card in that spot burst asunder? Why, on the theory of two
fluids, should there be a perforation made at all? On the
theory of one fluid, there out to be a perforation; for the elec-
tric particles being suddenly arrested in their motion through
the card, must be accumulated in the spot through which they
pass, that spot must become intensely electrified *plus*, and there-
fore its corpuscles must have a strong tendency to burst asun-
der, according to the law that " similarly electrified bodies repel
each other." And, since the one knob is electrified positively,
and the other negatively, the former knob must at the moment
of rupture repel, and the latter attract, the disrupting particles
of the card. It is so, even when every precaution has been
taken to insure equality in the intensity of the opposite knobs.
The superior length of the vitreous spark, and the superior ex-
tent of the light at the positive point, might also be pleaded
in corroboration of the evidence already adduced;—that evi-

dence fully justifies the conclusion that there is only one electric fluid. The theory of a single electric fluid is capable of assuming two forms, in which material idio-repulsion is entirely discarded. The one form is that of an original theory adopted by Mr. Kennedy, in the year 1825. It was deduced from a rather complicated fluxionary calculation, by which a beautifully simple result was obtained. An exponential fluxionary equation, involving all the possible powers and simple functions of the electric force, was employed. The result of the calculation was surprisingly simple, namely, that electrical action varies in the inverse ratio of the electric quantity; or, $A \propto \frac{1}{q}$.

Let A' represent the attraction of a material corpuscle for electricity, in any given electrical condition, suppose the neutral state, and q the quantity of electricity which that corpuscle then contains. The tendency of two material corpuscles c c' towards each other, may be denoted by T, and is $= 2 A'q = A' \times q + A' \times q$. Now, if the electrical quantities of c and c' become each $=x$, the attraction of each of these corpuscles for electricity will become $= \frac{A'q}{x}$, and T will become $= \frac{A'q}{x} \times x + \frac{A'q}{x} = 2A'q$, as before. That is, the joint tendency of the two corpuscles to mutual approach remains unaltered, so long as their electrical quantities are equal to each other, whatever each of these electrical quantities may be, whether a large quantity, or a small. Suppose next, that the electrical quantity of c becomes $=x$, and that of $c'=y$, then their respective attractions for electricity will be $\frac{A'q}{x}$ and $\frac{A'q}{y}$; and T will be $= \frac{A'q}{x} \times y + \frac{A'q'x}{y}$, or $\frac{A'q'x}{y} + \frac{A'qx}{y}$. Now this must be greater than $2A'q$, in every case in which x is unequal to y; for if x is unequal to y, (because A' and q are constant quantities,) $\frac{A'qy}{x}$ must be unequal to $\frac{A'qx}{y}$. If x be greater than y, $\frac{A'qx}{y}$ must be greater than $\frac{A'qy}{x}$. Now $\frac{A'qy}{x} : A'q :: A'q : \frac{A'qx}{y}$. Whence, (Euclid's Elements, Book v. prop. xxv.) $\frac{A'qy}{x} + \frac{A'qx}{y}$ are greater than $2A'q$;[*] that is, the

* $8 : 4 :: 4 : 2$. Now, $8 + 2$ exceeds $4 + 4$. Again, $3 : 9 :: 9 : 27$; but $3 + 27$ exceed $9 + 9$.

least possible value of T is $2A'q$;—in common language, the tendency of the two corpuscles to mutual approach, is the least possible when they contain equal quantities of electricity. It follows also, that the more unequal x and y are, the greater must the sum of $\dfrac{A'qy}{x}$ and $\dfrac{A'qx}{y}$ be; that is,' the more the electrical quantities of the two corpuscles differ, the greater is their tendency to mutual approach. The phenomena of electrical attraction, repulsion, and quiescence, may be explained on this theory, in a manner perfectly satisfactory, and exceedingly easy.

1. QUIESCENCE.

Case 1.—*Let A B C D be four small balls, equal to each other, homogeneous, equidistant in the same straight line, and all in the neutral electrical state.*

C   A   B   D

k

o

◎   ◉   ◎   ◡

o

l
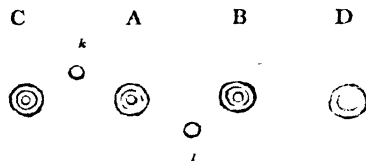
Then, according to Coulomb's law, the mutual attractions of A B C D will be the same in amount as if all the matter in each, and all the electricity in each, were collected at their very centres. Now, it is evident, that since the four balls are equal, homogeneous, and in the neutral state, they must all contain equal quantities of electricity, and must attract electricity with equal forces. A will attract B's electricity with as much force as that with which it attracts C's electricity. Also, B will attract A's electricity and D's, with equal forces ; consequently, A and B would be exactly balanced between C and D ; for the air around the balls could have no effect upon them, because the opposing actions of its various parts completely balance each other, so long as it remains throughout in the neutral state ;— thus take, on any side of A, and at any distance from A, a small portion, k, of the surrounding air; then opposite to k, and equidistant from A in the same straight line, there is an equal space or portion of air l, whose action must completely balance the action of k ; for A must attract k's electricity and l's with equal forces. Also k and l must attract A's electricity with equal forces. Hence it appears, that the influence of the

air around the balls may be left out of consideration, so long as that air remains in the neutral state. We have only to take into account the mutual actions of the balls A B C and D, and their electricity. These actions are perfectly equilibriated, and therefore the bodies must remain at rest.

Case 2.—*Let A be electrified plus, while C B D remain in the neutral state.*

Then it is evident, that as C and B contain equal quantities of electricity, they must attract A's electricity with forces precisely equal: and A must attract B's electricity and C's with forces precisely equal: consequently, A and B must still remain at rest.

Case 3.—*Let A be electrified minus, while B remains in the neutral state.*

Then since, as in the last case, C and B contain equal quantities of electricity, the forces which urge A towards B must be exactly balanced by the forces which urge A towards C: consequently, A and B must still remain at rest.

2. ATTRACTION.

Let A be positively, and B be negatively, electrified.

Then the material corpuscles in A and C, B and D, will tend towards each other with more than the minimum force, because these corpuscles contain *unequal* electrical quantities. But the material corpuscles in A and B will tend to each other with greater forces than do the corpuscles in A and C, B and D. Thus A will be urged towards C, and B will be urged towards D, with augmented forces. But the forces which urge A towards B will be augmented in a higher ratio, because the electrical quantities of their material corpuscles are more unequal than the electrical quantities of the material particles in A and C, B and D. In other words, + tends to — more forcibly than + tends to =, or than — tends to =; that is, the corpuscles which are electrified *oppositely*, tend more forcibly towards each other than towards the corpuscles which are in the neutral state. Thus A and B will acquire an increased tendency to separate, and they will also acquire an increased tendency to approach each other; but the tendency to approach augments in a higher ratio than the tendency to recede; and the air around the bodies can have no efficient action on them. Hence A and B must move towards each other, if left free to obey the electric forces.

3. REPULSION.

Case 1.—*Let A and B be alike electrified plus.*

Then the mutual attraction of their corpuscles will still be the *minimum*, because their electrical quantities, though altered,

are *equal* among themselves.   Hence the tendency of A and B
to mutual approach will be the minimum tendency correspond-
ing to 2 *A' q*.   But the tendency to mutual approach possessed
by A and C, B and D, must be greater than the minimum,
because the electrical quantities of their corpuscles are *unequal.*
Hence the bodies A and B, is free to move, must recede,—A
being drawn toward C, and B towards D, by the superior at-
traction.

Case 2.— *Let A and B be alike electrified minus.*

Then their mutual attraction will be the *minimum,* because
their electrical quantities are *equal.*   But the mutual attraction
of A and C, B and D, will be more than the minimum, because
their electrical quantities are *unequal.*   Thus, the forces which
tend to separate A and B are greater than the forces which tend
to bring them towards each other.   Hence, A and B, if free to
move, must mutually recede, being drawn asunder by the supe-
rior attractive forces which urge A towards C, and B towards D.

Corollary.—The greater the electrical intensity, the greater
must the electric action of small electrified bodies be.

Observation.—In large electrified bodies, inductive influence,
by affecting the distribution of their electricity, will modify
their attractive and repulsive tendencies— or, to speak with
greater correctness, their tendencies to mutual approach, or
mutual recession.

Hitherto, the case of four *homogeneous* balls has been consi-
dered ; but now let the exterior balls C and D be removed, the
spaces they have been occupying will not be left empty : these
spaces will be filled with air.   Now air contains electricity : air
obeys the same electric laws, and exerts the same electric influ-
ences, as other parts of ponderable matter do.   If, as is gene-
rally believed, electricity is distributed among bodies, not ac-
cording to specific affinities, but simply in proportion to their
volumes and surfaces,—then the electricity in the *equivalent*
spaces C and D must be the same in quantity with the electri-
city in A and B : and thus these aërial spaces or portions will
be quite as influential on the bodies A and B, as the solid bodies
were which they have replaced.   Further, the air in C and D
will be virtually *immovable* ; because when one particle of air
leaves either space, C or D, another particle of air must enter
in A's room, and perform the same functions.   Thus it appears
that it is quite possible to dispense with material idio-repulsion ;
and that a theory beautifully simple is capable of explaining all
the ordinary electrical phenomena.   The Franklinian theory,
when disencumbered of material idio-repulsion, and improved
by introducing the influence of the equivalent spaces, possesses
greater capabilities than the illustrious American electrician
was aware of, being able to explain the recession of negatively

electrified bodies, and the quiescence of unelectrified bodies. This Mr. Kennedy showed by algebraic formulæ and arithmetical calculations. Mr. Kennedy added, that the phenomena of excitation by compression afforded *experimenta crucis*, from which it appeared, that the Franklinian theory, though far more flexile and useful than it is commonly thought, cannot be the true theory. These phenomena supported Mr. Kennedy's own electrical theory. He added, that by supposing certain diversities to obtain in the electrical *capacity* of material corpuscles, the phenomena of cohesion' adhesion, capillary attraction, chemical affinity, electro-chemical decomposition, might be explained.

Mr. Bryson asked Mr. Kennedy, whether force did not involve repulsion; and if so, why he denied the existence of electric repulsion, and yet admitted electric force ? Mr. Kennedy replied, that he conceived he might admit electric attractive force, and yet deny the existence of repulsive force, when he showed that the phenomena were the result of attraction alone.

On the Temperature of the Earth in the Deep Mines in the neighbourhood of Manchester. By Mr. EATON HODGKINSON.

Mr. Hodgkinson having, some years ago, received from Prof. Phillips, four thermometers belonging to the Association, got, through the kindness of the proprietors of the following pits, and other parties connected with them, experiments made upon the temperature of the earth in each of them :—The salt-rock pit, 112 yards deep, belonging to the Marston Salt Company, near Northwich, Cheshire; the Haydock Colliery, 201 yards deep, near to Warrington ; the Broad Oak Coal-mine, 329 yards deep, near to Oldham. In the latter pit, a thermometer placed in a hole three feet deep, bored in " metal," and closed at the aperture, was examined weekly by Mr. Swain for twelve months, the temperature varying from 57° to 58½° Fahr.—it being lowest from the beginning of February to the middle of May, and highest in September and October to the middle of November. The experiments above mentioned were made in 1837 and 1838, and the results mentioned at the Birmingham meeting ; but the Broad Oak pit having been increased in depth since that time, a thermometer was inserted in it, in a hole bored in metal, as before. It was in a place 408 yards deep, and indicated a temperature of 61°, remaining nearly constant for twelve months. Mr. Fitzgerald being recently engaged in sinking a deep coal-pit at Pendleton, two miles from Manches-

ter, Mr. Hodgkinson conceived this to be a favourable opportu-
nity for getting additional information on the subject of subter-
ranean temperature, and, on his application to the proprietor,
the engineer (Mr. Ray) readily made for him, during the sinking
of the pit, and afterwards in the workings, the experiments of
which the results are below.   At 408 yards from the surface,
the temperature, in a hole from three to four feet deep, bored
in dry rock, was 66°; at 450 yards deep it was 67°; and at
480 yards it was 69°.   In the workings at 461 and 471 yards
deep, it was in both cases 65°.   The mean temperature of the
air at Manchester, according to Dr. Dalton's experiments, is
48° Fahr.; and, as the pits above mentioned are not very far
from Manchester, the mean temperature of the earth at the sur-
face of each of them may be considered as 48°.   With that
supposition, the distance sunk for each degree of Fahrenheit
would be as below :—

In the rock pit.................... 32 yards.

Haydock coal pit............ 20   ,,

Broad Oak pit ....33.7 ⎱ 32.5 ,, = mean.
                            31.4 ⎰

Pendleton pit (shaft) ..23 2 ⎫
                        23.7 ⎬ 23.2 ,, — mean.
                        22.8 ⎭

Ditto   (in workings) ..27.1 ⎱ 27.4   mean.
                        27.7 ⎰

The mean from the whole being 27 yards for each degree of
temperature.

The President remarked, that Mr. Hodgkinson's results gave
the rate of increase of temperature greater near the surface,
and then decreasing, which did not agree with the results of
other observers: this, he conceived, arose from nearly the same
cause as that already remarked upon when Mr. Fox's report
was under consideration. Mr. Hodgkinson commenced reckon-
ing his descents or depths, not from the surface, but from
the plane of invariable temperature, which in these latitudes
was not far from 60 feet.—Professor Forbes illustrated simply
by a diagram how this caused the rate of increase at first to be
too high, and then to diminish.   He then alluded to the frozen
soil of Siberia, gave a description of it, and said, that it had
been sunk through to a depth of 382 feet without being pen-
etrated—that is, without reaching a temperature of 32°,
although the temperature of the surface was not below 18°.
In this case, the rate of increase was rapid.

On the Agency of Sound. By Mr. SHAND.

Much has been done towards preserving and improving vision ; on the other hand, comparatively speaking, nothing has been done towards preserving or assisting our sense of hearing. So much are we in the dark in regard to the economy of speech in apartments, that it is a matter of chance whether any building will answer the purpose intended, and frequently, when too late, it is discovered to be in a great measure useless. Mr. Shand then adverted to certain rules and principles by which sound, he said, was in a great measure governed. 1st. Sound is usually produced, in bodies more dense than the atmosphere, by sudden percussion, and the action of one body upon another ; and it is considered to be the result of different modifications of matter only. 2nd. Rapid agitation, causing the atoms or crystals of a solid by their extremities to act upon each other, creates sound, whether this action be occasioned by original impulse or by reflection. It is regulated by the principles of attraction and repulsion, and it cannot be produced or conducted in any case, without being preceded by vibratory action. 3rd. As the atoms or crystals of solids vibrate repeatedly, and ultimately return to their primitive positions, they produce more intense and continuous sound than fluids, the component parts of which pass each other, and do not return to their original positions. 4th. Hard bodies, as they conduct with more rapidity than fluids, must precede the atmosphere in action and sound, and give out their sounds to the more tardy conductors, consequently to the atmosphere. 5th. In conformity to the density of the atoms, their form, and the medium distance between them, is the intensity, duration, and velocity of sound. 6th. As all sonorous bodies, whilst they conduct or reflect, also create sound, it is obvious, that to preserve the original character of sounds, the reflecting or conducting body must, in its movements, accord in time with those of the body which produces or forms the original sounds. 7th. As vibration is necessary to produce or conduct, every still body must arrest sound, on the same principle that a body at rest being in contact with a wheel moving round its axis impedes its progress. 8th. Slow pressure compresses a few atoms only, but rapid percussion occasions action, re-action, and sound throughout hard bodies. 9th. A solid, to produce much sound without great impulse, must be of limited diameter, in one direction, for it vibrates most in this direction, because the atmosphere yields more than the solid. 10th. In all matter in a state of action, there are two distinct motions, the vibratory o tremulous, by which all the atoms throughout a body are agitated together, and the undulatory or oscillating, which consist of a certain number · of atoms, and determines each distinc sound in a body and in the ear. It is most important to under

stand in what manner and by what means these should be regu-
lated, as on this depends the consistency of reflected with original
sounds. 11th. The chief distinction between hard solids and
fibrous substances is, that the latter possess more of the adhesive,
and less of the repulsive principle—they require to be more dis-
tended in a longitudinal or superficial direction; and intensity of
sound is more by the extent of their excursions than molecular
action. This is the cause of the different effects that are expe-
rienced between wood and stone as the medium of support and
contact in railways. 12th. Fluids are more powerful conductors
than productors of sound, but conduct less rapidly than solids.
Their atoms or component parts pass each other, and do not return
to their original places as do those of solids: this accounts for
sound passing in all directions in the atmosphere; also why the
same degree of percussion produces more sound on hard
solids than in the atmosphere; and why, in transit, there is
less change in its original character. 13th. Sound is much
influenced by moisture in the atmosphere. Intensity and dis-
tance of transit are regulated more by the adjustment of par-
ticles than the proportion of moisture. For instance, it is
loud and passes furthest during frost, and at all times when
objects are seen to a great distance. This is peculiarly
perceptible within the tropics, and in this country in sum-
mer just as the sun sinks - under the horizon; but when
cold increases, and the particles of moisture become larger, these
effects are diminished. It follows, as a matter of course, that
its transit must be more or less rapid under such varying cir-
cumstances. 14th. Water conducts more powerfully and
rapidly than the atmosphere; and, so far as ascertained, with
increased effect, as it approaches the temperature of the human
body. This is exemplified in tropical rivers, and in the human
ear, where this fluid is the only body in contact with the nerve
of hearing, to which it must communicate sound consitently
with its original character. 15th. Sound is not produced by
the atmosphere alone without violent concussion, or being in
contact with a more dense medium. Being the offspring of
atomical action in bodies, and in degree in the ratio of their ad-
hesive and repulsive principles, it cannot be produced in a
vacuum nor in a still body; but the latter may, by concentrating
and confining a fluid, increase action and sound for a time after
a certain impetus Has been given to the fluid, in like manner as
a stream of water is increased in velocity by being confined.
As it not only operates differently in every different substance,
but undergoes certain changes by every change in the molecules
or surface of any mass of matter, there are no limits to the effects
that are produced by such changes.—It is difficult to reason on
the operations of nature and the motions and influence of matter
not perceptible to the eye. In the present case, however, said
Mr. Shand, we are enabled to judge, partly by our ocular faculty

and in part from our sense of hearing. That the vibratory and undulatory or oscillatory motions are not only prevalent in the musical string, but in all matter in a state of agitation, is indicated by the following facts :—1st. In a musical string of given diameter and tension, when set in motion, the extent of the undulations are in the ratio of the length of the string—each undulation gives out a distinct sound, conformable in duration to the extent of the undulation. 2nd. In the walls and ceiling of an apartment these principles of action are also equally apparent ; wherever there is an extended surface in any one place, the undulations are also extended, and these produce distinct sounds in the ratio of their extent. If the reflections of the human voice, by this means, be prolonged, the reflection of one letter falls upon the original sound of another letter, and occasions as much derangement as if one syllable or word were intermixed with another syllable or word ; as one letter differs in sound from another letter as much as do syllables or words. This is one great and leading error in the construction of places for public speaking ; and it is alone sufficient to show how fallacious the idea is, of relying on the mere form of an apartment, without attending to and regulating this action, in not only the walls and ceiling, but in every reflecting body in an apartment, especially in glass, which is the most sonorous material. 3rd. The same rules of action are exhibited in water. In the ocean, the reach of sea is regulated according to the expanse of water : where there is an indent in the land, the wave is extended, and the sound it produces is prolonged. Were this action regulated by the current of air only, the waves would pass in one uniform direction ; but this is not the case. 4th. These principles of action are, however, more perfectly defined in the atmosphere, through which sounds are transmitted with least change, and are preserved separate and apart from each other. If analogical reasoning is to be applied in this case, and it be admitted that sound is only produced by the action of bodies on each other, and ceases the instant these become still, there must be spherical intervals of rest during vibratory motion, in order to keep sounds apart, in conformity to their original formation. In most cases we reason as if the atmosphere were the productor, and the only medium of conduction, while we overlook the influence of the solid as a sonorous reflector. Because analogy is experienced in certain points, we endeavour to reconcile its properties to those of light, which, like heat, is diminished as it spreads ; but in sonorous solids, as it extends to new matter, by bringing additional atoms into action, sound is propagated, until action in these atoms is reduced by friction. When the influence of the church bell is more from a different direction to that of original sound, by being reflected from a distance by the walls of buildings, does

not the tremulous action of the atmosphere impinging on these
walls, bring millions of new atoms in the building into action,
and consequently new sounds are produced from a distance, and
in a different direction ? Having in view mainly the economy
of speech in apartments, I shall proceed to this part of my subject
to which the following facts are applicable :—An individual who
is so deaf that he is insensible to upwards of a thousand people
singing in church, on applying one end of a forked piece of wood to
his teeth, and the other end to the ledge of the division of the seat
before him, he is enabled by this to hear and join in the tune.
Now it is not merely the partial agency of this wood that is to
be considered, as, by the spread of the atmospheric vibrations,
the voice sets in motion every atom of every solid in the church,
and it is distributed throughout these with more rapidity and
intensity than by the air, which is incapable of communicating
the same measure of vibratory influence at any one given point ;
and it evinces that, being the more rapid and profuse conductor,
it is the wood that is most rapidly set in motion, and communi-
cates action and sound to the air in a room. If these observa-
tions be correct, nothing can be more erroneous than to suppose
that speech can be regulated within the walls of an apartment
without regulating the action of the solids which predominantly
govern it in this case. If sound predominates more in the fibre
of the wood of the stethoscope than in the aërial passage in it,
must not the same rule apply in a church, where the seats and
lathing are almost invariably of pine ? In the Albion Church,
in Glasgow, are exhibited the short undulation, which accords
with the articulate sound of the voice, and the lengthened un-
dulation, which, by prolonged sounds overcomes articulation.
On listening to a preacher there, when it was densely filled, my
seat was at an extreme angle in the gallery from the pulpit ; I
heard the speaker with perfect distinctness when he spoke in his
natural tone, as his voice was mostly reflected by the walls,
which are of solid masonry ; but when his voice was raised so as
to act with more force on the ceiling, the longer excursions and
undulations of the then hollow ceiling produced prolonged
reflections, which drowned speech. In St. Andrew's Church
very different effects are produced in the galleries and lower
part of it. In the galleries the ceilings are low and curved, and
the voice, acting within the curvatures, produces prolonged and
concentrated reflections (as in all such cases,) inimical to speech ;
the windows are much exposed to the voice, and the divisions
of the seats rise too much above each other, all which occasion
lengthened reverberations, to the prejudice of speech. The
asperities presented by the ornaments on the walls, and the
capitals of two ranges of Corinthian pillars, occasion harsh re-
flections, which are unpleasant. All these defects, are however,
lost in a great measure, in the lower part of the building, where

little inconvenience is experienced. As the detection of error points the way to truth, I shall now advert to the defects *in two churches. First, Dr. Lee's, in St. Giles's, Edinburgh, in which the General Assembly met, but were obliged to abandon it as their place of meeting. The body of this church is of considerable length, but narrow, and the walls being deep and near to each other, the vibratory and undulatory actions operate powerfully upon the voice of a speaker. In these side walls are immense Gothic windows opposite to each other, and between these echo must sport like boys at battledore and shuttle-cock. A few feet behind the pulpit is a large window, and a vertical sounding-board, parallel with the back part of the pulpit; while the recess where the pulpit is placed on one side of the nave of the church is wainscoted to the height of about four feet. The wooden floors are mostly hollow or vaulted underneath, and the lower edges of the divisions of the seats on the ground floor rest on the hollow flooring; so that the whole of this concatenation of glass and thin deal boards are arranged as if the architect had intended to produce as much vibratory action as possible, and, consequently, sonorous reflections. Such are the effects, that the preacher is very indistinctly heard at the distance of twenty feet, and there are two galleries at the extremities of the church which are locked up as useless. Similar causes produce similar effects in St. Luke's Church in Liverpool. Here there is a locomotive pulpit, for the purpose of rolling the preacher from place to place; but there is even a gross evil in this vehicle, which accompanies it and the speaker to whatever point he may be conveyed. The canopy over his head is a deep hollow body, formed of thin deal; it is literally a drum, as may be understood by striking it with the knuckle of the hand, and produces deep hollow sounds, operating in a transverse direction, and most prejudicially on the voice of a speaker. But the chief cause of confusion is metal windows with large panes of glass; the crystals of both these bodies, having similar action, are more sonorous than wood and glass combined. The divisions of the seats rest on porous freestone, and the foundation of this building, like others in the same locality, is probably on sandstone, and both these give additional effect to other sonorous materials connected with them. The chancel of St. Luke's is much narrower than the body of the church, therefore the windows are brought nearer together, and to the person officiating at the communion table; consequently, the reflections from these must so overcome his voice, that he cannot be understood by the congregation in the nave of the church. It is not by creating additional or increasing reflected sounds, but by bringing the action of the reflecting surrounding solids to move in time with the mechanism by which speech is produced, and, by this means,

reflected sounds to accord with every distinct letter that the speaker pronounces; it is by shortening the action, and limiting the time of each distinct reflection from the glass, thin deal boards, &c., to the time in which each letter is formed by the speaker. This, in fact, however simple it may seem, must be effected, otherwise no form in the walls of an apartment for public speaking can accomplish what is necessary for the economy of speach. It is true, we are told by the late evidence before a Committee of the Commons, on Sound, &c., that reflections in aid of speech must be taken from one surface, and that surface possessing the properties of the pianoforte sounding-board; which is the principle of all others that I would avoid. It is precisely that which is adopted in the drum of the locomotive pulpit in St. Luke's on an extended and more prejudicial scale, as it must give out prolonged reflections in a transverse direction to that of the voice, which is predominantly delivered horizontally. To shorten the oscillations on ceilings, walls, and windows of places for public speaking, is not the only consideration; but, until this shall be effected, no material aid can be given to speech within the walls of a building.

Mr. Epsy read a paper to show that the four fluctuations of the barometer, which occur daily, are produced entirely by the increasing and diminishing elasticity of the air, due to increas ing and diminishing temperature.—When the sun rises, the air begins to expand by heat; this expansion of the air, especially of that near the surface of the earth, lifts the strata of air above, which will produce a reaction, causing the barometer to rise; and the greatest rise of the barometer will take place when the increase of heat in the lower parts of the atmosphere is the most rapid, problably about 9 or 10 A.M. The barometer, from that time, will begin to fall; and at the moment when the air is parting with its heat as fast as it receives it, the barometer will indicate the exact weight of the atmosphere. The barometer, however, will continue to descend on account of the diminishing tension of the air, and consequent sinking upon itself, as the evening advances, and its greatest depression will be at the moment of the most rapid diminution of temperature, which will be about 4 or 5 o'clock At this moment the barometer will indicate a less pressure than the true weight of the atmosphere. The whole upper parts of the atmosphere have now acquired a momentum downwards, which will cause the barometer to rise above the mean, as the motion diminishes, which must take place some time in the night. This rise will be small, however, compared with that at 9 or 10 A.M. As the barometer now stands above the mean, it must necessarily descend to the mean at the moment when it is neither increasing

nor diminishing in temperature, which will be a little before
sunrise. If this is a true explanation of the four fluctuations
of the barometer in a day, it will follow that the morning rise
ought to be greater at considerable elevations, provided they
are not too great, because some of the air will be lifted above
the place of observation; and •such•was found to be the case
by Col. Sykes in India. As this morning rise of the barometer
depends on the increasing elasticity of the air, and this increas-
ing elasticity, on heat, Mr. Espy proposed to the mathematicians
to calculate how much the whole atmosphere is heated from
sunrise till the time when the barometer stands highest, the
actual rise of the barometer being given. In this way meteor-
ology may assist astronomy.

Prof. Forbes doubted the correctness of Mr. Espy's views of
the cause of the great daily fluctuation of the barometer at
elevated stations ; for, towards two or three o'clock, the heat
being greatest, its effect in lifting up the inferior air to and
above the elevated station should then be greatest, whereas that
time of the day was nearer to the time of minimum height
of barometer than its maximum.

ELEMENTARY LECTURES ON ELECTRICITY.

LECTURE V.

When the electric force, of either a negatively excited stick
of sealing wax, or of a positively excited glass tube, is suffi-
ciently powerful, their polarizing influence may be extended
through a series of metallic rods, placed end to end, having a
plate of air, of about half an inch in thickness, between them.
If, for instance, we employ two of these rods, and place their
axes in the same right line, at 'about half an inch asunder,
as represented by fig. 3, and furnished with pith balls, we shall
have them ready for the experiments.

fig. 3.

Let us now excite the glass tube, and you will see that, when
it is presented to the outside ball of the rod B, in the manner
shown in fig. 2, page 172, the whole of the four pairs of pith

balls will diverge, showing that both the rod B and the rod A are electrical.   You will observe, also, that those two next to the excited tube, will not only diverge from one another, but they will also have a tendency to approach the tube.   And by paying attention to the other balls, it will be seen that those belonging to the inner extremities of the rods A and B, have also a tendency to come together ; that is, although the balls of each pair will diverge from one another, those belonging to the different rods *lean* towards each other ; and, indeed, very frequently come into contact with each other.

Now, from the facts which I have before brought to your notice, respecting the conditions under which bodies attract one another by the influence of electric forces, you will easily understand that the outer balls of the rod B, which have a tendency to approach the glass tube, must necessarily be in an opposite electric state to that tube, otherwise they would have no tendency to move in that direction.   Hence, they are negatively electrical ; and as they are in the same condition as that end of the rod to which they are attached, that part of the metal is also negative.   This is also the case with the balls attached to the inner extremity of the rod A, and consequently with that end of the rod itself : and as the balls attached to the inner extremity of the rod B, are attracted to those belonging to the inner extremity of A, those extremities of the two rods are differently electrical, and consequently the inner extremity of the rod B is positive.

The balls attached to the outer extremity of the rod A, and consequently that end of the rod itself, may be shown to be positively electrical by the usual means, already noticed, either by the application of a negatively excited stick of sealing wax, or by a positively excited glass tube.   And the same tests may also be applied to the two pairs of balls attached to the inner ends of the rods A and B.   The outer pair of balls belonging to the rod B, may also be tested by a negatively electric stick of sealing wax.

If, instead of employing two rods only, as in those last experiments, we were to place three or four in a row, every rod would become electro-polar, upon the principle above descrided. For instance, by presenting an excited glass tube, the nearest extremity of every conductor would become electro-negative, and their farther extremities electro-positive.   In such cases, the brass rods polarize one another after the polarizing of the first one, by the excited glass tube ; for the accumulated electric fluid at the remote extremity of the first rod, displaces the fluid belonging to the second one ; and the accumulated fluid at the remote extremity of the second rod exercises a similar action on the fluid of the third rod ; and so on through the

whole series. But as the polarizing action decreases with every additional rod, the series of rods which can be polarized by these means, is limited to a very few. But, in all cases, the polarizing effects are exalted by connecting the rod, most remote from the glass tube, with the ground by some good conductor; the reason of which is, that the resistance to the disturbance of the fluid, in the other rods, is lessened by giving free access to the ground to the fluid in the most remote rod.

This last fact will lead us to another by merely a trifling variation in the experiment. Let us now employ one insulated brass rod only; and polarize it by the approach of a positively excited glass tube. Its pith balls, at both extremities, will diverge as usual. Now place a finger on the remote end of the metallic rod, and the balls at the nearest end will diverge more than before. Take away the fingers whilst the rod is still under the influence of the electric tube, and then gradually draw the tube away also. You will now observe that both pairs of pith balls first collapse, and when the tube is entirely removed from the vicinity of the rod, they again open, and remain divergent for some considerable time afterwards. Now test the electric state of the rod, and it is found that the whole of it is negatively electrical. Now the reason of this is very obvious when we consider some of the particulars of the previous experiments; for the application of the finger to the rod whilst under the influence of the excited glass tube, gave an opportunity for a portion of the electric fluid to depart from the rod; and by taking away the finger whilst the rod was still under that influence, the latter was left insulated with less fluid than it previously had possession of.

I must now point out another fact which very frequently attends many of the experiments I have hitherto offered to your notice, because if you were not acquainted with it you might probably, on many occasions, arrive at wrong conclusions respecting the electric characters of any electrized body presented to the electroscope. When the excited glass tube is kept presented to the brass arm of the electroscope for a minute or two, a portion of the fluid naturally belonging to the brass arm, is driven out through the medium of the asperities on the surfaces of the balls, and the fibres of the threads by which they are suspended: and, although the polarization of the brass rod holds good whilst under the electric force of the tube, when you withdraw the latter slowly, the polarization gradually subsides, and at a certain distance the force on the tube permits the balls of each pair to collapse and hang together; but when the tube is still further removed from the brass rod, its electric influence is no longer in operation, and the balls again diverge, being left in a negative condition in common with the rod to which they are attached. It is exceedingly important that this fact

should be well understood, because there is a strong probability that from a want of this piece of information, many errors have arisen by those who only occasionally employ an electroscope. I shall, very shortly have to describe electroscopes of much greater delicacy than that we have hitherto operated with ; but they are all susceptible of similar electro-polarization, and loss of fluid, which, if not taken into consideration, would necessarily lead to mistaken conclusions.

The employment of negatively electric bodies, such as excited sealing wax against fur ; for, in such cases, the remote extremities of the electroscope being negative by the polarizing influence of the wax, they draw in an additional quantity of the electric fluid from the surrounding air, which, when the wax is withdrawn, leaves them in an overcharged or positive condition, as may be understood by the usual tests with which you have now become familiar.

Hitherto we have employed brass rods only, for our illustrations of electro-polarization ; but we must now proceed with metallic bodies of other figures ; and we will commence with a thin metallic disc, which may be about six inches in diameter. It must be well rounded, and made smooth at the edge, and insulated on a glass stem with its plane in a vertical position. A disc of common tin, (tinned iron) neatly wired round its margin, answers very well for purposes of this kind : and a thread passing through a small hole a little above its centre, having a light pith ball at each end, answers very well for an electroscope.

The manner of polarising the disc of metal, is by approaching one of its flat sides with an electrized body : for instance, either an excited glass tube, or an excited stick of sealing-wax. The pith ball on that side next to the polarizing body, will approach it, and the other will recede from it.   Hence, you will observe, that both balls quit the surface of the metal, each being repelled by the surface to which it belongs.   They may be proved to be in different electric conditions by the different tests. When the polarizing body is excited smooth glass, the ball next to it is negative, and that on the opposite face of the disc is positively electrical.   Hence, the law of polarization of the thin disc is in accordance with that which is developed by the polarization of long pieces of metal ; and it is a matter of no consequence how thin the disc may be, the polarization of its two surfaces is as complete as if it were of considerable thickness ; one face being electro-positive, and the other electro-negative.

The electro-polarization of thin pieces of metal lead as to the employment of another piece of apparatus, called the electrophorous ; whose action could not have been well understood without a previous acquaintance with that fact.

The base M. M. of the electro-phorous, fig. 4,

Fig. 4
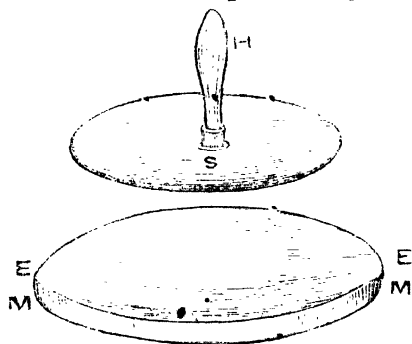
consists of a circular tin dish, with a vertical rim, about three
quarters of an inch high.   This metallic dish is to be filled with
some resinous matter, whilst in a melted condition.   Pitch
alone is too soft, otherwise it would answer for the resinous part
of the apparatus.   A mixture of about equal parts of pitch and
rozin, well incorporated with each other, form a very good
compound for this purpose.   Some employ lac, rozin, bees'
wax, &c., mixed together, but I never yet saw a better electro-
phorous than those made with a mixture of pitch and rozin.
The size will, of course, vary the electric power of the instru-
ment, but when a dish is about twelve inches diameter, the
power is sufficiently great for ordinary purposes.   Besides the
metallic base, and the resinous cake, there is also a disc of metal
S, of about two inches less diameter, with a glass handle H,
which form the cover of the instrument.   Hence, the electro-
phorous, consists of three principal parts.   The sole M, M, the
resinous cake E, E, and the cover S, which, by its ingeneous
inventor, M. Volta, is called *Scudio.*

The electrophorous is employed in the following manner :—
Excite the resinous cake, when warm and dry, by whipping its
surface with a warm and dry silk handkerchief   This process
gives it a negative electric action : and the excitation is best
accomplished when the sole M, M, is in good conducting con-
nexion with the ground.   Touching it with the finger does pretty
well, but resting it on your knee, whilst sitting, answers much
better

When the electrophorous has become excited, place upon
its surface, by taking hold of the glass handle, the cover S,
leaving a margin of the resinous cake uncovered.   In this posi-
tion, the inequalities on the surface of the cake, prevent the

cover from coming into general contact with it: which, instead
of partaking of the negative character of the cake, becomes
electro-polar from its influence : its lower side being electro-
positive, and its upper one electro-negative. Now touch the
cover with a finger, and a smart spark is seen passing from the
finger to the cover. Now take up the cover by its glass handle,
and present its edge to your knuckle, and again a spark passes
between them. You may now repeat this experiment many
times, by touching the cover each time before it is removed from
the resinous cake, without any further excitations. When the
apparatus is kept warm and dry, it will keep in action for several
days.

Prize Volumes of the Annals of Electricity, &c.

In order to stimulate and promote experimental inquiry, in
the various departments of Electricity and Magnetism, the
Editor proposes to offer prize volumes of the Annals, to those
experimenters who may be most successful in the following
subjects :—

1st. For a description of the most powerful soft iron, or
Electro-magnet, in proportion to the weight of the iron em-
ployed in its structure ; which is not to be less than 10lb. The
voltaic battery employed will be at the option of the experimenter ;
and is to be described by him, with the manner of using it in the
experiments with the soft iron magnet.

2nd. For the invention of an electrical-machine, more power-
ful, in proportion to size, than the usual plate, or cylindrical form.
A full description of the apparatus, with a suitable drawing, will
be required.

3rd. For an account of the most extensive and best conducted
experiments of the electricity of the steam of boilers of high or
low pressure engines ; with all the particulars respecting the
character of the water employed in each boiler ; and such other
particulars as may appear interesting.

4th. For the best mode of procuring Electro-type Plates, dif-
ferent from those published.

5th. For the best paper on any branch of experimental research
in Electricity or in Magnetism.

The prize for each of the above subjects will be Volume VI.,
of the "Annals of Electricity, Magnetism and Chemistry, &c."
bound, and gold lettered in the first-rate style, with a suitable
emblem and motto. To be presented to the successful candidates,
or their agents, (in London, if required,) on the first day of
August, 1841.

The communications on the above subjects are to be addressed
to Mr. William Sturgeon, Royal Victoria Gallery of Practical
Science, Manchester, on or before the 1st day of May, 1841.
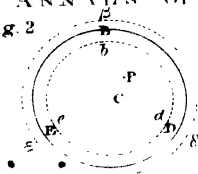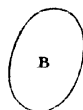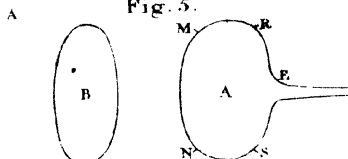
Fig 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.

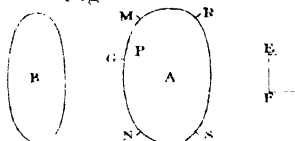Fig. 11.

Fig. 12.
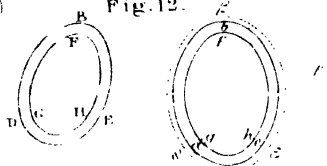
Fig. 14.
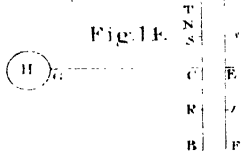
Fig. 17.
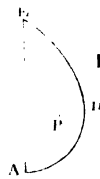
Fig. 15.

Fig. 16.

Fig. 19.

ELEVATION

Fig. 1.

lbs
2500

SECTION

Fig. 2.

PLAN

Fig. 3.

AN ELECTRO MAGNET.
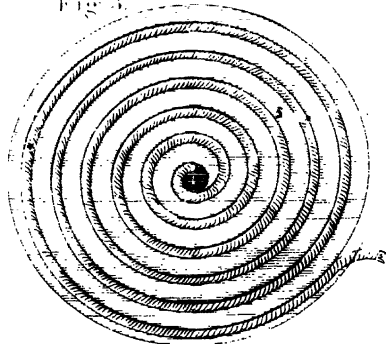
Invented by Joseph Radford, Esq.

Fig. 1

Fig. 5.

Fig. 6

Fig. 4

Fig. 3

Fig. 7

Fig. 2.

Fig.61

Fig.58

Fig.66
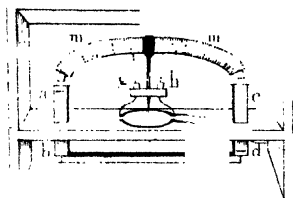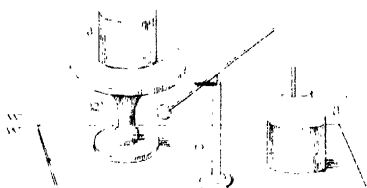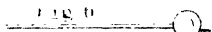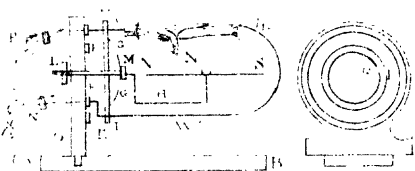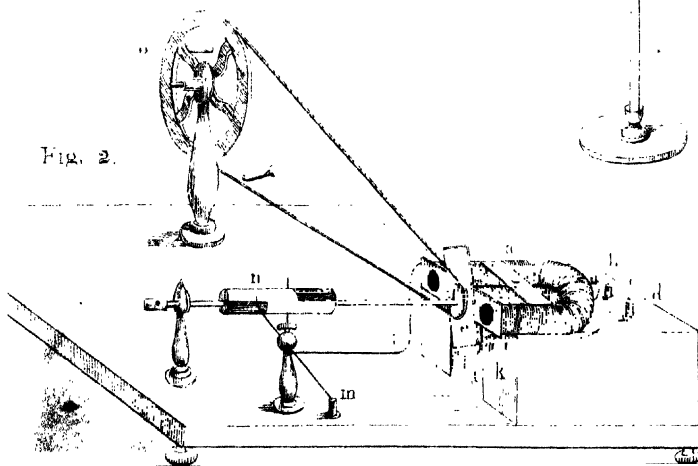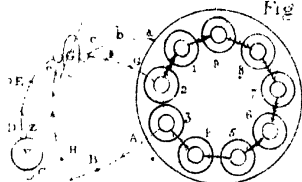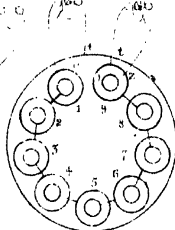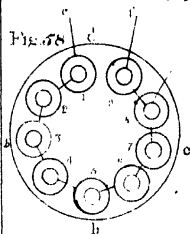
Fig.67
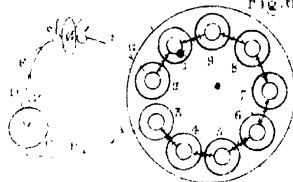
Fig.65

Fig.62

Fig.63

Fig.64
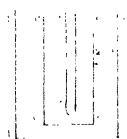
Fig.59.
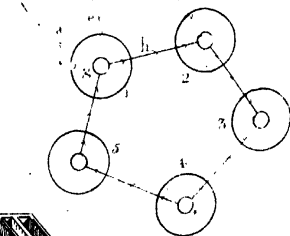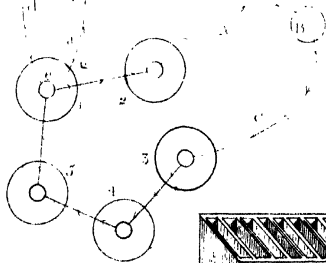
Fig.60.

VOLTAIC BATTERY

LIV. *Historical Sketch of the rise and progress of Electro-magnetic Engines for propelling machinery.*

To trace the invention of electro-magnetic engines to the first ideas which were formed in the minds of philosophers, concerning the probability of their structure and usefulne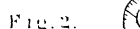ss, would be an undertaking which no one could possibly accomplish; but, as has been the case in many other inventions, it may probably have been at a much earlier period, in the history of electro-magnetism, than the date of any contrivance, for that purpose, which has hitherto been described. The electro-magnetic rotations. first suggested by Dr. Wollaston,* and actually performed by the ingenious contrivances of Mr. Faraday,† were depending upon forces too feeble to anticipate from them an accumulation of power to a sufficient extent to be applicable in propelling machinery; though, as we are well aware, that subsequent forms of the apparatus, especially the stellar wheel of Mr. Barlow,‡ and the rotating disc of Mr. Sturgeon § on an extensive scale, were subjected to trials for this purpose; but, although voltaic apparatus and horse-shoe magnets of great powers, were employed in these trials, the combined forces were found to be insufficient to keep even the wheels themselves in motion. Mr. Sturgeon's discovery of magnetizing bars of soft iron to a considerable power, and rapidly changing their polarity by miniature voltaic batteries,|| and the subsequent improved plan, by professor

* Phil. Trans. for 1823. Quarterly Journal of Science, Vol. XII. p. 79. Vol. XV. p. 289.
   † Quarterly Journal of Science, Vol. XII. p. 74.
   ‡ Barlow's Magnetic Attractions, 2d. Edit. Part III. p. 280.
   § Transactions of the Society of Arts. London. Vol. XLIII.
   || Transactions of the Society of Arts. Vol. XLIII. 1825.

Henry, of raising the magnetic action of soft iron,* developed new and inexhaustible sources of force which appeared easily and extensively available as a mechanical agent: and it is to the ingenious American philosopher, above named, that we are indebted for the first form of a working model of an engine upon the principle of reciprocating polarity of soft iron by electro-dynamical agency. Dr. Henry's machine is described in the twentieth volume of Silliman's American Journal of Science, in a letter to the editor of that excellent periodical. The following is a copy of Dr. Henry's letter.—

" On a reciprocating motion produced by magnetic attraction and repulsion: by Professor Joseph Henry.

To the Editor.

"Sir,
"I have lately succeeded in producing motion in a little machine by a power, which, I believe, has never before been applied in mechanics—by magnetic attraction and repulsion.

" Not much importance, however, is attached to the invention, since the article, in its present state, can only be considered as a philosophical toy; although, in the progress of discovery and invention, it is not impossible that the same principle, or some modification of it on a more extended scale, may hereafter be applied to some useful purpose. But without reference to its practical utility, and only viewed as a new effect produced by one of the most mysterious agents of nature, you will not, perhaps, think the following account of it unworthy of a place in the Journal of Science.

"It is well known that an attractive or repulsive force is excited between two magnets, according as poles of different names, or poles of the same names, are presented to each other.

"In order to understand how this principle can be applied to produce a reciprocating motion, let us suppose a bar magnet to be supported horizontally on an axis passing through the centre of gravity, in precisely the same manner as a dipping needle is poised; and suppose two other magnets to be placed perpendicularly, one under each pole of the horizontal magnet, and a little below it, with their north poles uppermost; then it is evident that the south pole of the horizontal magnet will be attracted by the north pole of one of the perpendicular magnets, and its north pole repelled by the north pole of the other; in this state it will be at rest, but if, by any means,

* Silliman's American Journal of Science. Vol xix, P. 329.

we reverse the polarity of the horizontal magnet, its position will be changed, and the extremity which was before attracted will now be repelled; if the polarity be again reversed, the position will again be changed, and so on indefinitely: to produce, therefore, a continued vibration, it is only necessary to introduce, into this arrangement, some means by which the polarity of the horizontal magnet can be instantaneously changed, and that too by a cause which shall be put in operation by the motion of the magnet itself: how this can be effected, will not be difficult to conceive, when I mention, that, intead of a permanent steel magnet, in the moveable part of the apparatus, a soft iron galvanic apparatus is used.*

" The change of polarity is produced simply by soldering to the extremities of the wires which surround the galvanic magnet, two small galvanic batteries, in such a manner that the vibrations of the magnet itself may immerse these alternately into vessels of diluted acid; care being taken that the batteries are so attached that the current of galvanism from each shall pass round the magnet in an opposite direction.

"Instead of soldering the batteries to the ends of the wires, and thus causing them at each vibration to be lifted from the acid by the power of the machine; they may be permanently fixed to the vessels, and the galvanic communication formed by the amalgamated ends of the wires dipping into a jar of mercury.

" The whole will be more readily understood by a reference to fig. 4, Plate XI, where A B is the horizontal magnet, about seven inches long, and moveable on an axis at the centre; its two extremities when placed in a horizontal line, are about one inch from the north poles of the upright magnets C and D. G and F are two large tumblers containing dilute acid, in each of which is immersed a plate of zinc surrounded with copper. *l*, *m*, *s*, *t*, are four brass thimbles soldered to the zinc and copper of the batteries and filled with mercury.

"The galvanic magnet A, B, is wound with three strands of copper bell wire, each about 25 feet long; the similar ends of these are twisted together so as to form two stiff wires, which project beyond the extremity B, and dip into the thimbles *s, t*.

"To the wires *q*, *r*, two other wires are soldered so as to project in an opposite direction, and dip into the thimbles *l, m*. The wires of the galvanic magnet have thus, as it were, four

* For a method of constructing the galvanic method on an improved plan, see my paper in Vol, XIX. p. 329, of this (Silliman's) Journal.

A description of Prof. Henry's method of making soft iron magnets by voltaic electricity will soon appear in these Annals. EDIT.

projecting ends; and by inspecting the figure it will be seen that the extremity *m*, which dips into the cup attached to the copper of the battery in G, corresponds to the extremity *r*, connecting with the zinc in F.

" When the batteries are in action, if the end B is depressed until *q*, *r*, dips into the cups *s*, *t*, A B instantly becomes a powerful magnet, having its north pole at B; this of course is repelled by the north pole D, while at the same time it is attracted by C, the position is consequently changed, and *o*, *p*, comes in contact with the mercury in *l*, *m*: as soon as the communication is formed, the poles are reversed, and the position again changed. If the tumblers be filled with strong dilute acid, the motion is at first very rapid and powerful, but it soon almost entirely ceases. By partially filling the tumblers with weak acid, and occasionally adding a small quantity of fresh acid, a uniform motion, at the rate of 75 vibrations in a minute, has been kept up for more than an hour, with a large battery and very weak acid, the motion might be continued for an indefinite length of time.

" The motion before described, is entirely distinct from that produced by the electro-magnetic combinations of wires and magnets; it results directly from the mechanical action of ordinary magnetism: galvanism being only introduced for the purpose of charging the poles.

" My friend Professor Green, of Philadelphia, to whom I first exhibited this machine in motion, recommended the substitution of galvanic magnets for the two perpendicular steel ones. If an article of this kind was to be constructed on a large scale, this would undoubtedly be the better plan, as magnets of that kind can be made of any required power, but for a small apparatus, intended merely to exhibit the motion, the plan here described is perhaps the most convenient."

This ingenious invention of Professor Henry does not appear to have been any further pursued, nor any improvement in the original instrument to have yet taken place, though it is obvious that for some purposes, where a reciprocating motion is wanted, as in the pistons of pumps &c., it might be brought into play with more facility than most other forms of electro-magnetic engines that have hitherto made their appearance.

In 1835 Dr. Schulthess published a small work in which he speaks at considerable length about Dal Negro's* apparatus, or rather about the unintelligible manner in which it is des-

* Dal Negro's was rather for experiments than for work.

cribed ; and then gives a description of one which he himself had made and exhibited in a lecture which he delivered to the Philosophical Society at Zurich in February, 18:3. Dr. Schulthess's miniature machine consisted of two horse-shoe soft iron electro magnets, placed horizontally with their ends opposite to each other, with a small space between for the play of another small magnet which performed a pendulous motion. The whole was supported upon a four-legged wooden frame resembling the frame which supports the plate of an air pump. See Taylor's Scientific Memoirs Part IV. p. 534.

The first rotatory electro-magnetic engine of which there is any account on record, is that invented by Mr. Sturgeon, as described in the first volume of this periodical.* This engine was constructed in 1832, and appears to be the first of this class which gave motion to working models of machinery or, indeed, to which any model of the kind was attached. This engine would draw light loads on a railway, pump water, saw thin pieces of wood by means of small models which it worked with considerable facility : and though the batteries were of small dimensions, yet when the zinc cylinders were amalgamated,† and enveloped in either a bladder, pasteboard, or paper case, to prevent the transmission of the mercury to the copper surface, and also to prevent the admixture of the exciting liquids‡ employed, the action would continue for some considerable time. But the force produced by these means, though it would last much longer, it was far from being so energetic as when dilute nitrous acid alone was employed for the exciting liquid of the battery. The moveable part of Mr. Sturgeon's engine works on a vertical shaft, which carries four steel magnets, whose attractions and repulsions operate in concert, and in unison with the attractions and repulsions of four vertical soft iron magnets, whose forces are brought into play by the electric currents from the two batteries : and the reversal of their poles is accomplished by four wires which, in succession, come into contact with four quadrantal copper plates.

* Annals of Electricity, &c. Vol. I. p. 75.
† The history of amalgamated zinc being employed in voltaic batteries, will be found in Vol. I. p. 81.
‡ Sir H. Davy appears to have been the first philosopher who employed two or more different exciting liquids at the same time in the voltaic battery. He was thus enabled to construct batteries which consisted of one metal only. Nicholson's Journal, quarto, Vol. V. p. 78, 341. Phil. Trans. for 1801, and 1826.

Professor H. Jacobi " constructed a magnetic machine for continued circular motion," in May 1834,* which was described in a note presented to the Academy of Sciences at Paris, in November of the same year.† Professor Jacobi published an excellent essay on electro-magnetism as a motive force, and on its application as a first mover in machinery. A translation of this essay, from the French original, by Mr. Lang, will be found in the first volume of these " Annals," commencing at page 408. Jacobi's engine consisted of sixteen soft iron magnets, eight of which were fixed to, and rotated with, a horizontal shaft or spindle ; the other eight being fixed to a steady frame in such a manner that their poles might be approached as close as possible, without being touched by those of the moveable magnets in their rotation. The whole of these magnets were brought into play by four voltaic troughs, whose electric currents traverse spiral conducting wires which surround the iron bars. Besides a description, with an illustrative plate, of this engine, much curious and interesting information will be found in the author's essay.

At the meeting of the British Association at Dublin, in 1835, the Rev. J. W. Mc.Gauley produced an electro-magnetic engine which attracted considerable attention. The author proposed to augment its power by several improvements which he then had in contemplation. At the meeting at Bristol, the following year, Mr. Mc.Gauley again brought forward his engine, and detailed an extensive series of experiments, the results of which did not hold out to him much further prospects of making his apparatus applicable to general use.

Professor Callan, of Maynooth College, who has published some valuable papers on electro-magnetism, was constructing a large engine on this principle in June, 1836.‡ This engine was tried towards the latter end of the same month, but was found defective in its first form : but by a new arrangement it gave rapid rotation to a wheel which weighed about one hundred pounds.§ From the prospects afforded by this small engine, Professor Callan was induced to proceed to the structure of one of much larger dimensions ; which was to be worked by forty electro-magnets, and expected to propel a

* Annals of Electricity, &c. Vol. I. p. 410.
† Ibid. p. 409.
‡ Annals, Vol. I. p. 378.
§ Ibid, 494.

carriage and its load weighing thirteen hundred weight, at the rate of seven or eight miles an hour. The author has entered on some interesting calculations on the probable advantages of electro-magnetic engines, which the reader may consult at page 494, Vol I. of these Annals.

In November, 1836, Dr. C. Page, of Salem, United States, who is the author of several beautiful pieces of electro-magnetic apparatus, turned his attention to engines of this kind; and published an account of his progress, in the American Journal of Science, for October, 1837. The revolving iron electro-magnets in this philosopher's contrivance, are of the horse-shoe form: and arranged on arms, or radii, at right angles to the rotating shaft, like the spokes of a wheel; having the poles outwards and nearly in the circumference of a circle in which the stationary magnets are fixed. Dr. Page mentions a large engine which he was then constructing, whose revolving apparatus would weigh nearly a hundred pounds. See these Annals, Vol. II. p. 216.

In April, 1837, Mr. Sturgeon announced his having succeeded in propelling a boat, and also a locomotive carriage by electro-magnetism. Annals of Electricity. Vol. I. p. 250.

The electro-magnetic engines constructed by Mr. Davenport of Bandon, near Rutland, Vermont, United States, were first announced in the American Journal of Science, for April 1837 (See Annals of Electricity Vol. II. p. 257). Since that time various accounts of these engines have appeared through the same medium, and others by the medium of the Journal of the Franklin Institute; all of which have been transferred to the pages of these Annals. (See Vol. II. p. p. 257, 284, 347, &c.) Mr. Davenport's efforts, in bringing electro-magnetic engines into repute, have been strongly supported by the liberality and patronage of Mr. Ransom Cooke, and the apparent success with which they were attended, has been the means of forming an electro-magnetic association in America.[*] In February 1837, Mr. Davenport secured an American "patent for the application of electro-magnetism to the propelling of machinery" in his own name;[†] and in the Summer of 1838, a Mr. Coombes was sent to this country with a locomotive engine of Mr. Davenport's construction, for the purpose of securing a similar patent for these realms.[‡] We are not aware why Mr. Coombes returned to America without accomplishing the object of his mission; but we imagine that

he could have little difficulty in discovering that Mr. Davenport had been anticipated, in almost every particular, in this country. It would occupy too much room, in this place, to pourtray an outline of the various accounts which have been published of the performances of Mr. Davenport's engines, nor indeed would it be necessary to do so, since they are to be found complete in the different parts of these Annals to which we have already alluded.

In the American Journal of Science, for October, 1837, Dr. B. R. Mc.Connell gives an account of an electro-magnetic engine, which works upon the principle of the electro-magnetic wheel between the poles of a horse-shoe magnet. We refer the reader to Vol. II. p. 123, of these Annals for the author's own account of this instrument.

An exceedingly ingenious arrangement of electro-magnets of soft iron was invented by J. P. Joule, Esq. of Salford, Manchester, in 1837, and described in these Annals in the January No. for 1838. (See Vol. II. p. 122.) The bars, in this beautiful instrument, are of a peculiar construction, and the transposition of their polarity effected by an exceedingly ingenious contrivance. Mr. Joule proposes to apply his engine both to locomotive carriages and to boats. The description is accompanied by two neat figures of illustration. Mr. Joule also mentions another small engine which he was then constructing; it was to consist of forty magnets.* A description of another engine with a series of interesting experiments by the same gentleman, will be found at the latter part of this article.

The Rev. F. Lockey describes a very pretty arrangement of magnets for an engine, in a letter to the Editor of the Annals of Electricity, dated June 12th 1838. (See Annals Vol. III. p. 14.). Mr. Lockey's plan not only differs from every other yet published, but appears to be well adapted for efficiency of power. The moveable magnets, forty-eight in number, are of soft iron with the usual wire coils for the transmission of electric currents; and arranged in eight groups of six in each group, on the revolving central shaft. The fixed magnets are of the same kind and number, and also arranged in eight groups around the circle described by the outer poles of the moveable ones, in such a manner that both systems of magnets may operate mutually on each other and produce rotatory motion. See fig. 9, Plate I., Vol. III.

About the latter part of the year 1837 a variety of ingenious contrivances, in the form of rotating engines, were to be seen in the different philosophical instrument makers' shops in London. Some of them had miniature saw mills, pumps,

and other pieces of machinery attached, and put in motion by them. Mr. Watkins gave a description of one in the Phil. Mag. for February, 1838, consisting of four vertical steel magnets, placed at equal distances from each other in the circumference of a circle; and four small iron electromagnets which were fixed at right angles to and rotated with, a vertical spindle, situated in the axis of the system. Mr. Palmer, of Newgate Street, has long had in his window a very great variety of rotating engines, with attached models of machinery which are kept in motion by them. It is highly probable that every individual instrument maker has some peculiarity in the mode of fitting up this class of apparatus.

We close this general historical sketch of all that has hitherto been accomplished in the application of electro-magnetism as a motive power, with the following description of another ingenious contrivance by Mr. Joule.

Description of an Electro-magnetic Engine, with experiments, in a letter to the Editor of the Annals of Electricity, &c.

New Bailey Street, Salford, December 1, 1838.

Dear Sir,

In Vol. II. p. 122 of your interesting work. is a communication of mine, describing a method of making electro-magnetic engines. which I thought might be adopted with advantage. I hoped that when the engine I was then constructing was finished, I should be able to send you a detailed account of its duty. The engine was completed in the summer: it weighs 7½ lbs. The utmost power that I have succeeded in developing, on working it with a battery of forty-eight four-inch plates was equal to raise 15 lbs. a foot high, per minute.*

The very inferior power of the engine convinces me that the distances between its poles was much too little, and that the magnets should have been fewer in number, and of much greater bulk.

I was desirous, before I attempted to make another engine, to satisfy myself by experiments, conducted on a smaller scale, how far it was possible to increase the velocity of rotation. Now, of the many things which prevent an infinite velocity, the resistance which iron opposes to the instantaneous induc-

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* In this estimate the enormous friction of the engine was accurately measured and reckoned as the load; the velocity of the moving magnets was about 3½ feet per second.

tion of magnetism is of considerable importance. I think I shall be able to show how this may be obviated in some measure.

The power of the electro-magnetic machine is much increased by the insertion of a bundle of wires instead of a solid nucleus of iron into its coil; this phenomenon is evidently occasioned by the peculiar texture of the wires, which allows Mr. Sturgeon's magnetic lines[*] to collapse with greater suddenness.

With a view to determine to what extent the velocity of rotation might be increased by the use of wire magnets, I constructed an apparatus as represented by fig. 1, Plate XIII., where *a b*, *c d*. are the steel magnets; *e f*, brass screws with very minute holes in their ends to receive the fine points of the steel axle on which the electro-magnet *m m* is fixed; *g*, *h*, are mercury cups to connect the wires of the electro-magnet with the pieces of watch spring *i*, *k*, which dip in the mercury cells; *w*, *w*, are wires which connect these cells with the battery; the distance between the poles *a*, *c*, was eight inches. I had four electro-magnets, which, with their spindles &c., could with great expedition be put off or on the machine, by means of the screws *e*, *f*.

No. 1 electro-magnet was made of a round bar of iron 1090 grains weight; No. 2 of nineteen iron wires of about $\frac{1}{16}$ inch diameter, weighing 642 grains: no particular pains were taken in annealing the iron and wires of these magnets. No. 3 and No. 4 magnets were made of iron and wire of the same quality and dimensions, but were annealed with very great care by a process in use among the manufacturers. Each of the magnets was first enveloped with a double covering of muslin, and then wound in the same manner with eleven yards of copper wire of $\frac{1}{16}$ inch diameter, well covered with silk : care was taken to make the friction of the pivots equal in each.

Here are the results of some experiments which I made with this apparatus. The numbers given in the table are revolutions per minute.

	No. 1. Hard iron.	No. 2. Hard wire.	No. 3. Soft iron.	No. 4. Soft wire.
With a single constant battery.	146	177	196	192
With two constant batteries arranged for intensity.	233	274	283	321

* See Mr. Sturgeon's Theory of Magnetic Electricity, Vol. I. p. 251, 277.

Ditto with weaker charge.	196	173	224	209
Mean.	192	208	234	241

These experiments were frequently repeated with similar results, and every precaution was taken to guard against error. I obtained much greater velocities than those given above, which I rejected partly because I could not count them, and partly because the resistance of the air began seriously to affect the results.

Some allowance should be given to the wire magnets; for while the weight of iron in them was little more than half that in the others, their bulk and, consequently, the resistance of the air remained the same.

The sparks and shocks, on breaking the battery circuit, were hardly sensible in No. 1; twice as great, at least, in No. 2 and 3; in No. 2 they were a little greater than in No. 3; but by far the most brilliant and powerful in No. 4.

I intend to make another engine soon, and shall construct its magnets of wires drawn from rectangular holes; if I have more success with it than I had with my last, you shall hear from me again.

<div style="text-align:center">

I am, Dear Sir,

Yours truly,

J. P. JOULE.

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LV. *Upon Telegraphic communication, especially by means of Galvanism. By* Dr. Steinheil, *Professor of Mathematics and Natural Philosophy, at the University of Munich, &c. &c. &c.**

Telegraphic communication may in its most general sense be defined as the method employed by one individual to render himself intelligible to others, and being, when viewed in this light, synonymous with intercourse is no human discovery, but one of the most wonderful gifts of nature, not to man alone, but in common with him to all social creatures is granted the faculty of communicating his sensations to others, and of exciting in them conditions similar to his own. Communication is the most powerful tie of the living creation; it connects one individual existence with another, reproduces in one what is granted to all, thus forming out of individuals, species, which in their turn present themselves as organic beings.

* Translated from the German by Julian Guggsworth.

There is assuredly nothing in nature more calculated to call forth our admiration than the contemplation of the variety of the means employed for attaining this mutual intelligence. From the undeciphered hieroglyphic signs of insects up to the complicated speech of man we observe a series of the most varied possibilities of reciprocal intercourse. In man, however, this gift of nature has attained an astonishing development, a development which in the form of speech and writing keeps pace with the march of his improvement, a development to which, as in the case of that improvement itself no absolute limits are assigned, and which equally with that improvement will struggle against those fundamental shackles of matter, time and space till it extends them, fixed though they be, up to the unchecked ranging of mind.   And as writing lays shackles on the passing sound and redeems it from fleeting time, so in like manner are the remotest distances to be annihilated and thoughts to be interchanged in an instant with those afar.   The means of accomplishing this do not however lie directly within our reach ; but by the patient observance of the powers and the phenomena of nature we render these subservient to us and make them the bearers of our thoughts, and it is this task which in the ordinary acceptation of the word is termed telegraphic communication.

In the works that have appeared, connected with this subject, we see that the natural phenomena thus applicable to the transmission of thoughts to remote distances are by no means few in number.   We likewise at the same time observe that the choice of the signals, representatives of the ideas we transmit, admits of much variety.   It must, however, be confessed that none of the proposed plans appear to amount to more than simple discoveries, and seem never to have originated in a leading fundamental idea of what conditions it is essential and indispensable to have in view.   It has hitherto been considered sufficient to add another new possible means of corresponding to those already known, without examining whether there are not yet other methods better suited to the purpose and of greater simplicity.   The more advisable course would be to establish at the outset what conditions are indispensable in complete telegraphic communication, and not to grapple with any difficulties with which the practical application of the scheme may be beset, for in most cases the method of carrying a plan into effect suggests itself to us as soon as we form a clear idea of what we have in view.   Let us now make the attempt.

What we propose to attain by telegraphic communication is the transmission of ideas with the utmost rapidity at all times and to any distance; and when the distances are but moderate this problem is at once completely solved by speech. Hence for more considerable distances nothing more is required than that the telegraph should imitate speech. It appears to be evidently not the shortest way to make the telegraph imitate an imperfect method of communication. The signs for instance made use of by the deaf and dumb; it should be made to copy the most perfect of all methods of communication—speech, in which the sound as it falls on the ear of itself arrests the attention, and is at once understood.

At the first blush this appears to be a very difficult task, inasmuch as oral communication has such different sounds at its disposal, and is hence enabled to convey notions by means of a few combinations: and this difficulty no former arrangement of the telegraph has been completely able to surmount. Inventors previous to Gauss were bent on producing a vast number of distinct signals, without reflecting that this was only to be attained at the cost of simplicity. It escaped them that a rapid communication is to be attained by other means besides a multiplicity of signs, and that in point of fact one single sign, if it can be repeated with sufficient rapidity and employed in groups properly combined, is all that we require. In order to explain this I should wish to enter upon the analysis of our written language.

I select the great Roman letters for this purpose. They consist of six different signs, namely, a straight line in four positions, horizontal, perpendicular, sloping from left to right, and from right to left, and lastly a semicircle turned to the right or left. Of these six different signs never more than four are met with in the same letter, as in M and W. Now if we calulate how many d.fferent letters may be formed out of these six signs, never using above four of them however in the same group, we find the number amounts to no fewer than 1554; whereas we require but 25. We see from this example how much more numerous than the case called for has been the amount of signs employed in our written language.

Let us now suppose that we have but two kinds of signs and see if these are not quite sufficient for a complete series of letters. Let the signs be as simple as possible, each a dot for instance, and let them be distinguished by their position, that is to say, by one being always on a higher line than the other. Now if it was an understood thing that in each sign there was not to be more than a single dot, it is clear there could be but two signs; if two dots might be united in the

same sign, four more new signs would have to be added to the two above, giving in all therefore six different signs. Assuming that three dots, but no more, might be combined for one sign we should get eight in addition to the above, making therefore in all fourteen distinct signs. Supposing, in fine, that four dots can be united for one sign would give us sixteen more signs, making in all therefore thirty, a number quite enough for giving not only all the letters but the numerals as well. Two signs may however be dispensed with, if instead of a second sign a quick repetition of a single one is employed. Hence we see that one sign, a dot for instance, enables us to render writing with greater conciseness than by the use of the various signs we ordinarily use ; and what *one* dot is as the simplest sign for writing, *one* sound is for speech. By repetition and by combination we may by habit form a language intelligible to the ear.

We are now therefore enabled to state the conditions which a telegraph of the simplest possible construction must fulfil. It need communicate but a *single sign* but must give that with the *utmost rapidity.* If it was besides to be got up in the most convenient method this sign should be made *audible.*

Let us now enquire in succession to what natural phenomena we may have recourse in order to make this single sign as we have described.

Light is only partially applicable to this purpose. Its transmission may, it is true. be considered as infinitely rapid. but we shall never be able to make it affect more than our sight. Moreover owing to the straight path it takes and the spherical shape of the earth, there are certain limits beyond which its use is not applicable. A telegraph whose signals are founded on the use of light can therefore only communicate with other stations when the attention of the observers at those stations is drawn to it. when the distance does not exceed a moderate number of miles, and when the weather is favourable. Notwithstanding these very confined limits imposed on its use the telegraph as constructed by Chappe has met with a favourable reception, and since 1793, when the first telegraphic line was established in France, been very generally adopted. It is remarkable that all the improvements proposed to be made in it should be but of minor importance. But although the principal defects, common to optical telegraphs, cannot of course be got over, yet it strikes me to be capable of being considerably improved. I shall here confine myself to the following.

The first thing must always be making the communications as rapidly as possible, and there are two methods of reducing

the time required for this. It can be brought about by steno-
graphic contractions, in other words, by the introduction of
numerous signals, but the employment of *a few* of which any
idea may be rendered. The same can be also attained by
diminishing the time required for giving the signal, and this
appears the preferable method. All the motions of long
levers, of which our telegraphs are composed, and of which
in order to be seen at a distance they must always consist,
require considerable time, in accordance with the laws of
dynamics. Hence it would be well to retain the present
mechanical arrangement of the telegraph no longer. The
proposal of Gauss, to which he was led by his experience in
the transmission of signals by means of the Heliotrope, appears
to me to be capable of being employed in practice without
difficulty.*

It is well known that a glass mirror of not above a few
square inches in size appears to the naked eye like a star at
the distance of five and twenty miles and upwards, when so
directed as to show an observer part of the image of the sun.
For the time there is no sunshine, during the night, or when
the weather is cloudy for instance, recourse may be had to
the glare produced by directing the flame of hydro-oxygen
gas upon lime. The signals would consist of flashes pro-
duced by turning or covering the mirror. This arrangement,
a more detailed account of which in this place would detain
us too long, would present the advantage of giving the signs
with great rapidity. It is easy to distinguish six separate
flashes of light in a second, and which on their disappearance
leave a corresponding impression on the memory, as is the
case with tones in rapid succession. We could therefore give
at least thirty signs by means of flashes, while one is now
given by the telegraph. There would further be the additional
advantage of dispensing with telescopes, and, what appears
to be of still more importance, only the observers at the stations
would notice anything of the signals. I do not however deny
that these advantages are more than balanced by insurmount-
able defects.

If we wish to carry on telegraphic correspondence without
the attention of those at the other stations being previously
called, we are *compelled* to affect the sense of hearing, whose
impressions are alone constant, and which, within certain
limits, allows us liberty of motion and permits us to occupy

* For a detailed description of the construction and use of the
Heliotrope of Gauss see "Breithaupts Magazin von den neuesten
Mathematischen Instrumenten" 2 tes. Heft. p. 47. Cassel 1835.

ourselves with other affairs.   But in order to produce signs that may be audible without difficulty, some piece of mechanism must be put in motion at the other station and made to strike on a bell or do something of that kind.

One would say it must be by no means an easy task to transfer a power to a great distance and cause it to perform there, certain functions at our pleasure and yet there are several possible methods of effecting this.   Sound, radiant heat, electric and galvanic currents, may, when applied in certain ways, be employed for this purpose.   The practical application however of each of these methods presents its own particular difficulties, and each carries with it its own inseparable defects, so that it appears advisable to examine them more closely in order to pronounce which of them presents the greatest advantages in its application.

We first come to the ordinary transmission of sound through the air, to the voice strengthened as it were by means of speaking trumpets.   But this manner of giving signals can only be employed with effect when the distances are but trifling.   The boatswain's whistle, the signal horn, and the alarm bell, are all similar methods, answering their particular purposes but inapplicable to telegraphic communication at considerable distances, inasmuch as any considerable increase of a sound would be too annoying to the immediate neighbourhood, but principally because the transmission of sound through the air, at the rate of about 1142 feet in a second, is not rapid enough.   The case is different in the transmission of sound through water.   Here the rapidity is nearly four times as great, and forms at least no longer an insuperable obstacle.   The experiments which Beaudent instituted at Marseilles and the more recent investigations of Colladon and Sturm at the Lake of Geneva* put it beyond a doubt that one can transmit audible tones through water to a distance of many miles.   The nature of the transmission of the tone is however of such a kind that one not only hears it but is also enabled to render it visible as a molecular motion by means of a particular apparatus, the lever of contact.   Consequently in place of the troublesome method of hearing under water a plan may be contrived which by the trembling motion of the particles of water shall repeat that motion on a larger scale, thus placing the mechanical power we require for producing a sound directly within our reach.   In this way of carrying on correspondence, and which we merely notice here

* Ann. de Chimie et Phys. XXXV. 113.   Gilbert's Annalen, LXXXVIII. 39.

in passing, it appears not unimportant when compared with telegraphs founded on optical principles, that the communications may be made under all circumstances. Consequently should it be established that sounds are transmitted through the plane surfaces of the water we have all around us, of which there is hardly a doubt, and if the employment of this method might therefore be generally applicable, the subject will merit a more complete investigation, the more so from its not requiring any particular conductor, for the transmission of the sound, and from the circumstance of no interruption of the signals being to be anticipated.

Another possible method of bringing about transient movements at great distances without any intervening artificial conductor is furnished by radiant heat when directed by means of condensing mirrors upon a thermo-electric pile. A galvanic current is called into play which in its turn is employed to produce declinations of a magnetic needle. The difficulties attending the construction of such an instrument though certainly considerable are not in themselves insuperable. Such a telegraph however would only have this advantage over those based on optical principles, namely, that it does not require the constant attention of the observer. But like the optical one it would cease to act during cloudy weather, and hence partakes of the intrinsic defects of that instrument; this method therefore is not probably able to stand a comparison with others.

The three principles of telegraphic intercourse here brought forward, namely, light, radiant heat, and the propagation of sound, have this in common, they do not require that there should be a direct connexion between the two stations. The air, water, and the ground, form the natural conductors of the conditions excited or rendered available, and which serve for making the communications. In this essential circumstance they differ advantageously from other feasible plans which we will now proceed to consider at some length. The velocity with which frictional electricity is transmitted along metallic conductors called forth as long ago as in the last century the idea of employing it for telegraphic communications. Winkler[*] at Leipsic in the year 1746 discharged several Leyden jars through a wire of considerable length, and on that occasion the river Pleiss formed a part of his circuit. Le Monnier in Paris[†] produced shocks through a length of wire amounting

* Priestley's History of Electricity, p. 59.
† Phil. Trans. Vol. XLIV. p. 1, 290.

to 12789 feet. Watson* extended the experiment over a space of four miles near Shooter's Hill, composing his circuit of two miles of wire and an equal distance of dry ground. Lomond† transmitted telegraphic signals to a neighbouring room by means of a pith ball electrometer acted upon by frictional electricity. Reiser‡ illuminated, by the electric spark, letters, formed upon plates of glass with strips of tin foil. Gauss§ makes mention of a communication from Humboldt according to which Bétancourt in 1798 established a communication between Madrid and Aranjuez, a distance of 26 miles by means of a wire through which a Leyden jar used to be discharged which was intended to be used as a telegraphic signal.

All these experiments put it beyond a doubt that frictional electricity may be employed for giving signals at any distances, and that when these signals are properly contrived they offer convenient means of telegraphic intercourse.

Frictional electricity has besides, as Gauss has already observed, the great advantage of not losing any of its force by increasing the length of the conducting wire, inasmuch as the whole of the electricity of one coating of the jar must traverse the entire length of the wire, be it what it may, to neutralize that of the other coating.

The experiments which Wheatstone instituted with a mirror in rapid motion to ascertain the rate at which the electric spark is transmitted through copper conductors, show that it travels with a velocity of 288,000 miles in a second,‖ a velocity exceeding that of the light of Jupiter's Satellites.¶ Using this as a standard, all distances upon our globe are done away with, and we accordingly see in electricity the instantaneous messenger of thought for all distances and in every circumstance. Nevertheless the practical execution of a telegraph founded on this principle presents some difficulty. Francis Ronalds constructed one at Hammersmith in 1816, and published a description of the instrument in 1823. In his arrangement there were clocks which kept time employed at the stations and which were furnished with a light disc of ciphers in place of hands, and having twenty different signs towards their circumference. At the moment the proper sign

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*· Phil. Trans. Vol. XLV. for 1748.
† Young's Travels in France, 1784, Vol. I. p. 79.
‡ Voigt's Magazin, Vol. IX. Part 1.
§ Gauss and Weber Resultate d. Mag. Veriens, Vol. II. p. 14.
‖ Phil. Trans. 1834, p. 595.
¶ 192,500 miles in a second, Brewster's Optics, p. 2.

the water decomposed was to serve as the telegraphic signal. It should be borne in mind that our knowledge of the effects of the galvanic pile was at that period but very limited. The discoveries of Oerstedt in 1819 from which we learnt that a magnetized needle can be deflected by a galvanic current, have opened a fresh field for the application of this power to telegraphic intercourse. Under this head may be comprised the hints for a galvanic telegraph thrown out by Fechner in his Manual of Galvanism,* and Ampères scheme of an electromagnetic telegraph which has been executed on a small scale by the late Professor Ritchie.† In this plan however Ampère departs so totally from the desirable principle of simplicity that upwards of sixty metallic connexions would be required for his telegraph. The arrangement which Davy‡ has proposed, in which illuminated letters are shown by the removal of screens placed in front of them, appears also to be far too complicated. The experiments instituted by Schilling§ by the deflection of a single magnetic needle seem much better contrived, he did not however succeed in surmounting the mechanical difficulties that attend the question in this shape. The subsequent discovery of Faraday, according to which the generation of galvanic currents is reduced to the mere motion of multipliers placed near fixed magnets has very materially simplified the problem, inasmuch as the voltaic pile becomes thereby no longer necessary.

To Gauss and Weber‖ is due the merit of having in 1833 actually constructed the first simplified galvano-magnetic telegraph. It was Gauss who first employed the excitement of induction, and who demonstrated that the appropriate combination of a limited number of signs is all that is required for the transmission of communications. Weber's discovery that a copper wire 7460 feet long which he had led across the houses and steeples at Gottingen from the observatory to the cabinet of Natural Philosophy required no especial insulation was one of great importance. The principle was thereby at once established of bringing the galvanic telegraph to the most convenient form. All that was required was an appropriate method of inducing or exciting the current with the

* Lehrbuch des Galvanismus 1829, p. 269.

† Froriep's Not. Vol. XXVII. No. 6. p. 86.

‡ Mechanics' Mag. No. 754, p. 261; No. 756, p. 296; No. 758, p. 327.

§ Allgemeine Bauzeitung 1837, No. 52. p. 240.

‖ Gött. gel. Anz. 1834, p. 1273, and Schumacher's Jahrbuch, 1837, p. 38.

power of changing its direction without having recourse to any
special contrivances for that purpose. In accordance with
the principles we have laid down, all that was required in
addition to this was to render the signals audible, a task that
apparently presented no very particular difficulty, inasmuch
as in the very scheme itself a mechanical motion, namely, the
deflection of a magnetic bar, was given. All that we had to
do therefore was to contrive that this motion should be made
available for striking bells or for making indelible dots. This
falls within the province of mechanics, and there are therefore
more ways than one of solving the problem. Hence the
alterations that I have made in the telegraph of Gauss and
by which it has assumed its present form may be said to be
founded on my perception and improvement of its imperfec-
tions, in harmony with what I had previously laid down as
necessary for perfect telegraphic communication. I by no
means however look on the arrangement I have selected as
complete; but as it answers the purpose I had in view, and
it may be well to abide by it till some simpler arrangement is
contrived.

As an inductor or exciter I employ a rotating apparatus
whose construction, speaking in a general way, is similar to
those of Clarke, of London, the multipliers of which however
my inductor is composed, consist of a vast number of turns of
fine insulated copper wire ; and this arrangement is necessary
in order that the resistance offered by the thicker wire com-
pleting the circuit, even should it be many miles long, may
be but little increased. Of the galvanic influence excited
during the entire half-turn of the rotating double multiplier
only a small portion is employed, and that when it is at the
maximum of its energy. By this means the duration of the
current is but very short, an arrangement which therefore in
a manner can cause merely a momentary deflection of the
little magnetic bars employed for giving the signals. In order
to heighten the action of these indicators as much as possible
they are surrounded by powerful multipliers. Small detached
magnets are so placed near these indicators that they are
thereby brought back to their original position immediately
that the induced current ceases, or in other words, as soon as
the deflection has taken place : we thus are enabled to repeat
signals in very rapid succession. The same indicator can be
brought with ease to make five deflections in a second, suc-
ceeding each other as fast as the sounds of a repeater when
striking. Hence if bells are placed at the proper striking
distance from these indicators they will ring at every deflection
produced, and as it is quite immaterial at what part of the

wire, completing the circuit, the multiplier containing the indicator is inserted, we have it in our power to produce the sign excited by induction at any part of the course the wire takes.   Should it be desired that the indicator instead of producing sounds should write, it is merely required to adapt to one end of the little magnetic bar a small vessel filled with a black colour. and terminating in a capillary tube.   This tube, instead of striking on a bell, thus makes a black spot upon some flat surface held in front of it.   If these spots are to compose writing, the surface upon which they are printed must keep moving on in front of the indicator with a uniform velocity, and this is easily brought about by means of an endless strip of paper which is rolled off one cylinder on to another by clockwork.   This is in a general way the construction followed in the telegraph erected here, a fuller account of which now would be out of place, as it may be found on referring to the appendix.   As far as the employment of the telegraph is concerned, it may be fairly said to perform all that can be reasonably required of it.   The excitation of the current is produced by half a turn of the inductor, and is equally available at all times.   The sounds of the bells close to the person making the signals, and which being produced at the other station too are also audible there. become, by practice, intelligible as a language.   Should they however be overheard or misunderstood, the communication presents itself simultaneously written down.   This can be done with closed doors, without any but the parties concerned being aware of it: the communications may be made at any distances, and either by day or night without any appreciable loss of time.   There is. therefore, every reason to be content with the performance of the instrument.

It is not however to be denied that the establishment of certain conditions indispensable to its action is nevertheless a matter of some difficulty.   We allude to the connecting wire joining the stations.

It has been stated above that Ampère required more than sixty such wires, whereas thirty or so were sufficient for Sömmering.   Wheatstone and Cooke* reduced their number to five ; Gauss and, probably in imitation of him, Schilling, as likewise Morse† in New York, made use of but a single wire running to the distant station and back.   One might imagine that this part of the arrangement could not be further sim-

    * La France industrielle, 1838, April 5, p. 3.
    † Mechanics' Magazine, No. 757, p. 332.   Silliman's Journal for October, 1837.   Annals of Electricity, &c., Vol. II. p. 116.

plified, such however is by no means the case. I have found
that even the half of this length of wire may be dispensed
with, and that with certain precautions its place is supplied by
the ground itself. We know in theory that the conducting
powers of the ground and of water are very small compared
with that of the metals, especially copper. It seems however
to have been previously overlooked that we have it within our
reach to make a perfectly good conductor out of water or any
other of the so called semi-conductors. All that is required
is that the surface that its section presents should be as much
greater than that of the metal as its conducting power is less.
In that case the resistance offered by the semi-conductor will
equal that of the perfect conductor; and as we can make
conductors of the ground of any size we please simply by
adapting to the ends of the wires plates presenting a sufficient
surface of contact, it is evident that we can diminish the
resistance offered by the ground or by water to any extent we
like. We can indeed so reduce this resistance as to make it
quite insensible when compared to that offered by the metal-
lic circuit, so that not only is half the wire spared but even
the resistance that such a circuit would present is diminished
by one half. This fact, the importance of which in the
erection of galvanic telegraphs speaks for itself, furnishes us
with another additional feature in which galvanism resembles
electricity. The experiments of Winkler, at Leipzig, had
already shown us that with frictional electricity the ground
may replace a portion of the discharging wire. The same is
now known to hold good with respect to galvanic currents.

The enquiry into the laws of dispersion according to which
the ground, whose mass is unlimited, is acted upon by the
passage of the galvanic current, appeared to be a subject
replete with interest. The galvanic excitation cannot be con-
fined to the portions of earth situated between the two ends
of the wire; on the contrary it cannot but extend itself inde-
finitely, and it became therefore now only dependant on the
law that obtained the excitation of the ground and the distance
of the exciting terminations of the wire whether it was neces-
sary or not to have any metallic communication at all for
carrying on telegraphic intercourse.

I can here only state in a general way that I have succeeded
in deducing this law experimentally from the phenomena it
presents: and that the result of the investigation is, that the
excitation diminishes rapidly as the distance between the ter-
minal wires increases.

An apparatus can, it is true, be constructed in which the
inductor, having no metallic connexion whatever with the

multiplier, by nothing more than the excitation transmitted through the ground, shall produce galvanic currents in that multiplier sufficient to cause a visible deflection of the bar. This is a hitherto unobserved fact, and may be classed upon the most extraordinary phenomena that science has revealed to us. It only holds good, however, for small distances. It must be left to the future to decide whether we shall ever succeed in telegraphing at great distances without any metallic communication at all. My experiments prove that such a thing is possible up to distances of 50 feet. For distant stations we can only conceive it feasible by augmenting the power of the galvanic induction, or by appropriate multipliers constructed for the purpose or, in conclusion, by increasing the surface of contact presented by the ends of the multiplier. At all events the phenomenon merits our best attention, and its influence will not perhaps be altogether overlooked in the theoretic views we may form with regard to galvanism itself.

To sum up in a few words what are the results of what we have here brought forward respecting telegraphic communications, we see that, with the present arrangement of the apparatus, no principle can be brought into competition with the galvanic telegraph, but that the establishing the metallic connexion indispensable to its action, although now materially simplified, still presents great difficulties in practice.   Indeed such a connexion is only practicable where it can be constantly watched, as, for instance, in the vicinity of railroads.

For very considerable distances without intermediate stations, galvanic or electric excitation must, on account of their rapidity, be always the best power to have recourse to. For lesser distances it yet remains open to enquiry whether, with proper modifications, some of the other methods we have pointed out would not be preferable, as they dispense with a metallic connexion.

(To be continued, with two large illustrative plates, in the next Number.)

LVI *Summary of new researches on the liberation of heat through friction. By* M. Becquerel, *President of l'Academie des Sciences for the year* 1838.*

Bodies are considered as formed by the connexion of an infinity of molecules or atoms surrounded by heat which is opposed to their immediate contact abstraction made by

* Translated by J. H. Lang, from the Comptes Rendus &c., No. 7, 1838.

every theoretical idea on its nature. When its quality increases or diminishes, the distance between the molecules becomes greater or less, and the size of the body experiences corresponding variations. • •

It is also admitted that these same molecules are subjected to an attractive force which tends to bring them nearer each other, and which is, consequently, opposed to the repulsive action of the heat. A third force is also introduced in the constitution of bodies, viz., the attraction of each molecule for the heat which surrounds the neighbouring molecules. The collective force prevailing over the two others, the body remains solid ; if the heat increase, the molecules will at length acquire a certain motion, and the body becomes liquid. Finally, if the quantity of heat become sufficiently great, the body assumes the gaseous form. The molecules of the body being kept at greater or less distances in proportion to the reciprocal action of the heat, and attractive force, they must be separated by interstitial spaces in which the phenomena of light, heat, affinity, and molecular attraction operate. Hence, it is in these spaces that the imponderable agents contend with the material principles of the body.

Heat should in this have the principal place, for, according to its intensity and mode of action, it produces light and electricity, and sets in motion chemical affinities. Hence, it is evident we cannot too deeply study the properties of this agent, in its relations to the particles of bodies, if we wish to discover its immediate influence in every thing which concerns the natural phenomena of the most elevated order. These considerations suggested the idea of a series of experimental researches, which have produced some new results of which I shall endeavour to give a sketch without, as much as will be in my power, entering upon the technical details, which would be difficult to comprehend in a rapid lecture.

Let us take a body that is in equilibrio of temperature with the surrounding medium. If by any means we disturb this body, so as to make its molecules lose their natural position of equilibrium, it is very evident all the imponderable agents which are in the intermolecular spaces will be set in motion : whence there results a number of phenomena that the philosopher tries to analyze by the assistance of means which science places at his disposal. We shall first dwell upon the effects of heat when friction is the mode of disturbance employed.

We know that when two bodies are rubbed together, heat and electricity are disengaged ; are or are not these two effects, which are concomitant, dependant on each other?

We will answer this hereafter; for the present let us consider the effects of heat.

All we know of the production of heat by the mutual friction of two bodies is reduced to this: the two bodies become heated, and the quantity emitted is sometimes so great that it is sufficient to inflame combustible bodies. Thus it is that a wheel, turning rapidly on its axle, takes fire, and that the savage, with an address and dexterity which we do not possess, is able to light two pieces of dry wood by rubbing them briskly together.

Every thing leads to the belief that the effects produced are due to the motion of vibration, impressed by vibration on the molecules, which the following facts tend to prove.

When an alloy, composed of one part iron and two antimony, is subjected to the file, it immediately throws off bright sparks, which shows that the temperature is raised even to a state of white heat. The blow of the steel on a flint produces a similar effect.

M. De Rumford[*] in boring a cannon, placed vertically, obtained sufficient heat to boil water, placed in a conveniently situated cavity. This is almost all we know of the liberation of heat by friction: hence, we are totally ignorant what part each body takes in the production of this phenomenon, as regards its nature and the state of its surface.

To determine how each body intervenes, we must be able to remove the causes which hide the effect we have in view; unfortunately we cannot effect this thoroughly. In fact, when we rub two bodies more or less briskly together, without causing the contact to cease, there is evidently a transmission of heat from the one to the other. The quantity transmitted by each of them depends on the conductibility of the body, its capacity for heat, and the state of its surface. On the other hand, the heat liberated in a body cannot be immediately tried, previous to its transmission to the other body, by ordinary thermometers, since their indications are not instantaneous. However, it is possible to operate under circumstances which permit the removal of several of the difficulties mentioned above: we are then led to a series of facts of which we are about to speak.

The apparatus intended for the observation of these facts is composed of a thermo-electric pile, in connexion with an excellent multiplier. Its sensibility is such, that the difference of about $\frac{1}{160}$ of a degree between the temperature of the two faces of the pile, will cause the magnetic needle to deviate so that the angle may be appreciable.

[*] Count Rumford. EDIT.

To reduce the question to the most simple expression possible, we took bodies of the same nature, bad conductors of heat, equal in all their dimensions and differing only in the state of their surfaces.   These bodies were conveniently fixed to glass stands.   The rubbed surfaces were each placed in contact with one of the faces of the pile; when these two surfaces have the same temperature, the magnetic needle remains steady while the two thermo-electric currents being equal, and having contrary directions, destroy one another.   But when the temperatures are not the same, the magnetic needle immediately deviates, and the angle of deviation measures the difference of temperature.   The friction is produced with a briskness and pressure regulated by convenient apparatus, so that its intensity may be always known: the two bodies are quickly separated from each other, and immediately subjected to experiment.

There are the means of experimentalizing: let us now pass on to the results.

We commenced by trying the effect produced on the magnetic needle by the contact of one of the rubbed surfaces with one of the faces of the pile; an effect due to this face being heated.

Experiment proves that whatever may be the nature of the disc rubbed, be it a conductor or not of heat, the time the needle takes to attain its maximum deviation, provided this deviation do not exceed 60°, is always $10.''$ For deviations from 60° to 70° is $9\frac{1}{2}''$, and $9'$ for deviations from 7.5° to 90°.

The magnetic needle here acts as a pendulum which oscillates under the action of weight among small amplitudes, since the deviations are isochronic; but, nevertheless, with this difference, that in the pendulum, when the amplitude of oscillation increases beyond a certain limit, the time of oscillation increases equally, while the contrary takes place in the experiments we have described, viz., that the time diminishes in proportion as the amplitude increases from 60° to 92°. This result is bound to the propagation of heat and electricity in bodies.

We will now take two bodies of the same nature, equal and disposed, as before.   For example, two discs of cork, of which one has a smooth, the other a rough surface. If we rub them together in a regular and determined manner, and present them simultaneously to the two faces of the thermo-electric pile, the magnetic needle is immediately deviated, and the direction of the deviation indicates that the disc with the rough surface has taken more heat than the other, and that in a proportion which varies with the rapidity of the

friction. The same is the result when we rub a piece of polished against an unpolished piece of glass. Under the circumstances which we have operated, the first has taken half as little heat as the second. Hence, we see that the absorbing power of bodies influences the liberation of heat by friction: however, this rule is not general, for white satin takes more heat than black which has a greater absorbing power.

If we submit to experiment, bodies of a different nature, we obtain the following results: 1st, polished glass and cork; the first takes more heat than the second in the proportion of 34 to 5. 2d, unpolished glass and cork; the relation of temperature is 40 to 7. 3d, silver and cork; the first is heated above the second in the proportion of 50 to 12. 4th, caoutchouc and cork; the temperature of the first is to that of the second as 29 to 11, and so on.

From the numerous results we have obtained by the friction of bodies of different natures, we have not yet been able to draw from them any simple laws, considering the different causes which concur to the general effect. It only appears that the nature of the body, and the abstraction made by the conductibility, excercise an influence which the state of the surface does not always destroy.

We have not hitherto been able to discover the true causes of this influence, which depends on the nature of the bodies, and probably on the arrangement of their molecules; but it is even much to have distinguished it by experiment, since it gives us an element more than the theory of heat will be able to take henceforth in consideration. If we now seek for the relation which exists between the production of heat and that of electricity, by the mutual friction of two bodies, the following are the consequences deduced from recent experiments; the displacement of the parts of the surfaces rubbed, always causes a liberation of heat and electricity, two effects which are in a reciprocal dependence; this dependence is as yet so hidden that it is impossible to affirm whether one precedes the other, and *vice versâ;* at present we can only make conjectures on head; conjectures which tend to show that heat proceeds from electricity, when the bodies are of the same nature, bad conductors of heat, and differing only in the state of their surfaces; that which is heated the most takes the negative electricity, and *vice versâ.* When the bodies are different the effects become very complex, and can only be understood by having the results before our eyes.

New facts permit us to extend to light, the relations discovered between heat and electricity: they are furnished

by phosphorescence. We know that this phenomena is mani-
fested whenever the particles of bodies, which are bad con-
ductors of electricity, are disturbed by percussion, friction,
heat, light, an electric shock, or even when they are decom-
posed by a chemical action. These are precisely the causes
which also liberate electricity; but the phenomenon being mole-
cular, the recomposition of the liberated electricities around the
molecules, causes an infinite number of small sparks, which
together produce a faint light similar to phosphorescence;
whence we may conclude that phosphorescence has an electric
origin.

We were ignorant from whence the phosphorescence in
Lampyres and Infusoires proceeded, and whether it also could
have an electric origin; some important experiments of M.
Ehrenberg have shown it to us. This skilful physiologist has
just studied, with particular attention, the light emitted in a
dark place by Infusoires and Annelides, which render the sea
luminous in certain countries, and especially when a light
breeze agitates its surface. Having placed some water, con-
taining these animalculæ, on the object-table of his microscope,
he was very much astonished to find the faint light which
surrounded them, was only the union of a great many small
sparks which proceeded from all parts of their bodies, and
chiefly from the bodies of the Annelides. These sparks, which
succeeded one another with great rapidity, had such a resem-
blance to those we observe in electric discharges, that M.
Ehrenberg did not hesitate in establishing their identity
with them. He is also certain that the light emitted is not
due to any particular secretion, but to a spontaneous act of
the animalcule: and it is shown as often as we irritate it,
either by mechanical or chemical means, viz., by agitating
the water or pouring either acid or alcohol therein. This
is another analogy to the torpedo which only gives its
discharge when irritated. In the animalculæ, as in the
torpedo, the discharge recommences after a certain interval
of repose. May we not conclude from this similitude of
effects, under the same circumstances, that there is an identity
in the causes? We know beyond a doubt that this cause in
the torpedo is electricity; we must therefore admit that this
also produces the phosphorescence of Infusoires and Annelides.
It is very remarkable that luminous or other phenomena,
which depend upon electricity, are so much stronger as the
animals are smaller. It appears that this profusion of electric
fluid, emitted only by beings of an inferior order, is destined
to perform some other functions in those of a superior order.

May we not after this conclude, as M. Berzelius and other philosophers have advanced, that the light emitted in combustion, which gives place to so great a liberation of electricity, also results from the discharge of an infinite number of small sparks produced by the combination of the combustible burning bodies.

Hence we see that the relations which connect light, heat, and electricity, are every day extending and convincing us that these three agents which preside over the constitution of molecular bodies, proceed, by all appearances, from one principle of ethereal nature, spread over space and all bodies.

LVII. *Organic Chemistry. · Note on the Formométhylal.* *By* M. MALAGUTI.[*]

The formométhylal, discovered by M. Gregory, is a body of great importance, for it is connected, by its composition, with one of the most creditable theories of the æthers, giving it one of the strongest arguments.

The formométhylal is considered as a formiate of tribasic mythylic æther, analogous to the acetal which is also considered as an acetate of trebasic sulphuric æther.

In order to see if the formométhylal had the constitution which was attributed to it, I decomposed an atom of it in the expectation of obtaining an atom of the formic acid; but I only obtained half an atom of pyroligneous spirit, and also a particular body endowed with several characters which, for want of a profound examination, might be confounded with the formométhylal from which it is derived.

The composition of this new body which I call methylal, is exactly represented by $C^{12} H^{16} O^4$, and the density of its vapour is 2.6 or 4 volumes, while the formula of the formométhylal is $C^{16} H^{20} O^6$, and the density of its vapour is 2.4 or 6 volumes.

We see by subtracting the formula of the methylal from that of the formométhylal, there remains half an atom of formiate of methylene.

$$C^{16} H^{20} O^6 = \text{formométhylal.}$$
$$C^{12} H^{16} O^4 = \text{methylal.}$$

$$C^4 \quad H^4 \quad O^2 = \tfrac{1}{2} \text{ atom formiate of methylene.}$$
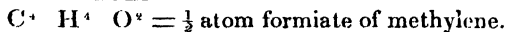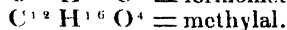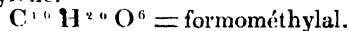
The methylal is limpid, has the same smell as the formométhylal, and requires about three volumes of water to dissolve

it; potassa separates it from its aqueous solution. It is soluble in alcohol, and at + 42 deg. c., under the pressure of 0.761 millim. Water being 1 it weighs 0.8551 at + 17 deg. c.

Notwithstanding the striking relation between the methylal and formométhylal, notwithstanding the density of this latter body, which sanction, if I may so speak, this relationship so evident and so simple, the formométhylal is only a mixture of the methylal and formiate of methylene.

If we distil some formométhylal, whose composition has been verified by very careful analysis, and divide the product of the distillation into fractions, we shall find that each fraction no longer presents the same composition.

The formométhylal which I have used in my experiments has given by analysis numbers which are confounded with those of calculation. On analyzing the first product of its distillation, I found in it 2.3p. 100 less carbon; and on the contrary, I found 1.1p. 100 more on analyzing the last product. As regards the nature of the methylal, my experiments are not sufficiently advanced to be able to decide, but I can say with certainty that the methylal, that by the action of certain oxidizing bodies, is converted into formic acid.

I cannot say whether pyroligneous spirits be formed at the same time or not, as my method of experimenting prevents me from insulating or even recognizing this body.

But whatever may be the nature of the methylal, it is as true as in formométhylal, far from being a tribasic æther as it has been supposed, it is only a mixture of formiate of methylene, and a substance which cannot be compared to the compound æthers.

As soon as I have completed the researches I have undertaken on the nature of the acetal, I shall have the honour of submitting to the judgment of the Academy, not only the results relative to this latter body, but also the whole of the details, an abridgment of which I am now permitted to present.

Organic Chemistry. Note on the products obtained by M. MASSON *from causing chloride of zinc to act on alcohol.*

M. Masson, an old pupil of the Normal School, and professor at Caen, had addressed to the Academy a memoir on the formation of æther which results from the action of chlorure of zinc on alcohol. He has just completed this work by some analyses of the products that he had obtained; made at the request and in the laboratory of M. Dumas, one of the

commissioners appointed to examine the first part of these researches.

M. Masson by distilling chloride of zinc with alcohol at about 160°, obtained an oil, the production of which did not cease until 200°. He compared it with sweet oil of wine, remarking that it could be separated into two parts of unequal volatility.

From another examination to which the author submitted these two products, the result was, says M. Dumas, that the least volatile ceases at about 300°, when it has lost all foreign matter. This product possesses all the characteristics of light sweet oil. Its analysis gave

$$\text{Hydrogen} \ldots \ldots \ldots 12.8$$
$$\text{Carbon} \ldots \ldots, \ldots 88.1$$

$$100.9$$

Which corresponds with $C^{32} H^{28}$ or with $C^6 H^7$. This analysis agrees with neither that of M. Hennel, nor of M. Sérules. It is nearly similar to that which M. P. Boullay and myself have given.

The most volatile product ceases at less than 100°, is very fluid, and its smell resembles that of naphtha; its composition is remarkable. It is the carbure of hydrogen, the most hydrogenerated liquid known. It contains

$$\text{Hydrogen} \ldots \ldots \ldots 15.7 \ldots \ldots 15.7$$
$$\text{Carbon} \ldots \ldots \ldots 84.5 \ldots \ldots 84.7$$

$$100.2 \qquad 100.4$$

Whence we take for our formula $C^5 H^9$. But from the density of its vapour, which is equal to 4.18, we may represent by $C^{32} H^{36}$.

Thus, there would be in M. Masson's oil two carbures of hydrogen which, by uniting, would reproduce bicarburated hydrogen from which they are derived.

$$\text{Volatile carbure} \ldots \ldots \frac{C^{32} H^{36}}{4} = C^8 H^9 \ldots 4.1$$

$$\text{Sweet oil of wine} \ldots \ldots \frac{C^{32} H^{26}}{5} = C^{16} H^{14}$$

Organic Chemistry. On the composition of Sugar of Gelatine; and on the nitro-saccharic acid of Braconnot. By M. BOUSSINGAULT.

Sugar of gelatine prepared in the usual manner by means of the reaction of sulphuric acid on strong size, according to

M. Boussingault always contains a soluble quantity of saline matters; so that after its combustion it leaves from 2 to 11 per cent ashes: but we may obtain it in a pure state by the decomposition of the soluble salt which it forms with baryte. For this purpose, a solution of sugar of glue is made to boil for some time with a milk of baryte; it does not disengage itself of ammoniac; it is filtered, and the baryte separated completely from the filtered liquor by means of sulphuric acid. It is afterwards evaporated to a pellicle and then the sugar quickly crystallizes.

Sugar of gelatine is rather more soluble in water than sugar of milk; like the latter it cracks under the teeth; its sweetness is rather intense and leaves a disagreeable taste behind. Once dried in a dry vacuo, at the ordinary temperature, it loses no more water at 130°.

The mean result of seven analyses, the details of which may be found in M. Boussingault's note, gives the following rough formula for the composition of sugar of gelatine.

$$C^{12} H^{41} O^{11} Az^6.$$
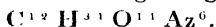
By causing a solution of sugar of gelatine to boil over powdered litharge, we obtain a liquid, which, filtered, presents an alkaline reaction, and, sufficiently evaporated forms by cooling a crystalline mass.

The mean of four analyses has given for this salt of basic lead, a composition which may be represented by the formula

$$C^{12} H^{47} O^9 Az^6, 3 (PbO),$$
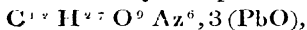
which compared with the preceding, would indicate that by combining with three atoms of oxide, the sugar of glue lost two atoms of water. Under this supposition the atom of anhydrated sugar would weigh 2517.0.

If we consider the combination of lead as neutral, the atom of the sugar will be reduced to 835.7 and the anhydrated sugar would be

$$C^4 H^9 O^3 Az^2.$$

But then the uncombined sugar must be represented by the preceding formula $+ \frac{3}{4}$aq.

The *nitro-saccharic acid* prepared by M. Boussingault, according to the process indicated by M. Braconnot, but with sugar of pure gelatine and with certain precautions indicated in the note, has given a composition which may be represented by the following formula:

$$C^4 H^{13} O^{10} Az^1$$

In the nitro-saccharate of copper prepared by the direct combination of acid with very divided oxide, then dried at

130°, the acid has been found in the same state as before the combination.

At this point of desiccation, the salt is of a pale blue; if we expose it to a higher temperature it takes a decidedly green tint. This colour appears at about 150°, and we perceive some water in the upper part of the tube containing the salt. At 180° or 182° the detonation takes place.

The salt, previously dried at 130°, and retained for several hours in vacuo at a temperature of 165°, loses 0.1771 of water in 100 parts.

The results of the analysis of the nitro-saccharate of anhydrated copper, permit the adoption of either of the two following formula for the composition of this salt:

$$C^2 \ H^3 \ O^3 \ Az^8 \ CuO, \text{ or } C^4 \ H^5 \ O^6 \ Az^4 2 \ (CuO):$$

but, says the author, it seems preferable to adopt the last of these formula, because it enables us to establish a relation with the sugar of gelatine.

The nitro-saccharic acid being $C^4 \ H^5 \ O^{11} \ Az^4$
If we take away the azotic acid $O^5 \ Az^2$

There remains $C^4 \ H^5 \ O \ \ Az^2$
The sugar of glue in the salts being $C^4 \ H^9 \ O^4 \ Az^2$

$$H^4 \ O^3$$

which shows that by combining with the azotic acid, sugar of glue loses more water than would be necessary for passing to the state in which it is found in the salts.   •

LVIII. *Memoir on a new electric condenser. By* M. Peclet.[*]

The new condenser is composed of three unpolished glass plates, which have been carefully ground together and completely covered with gold leaf fastened with albumine. One of these plates which I shall call A, is fixed to a common gold leaf electrometer; its upper surface is covered with varnish. The second which I shall distinguish by B, is placed on the first, and varnished on both surfaces. A small copper wire carefully gilded and unvarnished is fixed horizontally to a point in its circumference; it has at its centre, similar to the moveable plate of common condensers, a glass handle for moving it with. Lastly, a third plate C, is placed on this last with a hole in its centre through which passes the wire of the

[*] Translated by Mr. J. H. Lang, from the Comptes Rendus, &c. No. 9, 1838.

plate B; the plate C is only varnished on the lower side and its central orifice is supplied with a glass tube which surrounds the wire of the plate B, but of a less height.

The following is the method of using this apparatus. The upper plate is touched with the metal of which you wish to discover the action on the gold; the plate B is put in connexion with the ground; we suppress this connexion, raise the plate C and touch the A; this is repeated a certain number of times; at last by means of the wire on the plate B we raise B and C together; the gold leaves of the electrometer diverge, more or less, in proportion to the number of contacts.

The case which encloses the gold leaves is formed of parallel glasses, and placed on a trivet provided with a screw, on one side, a vertical plate pierced with a small hole, and on the other a portion of a circle divided vertically, whose centre is the same height as the hole in the plate and the upper extremity of the gold leaves; by looking through the hole in the plate we observe the deviation.

To give an idea of the power of this apparatus I shall relate two series of experiments. By touching the upper plate with an iron wire, after 1, 2, 3, 4, 5, and 10 contacts the gold leaves diverged $9\frac{2}{3}$, $20°$, $25°$, $31°$, $41°$, and $88°$.

By touching the upper plate with a platina wire, a single contact produced only a slight deviation, which was increased to $15°$ after three, and to $53°$ after twenty contacts.

The experiments with the platina were made by employing a platina wire, which had just been reddened in an alcohol flame, and washing my hands in distilled water. I was convinced beforehand, by making a great number of successive contacts in which I touched the upper plate with my finger, that the plates concealed no electricity.

The novel fact of the development of electricity by the contact of gold and platina, has also been directly proved by means of a simple condenser of extreme sensibility obtained by giving a sufficient thickness to the coats of varnish and making their surfaces perfectly smooth.

By means of the double and common condensers I have discovered that all the metals on which I have operated were positive with regard to gold, and these metals arranged in the order of their electro-motive power with regard to gold are as follows:

Zinc	Bismuth	Copper
Lead	Antimony	Silver
Pewter	Iron	Platina.

The effects produced by bismuth, antimony, and iron, differed so little from each other, that I could only classify them by taking the means of a great number of experiments.

It is evident from the arrangements of the apparatus that the quantity of electricity liberated, which causes the gold leaves to diverge, is proportional to the number of contacts ; but we find from numerous experiments that as far as about 20° the deviation is proportional to the number of contacts ; thus within this limit the deviation is in proportion to the *quantities of electricity.* It would be easy to make a table which should give the quantities of electricity *corresponding* to the deviations which exceed 20°, since these quantities are *proportional to the number of the contacts.*

The simple condensers or multipliers cannot however determine the relations of the effect produced by the contact of gold and different metals, since these proportions vary considerably with the thickness of the coats of varnish on the plates, of which I have satisfied myself by comparing the results of experiments made with different apparatus.

The instrument that I have the honour to present to the Academy, being of a sensibility somewhat indefinite. offers to philosophy a new means of investigation, which I hope will contribute to throw some light on the singular phenomena, which are produced by the contact of bodies.

LIX. *On the different phenomena of electricity at rest, and electricity in motion. By* Charles V. Walker, Esq.

1.   A paper was lately read before the Society, wherein the author endeavoured to prove, that homogeneous *attraction* and not *repulsion* exists between the particles of electricity.[*] He has candidly invited the opinions of others ; and, though adducing an experiment, the experiment to which the present communication will mainly refer, that at first sight seems to support his hypothesis, yet he intimates a readiness to abandon his position should it be found untenable.

2.   It may be urged, that the time for pursuing this subject was during the debate, which followed the reading of the paper ; and so, to a certain extent, it was.   But, I am sure it will be conceded me, that when a new opinion or experiment is first presented, it is not always possible to enter unprepared into the field of argument.   An *opinion* may be formed ; yet, oftimes the peculiar nature of the fact requires a train of thought, not to be pursued conveniently at a moment's notice.

3.   The authors of papers will, therefore, not conceive that the length of a debate is a measure of our attention to, and opinions upon, their communications.

[*] The paper alluded to is Mr. Griffin's. See Annals of Electricity, &c. Vol. III. p. 126.   Edit.

4. But our ideas may assume a more tangible form when meditated more at leisure, and it is greatly to be desired, that these our more *matured* opinions should be reduced to order—should be tested by experiment—and should be brought before the Society.   I would that, comet-like, every paper read here should leave a train, if not of *light*, yet surely of *reflection*, in its wake.

5. The main purpose of a Society being, that the mutual talents of members should be directed to any subject submitted to its consideration, it would tend greatly toward the *object and interest of these our evening meetings, were all present* to consider themselves as concerned to determine the truth or fallacy of those propositions, which are advanced here, and so to enter into the spirit of papers read before them, as to be prepared now or after with their yea or nay, their assent to or dissent *from the conclusions* adduced.

6. Conceiving the experiments offered in favour of homogeneous *attraction* as inconclusive, I have investigated them, or rather *one* of them, (the others being but modifications of this one) and have ventured to show the grounds of objection against their being a philosophical testimony in support of the author's hypothesis.

7. The experiment referred to is this :—

By suitable arrangements, two streams of electricity were made to pass in vacuo from two points, say A and B, to a ball, C.   It was invariably found that in their passage they converged, and entered the ball, C, in one compound stream, forming an appearance resembling the letter Y.

8. It would seem, that, unless homogeneous attraction be a fundamental principle of electricity, these two streams would have continued independent, pursuing the shortest course nor would have converged till they had reached the ball, C, forming in their passage a resemblance to the letter V.   But if, on the other hand homogeneous *repulsion* be (as we are anxious to prove) the fundamental principle, then surely they would diverge in their passage and present the form of letter U.

9. But it will occur to the attentive •observer, that one important element in this experiment has been overlooked, viz., *motion*.   An *important* element, because it leads to a branch of the science, unconnected with the matter in debate, to electro-*dynamics*, the laws and phenomena of which are entirely distinct from those of electro-*statics*.

10. In investigating the properties of the fluid, *per se*, it should first be accumulated, then left in a state of *rest*, and then examined.   The result of such an examination will, by

no means, correspond with the results obtained from the experiment now under consideration.

11.   The distinctive phenomena of the two states are so well known, that they scarcely need be mentioned ; yet, for the sake of method, 1 will avail myself of the following passage from Dr. Faraday's Researches (Third Series, § 267).

12   " The various phenomena exhibited by electricity may, for the purposes of comparison, be arranged under two heads : namely, those connected with electricity of tension, and those belonging to electricity in motion. This distinction is taken at present not as philosophical, but merely as convenient.   The effect of electricity of tension, at rest, is either attraction or repulsion at sensible distances.   The effects of electrical currents may be considered as 1st, evolution of heat ; 2d, magnetism ; 3d, chemical decomposition : 4th, phisiological phenomena ; 5th, spark."

13.   With the experiments, productive of these effects, all are so well acquainted, that it would be tedious to repeat them. They are mentioned merely to show *generally* the characteristic differences between the two conditions : they remind us how largely *motion* is concerned in producing the peculiar phenomena of electricity, and how very confined is its agency, (even though accumulated) when *at rest*.

14.   Our present object simply requires us to enter into those particular experiments wherein *attraction* and *repulsion* are concerned :—

Let the conductor of a machine, or a wire attached to the conductor sufficiently long to reach beyond the atmosphere of the cylinder, be connected with the earth.   In this case, however powerfully the machine may be in operation or however continuous and copious a current be passing along the wire, (providing it is not more than the wire is fitted to conduct) yet light substances will remain unaffected by its presence, although placed in the immediate vicinity.

15.   But this is not the case when the fluid is *at rest*. For if, instead of suffering it to pass off to the earth, it be permitted to accumulate in a Leyden jar, on testing its effects, by approaching a pith ball, the latter will be instantly *attracted ;* and, if insulated, as instantly *repelled.*

16.   This indicates an important difference between *rest* and *motion*. The *one* condition exercises a powerful disturbing influence upon neighbouring bodies ; the *other* is without influence ; at least so far as *attraction* and *repulsion* are concerned : and attractive and repulsive influence alone are we now examining.

17. There is one case, to which I will briefly refer, wherein both conditions, *motion* and *rest,* are involved. So far as my own information extends, attention has not been directed to it by any writer on the science. It is when a Leyden jar is in connexion with the prime conductor of a machine, while the cylinder is revolving.

18. In this case, it would seem that, so soon as the machine is put in motion, a current of fluid passes along the conductor to the jar. It there spreads itself in a thin layer, (if I am allowed the term) over the inner coating, over the surface of the wire, and over the surface of the conductor, as forming part of the wire. As the machine continues in motion, the tension of this accumulated fluid increases, by the addition of fresh layers beneath the former ; so that while electricity is in *motion* beneath the surface, other electricity is at *rest* on the surface. And this latter as might be expected exerts an attractive and repulsive influence over light bodies, the other being as before inactive ; but this is digression.

19. The comparative experiments bearing more immediately on the very point in debate are these :—

Let the negative wire of a powerful voltaic arrangement be in communication with the outer coating of a Leyden jar, while the positive wire is brought suddenly to the knob ; the jar will receive a charge, sufficient to affect an electroscope, the gold leaves of which will *separate,* indicating *repulsion.*

20. Let an apparatus of two parallel and moveable wires, delicately poised, be so placed in connexion with the same battery, as that, when the circuit is complete, the current shall pass along both. If while traversing wire A, in one direction, it traverse B in the opposite, the wires will exhibit signs of *repulsion ;* but if the current pass along both in the same direction they will be found to approach, evincing signs of *attraction.* To this circumstance then would I chiefly allude : that the same voltaic electricity which when accumulated in a jar, and thence distributed into the electroscope, caused the gold leaves to diverge, is found (while passing along fit apparatus in the same direction) to cause the wires to converge; being in the former case *at rest ;* in the latter *in motion.*

21. And it is precisely under similar circumstances that the experiment in question was made :

Two streams of electricity were passed in the same direction from points A and B, to a common destination, C.

22. In obedience to laws observed in other cases, it might be expected that they *would* (as they do) evince homogeneous *attraction,* (the only difference being in the nature of the

conductor, in the one instance wire in the other vacuum). But it is by no means legitimate to conclude from this, that homogeneous attraction belongs to electricity *per se ;* but rather that it belongs to electricity in *motion*.

23. Lest it be objected that the experiment in question was made with *frictional* electricity, and that *voltaic* is selected to illustrate it, I would observe that the selection is merely made because experiments with the latter, when in motion, are more generally known. But, to show that both are similar in their laws and effects, I will again avail myself of an authority to which all will pay respect. (Dr. Faraday, Third Series, § 360.) The general *conclusion* which must, I think, be drawn from this collection of facts is, that *electricity, whatever may be its source, is identical in its nature.* The phenomena in the five kinds or species quoted,* differ not in their character, but only in degree."

24. There is then inserted " a table of the experimental effects common to the electricities derived from different sources ;" by referring to which, it will be noticed that all the effects of common electricity are produced by voltaic, and vice versâ.

	Phisiolo-gical effects.	Magne-tic de-flection.	Magnets made.	Spark.	Heating power.
Voltaic electricity.	✗	✗	✗	✗	✗
Common electricity.	✗	✗	✗	✗	✗

25. In addition to this, and as if to supply every evidence of identity, the very experiment *in question* has been performed; wires have been arranged; electricity from the machine has been passed along them; dissimilar currents have repelled ; and similar have attracted.

26. From these observations, I think it will appear that the experiment in question is not of a kind to be admitted as evidence in support of the author's hypothesis : and, therefore, so far as the theory of homogeneous attraction depends on this, so far is it void of foundation.

27. In addition to the above, seventy or eighty experiments were made, (with the same object in view) by passing charges of electricity over the surface of cards capable of receiving a permanent mark thereby ; but as these are all liable to the

* Viz., 1, voltaic ; 2, common; 3, magnets; 4, therm.; 5, animal.

same objection, in that the electricity under examination was *in motion*, it will not be necessary to allude to them further.

28. In reference to the experiments in question, the author says: " If the above observations and experiments are not considered sufficient to establish the existence of the power of homogeneous attraction in electricity, I have other experiments devised, varying the above methods."

29. Should these other have been made with the fluid *at rest*, the results may be admissible : and, should they bear with them sufficient evidence, although wedded to the opinion of homogeneous *repulsion*, I will willingly resign it.

30. The principal design of the present communication having been to advert to the fact, that electricity *in motion* and electricity *of tension* are different in their effects, I have not thought it needful to investigate the *cause*; for whatever the *cause* be, the result is certain, is constant.

31. In conclusion I would observe that as these observations have been made in support of a theoretical notion, some may urge, that so long as we are acquainted with effects and the modes of producing them it is of little consequence whether they depend on this or on that theory ; whether, for instance, the phenomena of electricity depend on two fluids or on one ; or whether they originate in a series of expansions and contractions. For my own part I must candidly confess that I would at once close all my electrical books, lock up my apparatus, and withdraw my name from this Society, unless I hoped to derive something more than mere outward signs ; something more than mere visible effects and the modes of producing them, from devoting my time and thoughts to the study of this science.

32. The intellectual electrician is not satisfied with the shock, the spark, the heat, the decomposition, the deflection, the magnet, the mere *signa* operandi ; he would investigate further, and endeavour to trace the *modus* operandi in each instance ; he would penetrate behind the veil, and seek those mysterious laws which regulate the whole. By the induction of particulars, he would (as a good statesman) frame such a general code, as should suit every individual case : would reduce this code into so small a compass, that the whole statute book could be registered in the mind : and would render it so simple, that its application to every case might be readily perceived and as readily effected.

Kennington Grammar School,
    August 31st, 1838.

LX.   *On a new compound of Carbon and Hydrogen.*
By WILLIAM MAUGHAM, ESQ. *Lecturer on Chemistry, at*
the Royal Gallery of Practical Science, Adelaide Street,
London, &c. &c.   Communicated in a letter, dated June
21st, 1838, *addressed to the Honorary Treasurer of the*
London Electrical Society.

Read 3d July, 1838.*

Sir,

During the year, 1834, in consequence of certain experi-
ments that were made with different kinds of artificial lights,
at the Royal Gallery of Practical Science, by Messrs. Watkins,
Gardner, Wilkinson, and myself, I was induced to pay con-
siderable attention to the light produced by voltaic electricity,
through the medium of charcoal points, for the purpose of
rendering that light continuous and of the same intensity for
any given period.   I found that by increasing the number of
the pairs of points, a corresponding number of lights might be
accordingly produced of *apparently* the same intensity; the
several pairs of points being placed within the voltaic circuit
and insulated upon glass rods fixed in a piece of board.   On
bringing the points, thus arranged, in contact, and then
separating each pair at the same instant (the apparatus being
so contrived as to admit of this being done), sparks were pro-
duced simultaneously at the several interruptions.   This
experiment it is to be understood was suggested by my friend
Mr. Williams, who very ingeniously contrived the necessary
insulating apparatus alluded to.   Whilst repeating this ex-
periment with M. de la Rive, it was proposed by him that one
of the pairs of points should be placed in water, when the
spark, as might have been expected, was visible in that liquid,
and at the same time a spark was visible at each of the other
pairs of points out of the water.   The experiment was con-
tinued for about a quarter of an hour, and as it was proceeding,
a very peculiar odour, arising from the water in which the
electrical light was produced, became perceptible.

On reflecting upon this afterwards, it struck me that some
change must have taken place between the water and the
charcoal during the passage of the electricity.   To satisfy
myself of the truth of this, I prepared a battery of about sixty
pairs of four-inch plates arranged on Wollaston's principle;
to the ends of the two electrodes I affixed two pieces of char-

* From the Transactions of the London Electrical Society.

coal by means of platinum wires of sufficient length to allow the charcoal to be conveyed into a glass vessel containing distilled water. A glass tube was next filled with distilled water and then inverted in the water into which the charcoal points were to be immersed. On bringing these points together under the water so as to produce the electrical light with as little interruption as possible, the water underwent decomposition and the glass tube was soon filled with gas. When flame was applied to the mouth of the tube no explosion took place as when a mixture of oxygen and hydrogen gases is obtained by decomposing water when the action is electrolytic; but the gas burned silently with a flame similar in colour to that by which the flame of carbonic oxide is distinguished. Suspecting, therefore, carbonic oxide to have been produced, I passed 2 volumes of the gas obtained as above into an eudiometer with 1 volume of pure oxygen; the mixture was made over mercury, and lime water was then introduced above the mercury in the eudiometer. No change was observed in the lime water, but after passing an electric spark through the gases a gradual condensation of the whole took place and the lime water was rendered turbid, carbonic acid gas having evidently, in the first instance, been produced. If the spark be passed before the lime water is added the same condensation takes place as when we explode a mixture of carbonic oxide and oxygen, and lime water being then introduced it becomes turbid with a gradual absorption of the whole of the gas. A *very little* residual gas was sometimes observed, which disappeared after the introduction of a crystal of proto-sulphate of iron, showing the residual gas to have been oxygen added in excess.

It is to be remarked that on passing lime water to the gas obtained as above described, I have observed that it is sometimes very slightly changed, *but this certainly does not always take place.* It would, therefore, appear that a trifling quantity of carbonic acid gas is occasionally formed along with the carbonic oxide. Neither hydrogen nor oxygen can be detected. Therefore should these gases be found by any one else along with the carbonic oxide, it will be owing to the action becoming electrolytic in consequence of the spark not being continuous during the operation, but when the spark is produced the action is not electrolytic, and it is only then when the carbonic oxide is formed.

When water, therefore, is acted upon as already described, carbonic oxide is produced by carbon combining with its oxygen, and at the same time a peculiar compound is formed by another portion of the carbon uniting with the hydrogen of

the water, no hydrogen as has already been shown passing off in the gaseous state.

This compound, which does not appear to have been before observed, imparts a very peculiar and unpleasant odour to the water in which the charcoal points have been immersed whilst under the influence of the battery, but the process should be carried on for about a quarter of an hour to be satisfactory, during which period the spark from the charcoal points should be continually visible under the water. The water thus impregnated with the new compound is of an oily nature, which may be seen by pouring it into a clean tube, then emptying the tube and holding it up to the light. When kept for some time the liquid loses its odour and there is a precipitate of carbon. This spontaneous change takes place whether it be exposed to the air or kept in a stoppered phial.

I formerly thought that the substance in question was a compound of 1 equivalent of hydrogen + 1 equivalent of carbon and proposed calling it *protohydruret* or *protohydroguret* of carbon, but we have no proof of its being so constituted : for although 1 equivalent of the oxygen of the water evidently combines with 1 equivalent of carbon to form 1 equivalent of carbonic oxide, it is not evident that the 1 equivalent of the hydrogen of the water also combines with 1 equivalent of carbon: it may combine with 2, 3, or more equivalents.

The production of light under water by means of charcoal connected with a voltaic arrangement is a well known experiment; but I have not met with any one who is aware that the above-described change takes place, and therefore I venture to lay these observations before the public through the medium of the London Electrical Society.

When the experiment is made merely to obtain the compound under consideration in what may be considered a *concentrated* state, the less distilled water that is employed the better; about two or three ounces will be sufficient, it being merely requisite to keep the charcoal points covered with it, but more water will be required when it is intended to collect the carbonic oxide.

I have no doubt that some interesting results may be obtained by acting upon other fluids in a manner similar to the above with charcoal; and certain other substances might also be employed to effect the end I have in view; namely, the formation of compounds by producing the electric spark in water and other fluids, by causing the electrodes of a battery to be armed with different materials of a certain kind.

When steam is decomposed by passing it over red-hot charcoal for the purpose of obtaining carbonic oxide, I do not find

in the water over which the gas is collected anything like the compound in question. It may not be irrelevant to observe that Mr. Charles Cowper suggested to me that he thought carbonic oxide might be substituted for hydrogen as a combustible with oxygen for obtaining intense heat. A considerable quantity was accordingly burnt with oxygen by means of my blowpipe, and when the jet of inflamed gas was thrown on a piece of lime, the light produced was very little inferior to that which is obtained when hydrogen and oxygen are employed for the same purpose, and probably the light would have been improved had the carbonic oxide been passed through lime to free it from the carbonic acid which it always contains.

May not the hydro-carbon which is the subject of this paper be identical with the oily compound noticed by Berzelius as being produced when hydrogen gas is obtained from iron and dilute sulphuric acid? (See *Lehrbuch,* 186.)

I remain, Sir,
Your's truly,
WILLIAM MAUGHAM.

Royal Gallery of Practical Science,
Adelaide Street, West Strand,
June 21, 1838.

Note. At the ordinary meeting of the Society, July 17, the author of the preceding paper went through the experiments therein detailed, and the several results were highly satisfactory to the members present, being perfectly corroborative of what is stated respecting the formation of carbonic oxide and a previously unnoticed *hydro-carbon* when water is decomposed in the manner described.

LXI. *An account of a series of daily observations, made by* ANDREW CROSSE, ESQ., *of Broomfield, near Taunton, with a Sustaining Voltaic Battery, to ascertain the increase or diminution of the power of the same, as corresponding with the increase or diminution of the temperature of the atmosphere, during a part of the last winter, and commenced previously to the very severe frost which afterwards took place. Also a few remarks on the agency of heat in electro-crystallization.*

Read June 19th, 1838.*

For upwards of two years past I have found it convenient in the formation of crystalline and other matters, by the elec-

* From the Transactions of the London Electrical Society.

tric agency, to make use of porous earthen pots, of the same
nature as garden pots; but without an aperture in the bottom.
One of these being filled with a compound fluid, A and B,
and being plunged in a basin filled with another compound
fluid, C and D ; A of the one fluid having a greater chemical
affinity, (as it is called) to C of the other fluid, than it has to
B, with which it is united, and also B to D, so that by the
admixture of the two fluids, double decomposition would take
place ; an electric current being passed by means of primary
conductors proceeding from the poles of a voltaic battery in
constant action, from one fluid to the other, through the pores
of the pot employed, a slow union of A and C, or B and D,
or both, takes place, either at the positive or negative pole, or
on the inside or outside, or *within the substance* of the pot
itself; or in more than one, or in all of these, according to
the *nature* and *temperature* of the fluids employed, the *in-
tensity* or *quantity* of the electric current, the thickness of
the pots, and the presence or *absence of light,* which last is
in most cases of greater or less importance, and in some
absolutely essential.   The result of this union is common to
the production of regularly or irregularly formed crystalline
matters more or less firmly adhering to the substance upon
which or within which they are formed.   I have used these
pots in hundreds of experiments, in an infinite variety of
applications, and with considerable success.   I have likewise
used them more or less extensively in the place of bladder in
sustaining voltaic batteries, for which purpose they are
admirably suited.   They have, however, one•defect.   If,
while sulphate of copper is used for the negative cells, a neutral
salt be employed for the positive, in the course of time crystal-
lizations are formed within the substances of the earthenware
which separates the two fluids, and the pots are cracked in
all sorts of forms—sometimes longitudinally; sometimes
laterally ; sometimes in concentric layers, the outer or inner
portions scaling off like the bark of a tree ; and sometimes in
small angular or circular fragments which start off with a
slight explosion, so that after some months' action the earthen
vessel is spotted over with deep indentions either external or
internal, or both.   It is therefore safer and better on all
accounts to avoid the use of neutral salts in the positive cells,
which I commonly fill with simple water, when I wish to keep
up a uniform action for a considerable time, and when I em-
ploy these pots merely in the place of bladder.

The following observations were made in a room exposed
to the light, with a southern aspect, and situated about 800
feet above the level of the sea.—

Dec. 22, 1837. 10 P.M. I set in action a small sustaining battery composed of twelve two-inch square arcs of zinc and copper (the zinc not amalgamated), in small porous pots and glass basins; each zinc plate resting on a small piece of zinc, placed in a glass basin filled with common water; and each copper plate resting on a larger piece of copper placed within a pot which stood in the middle of the next glass basin, and which contained three ounces of sulphate of copper and water. It is a simple and economical mode of increasing the surface of the metals employed, to cause the pairs of plates to rest respectively on larger masses of the same metals, by which means small plates may be made to act in some degree with the power of large ones, and which partially saves the expense and trouble of casting. A Faraday's voltameter being filled with common pond or river water, was connected with the poles of the battery, and emptied of its gas each night at ten o'clock, and replaced in its former situation. A thermometer was likewise suspended above the battery, so that its bulb was immersed in one of the glass basins of water. This thermometer was examined at different intervals during the day and night, and the degrees of temperature carefully noted. It is obvious that by this arrangement only a portion of the electric fluid excited could possibly pass through the voltameter, as no acid was added to the water with which it was filled. For this I had reasons which I shall not here dwell upon.

The observations were continued for the space of one month or twenty-eight days, during which time neither water nor sulphate of copper was added to the cells of the battery, in consequence of which a good deal of the fluid had evaporated at the month's end. When the voltameter was first applied, a very small stream of the combined gases was extricated, but in the course of some hours it increased, and at the end of twenty-four hours 42-20ths or degrees of gas were evolved. It will be seen by inspecting the journal attached, that the battery did not arrive at its maximum of power till the third day from the commencement of its action. This was occasioned by the resistance of the earthen pot to the electric current, a thicker or a thinner pot affording a greater or a less resistance. It will likewise be seen that in general there was a more or less regular decrease of power, which seemed to be at the rate, as near as one may judge, of from one to two degrees of gas in twenty-four hours, supposing the temperature to remain the same; but that in general the power increased or diminished with the increase or diminution of temperature. Thus in the first week, as long as the thermometer stood at about 50, the diminution of gas was from one to two degrees

in each day : but on the last day of the year when the thermometer had sunk from 50.5 to 47, the quantity of gas obtained was lessened from 61 to 57.5 degrees: also between the 6th and 7th of January, with a diminution of temperature of from 42.5 of the thermometer to 39, there was a diminution of gas of from 51 to 45 degrees. This is what one might more or less have expected; but I know not how an increase, and a somewhat considerable one, of the power of the battery could take place under a *diminution* of temperature. Thus, on January 13th, with the thermometer at 32, forty-five degrees of gas were produced; when on the preceding day, with the thermometer at 36, only 42 degrees of gas were liberated. Again, on the 17th of January 47 degrees of gas were produced, with the thermometer at 34, when on the preceding day there were only 41 degrees of gas with the thermometer at 33. The one degree's increase of heat bears no proportion to the six degrees' increase of gas. Again, on the last day of the journal, with the thermometer at 32, there were 4 degrees of gas less than the preceding day with the thermometer at 31. This requires sifting and close examination. It may be observed that the degrees of gas produced on the last day, with the the thermometer at 32, and with ice in all the cells, were exactly the same as on the first day with the thermometer at 50. It may also be noted that the total quantity of gas obtained in the fourth week was only 4.5 degrees less than that which was obtained in the third week, notwithstanding the natural diminution of power in the battery, the increased loss by evaporation of fluids, and the five degrees' diminution of temperatures. I was prevented from prolonging these observations by the freezing of the water in all the cells. I may here observe that I had, previously to these experiments, as I have since tried the effects of heat in combination with voltaic electricity in the formation of crystals, that I have exposed various solutions under different conditions to the electric action, such solutions having been kept as nearly as possible at the boiling point from one to six weeks, the apparatus being plunged in sand baths, with fires kept up day and night without a moment's intermission, and the solutions being constantly replaced as they evaporated. In sixteen of these experiments which were carried on at the same time, the evaporation was so great that it exceeded seven gallons in every twenty-four hours. I am not prepared at present to give a succinct account of the different results of these operations, but shall state generally the following conclusions.

1. A piece of yellow sulphuret of copper was exposed to the electric action, in sulphate of copper at the negative pole,

in the cold solution, and found after a given time to gain a certain weight, the same being my friend Mr. Fox's experiment.

2. A similar piece of the same was exposed, exactly under the same circumstances, *in the hot solution* to the same electric power, and found to gain *thirty-one times* the weight of the preceding, within the same time. Such additional weight in both cases mostly consisted of metallic crystallized copper and red oxide of copper on the surface of each.

3. Although the solutions, in which the latter formation took place, were kept as constantly as possible at the boiling temperature, the crystals were generally of the most regular form, with their angles and facets quite as perfect as those of a natural formation.

4. In the production of crystallized copper and red oxide of copper, I found that with a single pair of plates plunged in boiling solutions the increase of crystallized matter averaged 60 *grains in each day*, or one ounce troy in every eight days; and that, consequently, even tons weight of crystallized copper and red oxide of copper may be formed in a comparatively very short space of time by an increase of electric power, and the quantity of solutions employed. The sizes of my plates were various, generally about two inches square.

5. By covering large plates of zinc, with plaster of Paris, unconnected with any copper plates, and laying them horizontally in large vessels filled with sulphate of copper, kept boiling, and well supplied with a fresh solution as the evaporation went on ; the most perfect octohedral crystals of metallic copper and red oxide of copper, were formed to the amount of some ounces in weight in less than ten days. These crystals were fully equal in all respects to those formed by nature.

6. The same effects took place in the cold, but in an infinitely inferior degree. In this way I have formed crystallized copper, silver, and lead, upon a zinc ingot.

7. In breaking the thick earthen pans, in which some of these formations have taken place, crystals of various sorts are found within their substance. Also veins of metallic copper crossing them in various directions, very similar to what are termed the leaders to a metallic lode. Under some circumstances, perfectly insulated crystals of various sorts, not in connexion with either pole or with any metallic substance, are formed in abundance. This I have frequently observed, but in much less quantity, to have taken place in the cold, during the last two years. The above is a correct but rude sketch of some of the general results which I have met with in the combination of heat and electricity.

Query. May it not be possible to apply the combined action of a boiling heat and continued electricity, to the extraction of metals from their ores in a pure state, and with less trouble and expense than the plans now adopted ?

<div align="right">ANDREW CROSSE.</div>

London,
June 18*th*, 1838.

Extract from the Journal referred to in the preceding paper

Day of the week.	Date.	Of gas Degrees.	Therm	Day of the week	Date.	Of gas. Degrees	
	1837.				1838.		
Saturday	Dec.23	42	50	Friday	Jan. 5	52	44
Sunday	24	64	50	Saturday	6	51	42½
Monday	25	69	50	Sunday	7	45	39
Tuesday	26	67½	50	Monday	8	43	37
Wednesday	27	65⅓	50	Tuesday	9	41	31
Thursday	28	64	50	Wednesday	10	44½	35
Friday	29	62	51	Thursday	11	44	34
Saturday	30	61	50½	Friday	12	42	36
Sunday	31	57½	47	Saturday	13	45	32
				Sunday	14	42	30½
	1838.			Monday	15	43	33
Monday	Jan. 1	56	47½	Tuesday	16	41	33
Tuesday	2	55½	46	Wednesday	17	47	34
Wednesday	3	54	44	Thursday	18	46	31
Thursday	4	52	42	Friday	19	42	32

<div align="center">

Gas obtained.   *Average temperature.*

</div>

1st week . . . . 434°	1st week	a little above 50°
2d do. . . . 388°	2d do.	not quite 46°
3d do. . . . 310½°	3d do.	not quite 37°
4th do. . . . 306°	4th do.	a little above 32°

LXII. *Researches in Magnetic Electricity and new Magnetic Electrical Instruments.* *By* CHARLES G. PAGE, M. D.*

1. *Compound Electro-Magnets for the magnetic electrical spark, shock and decomposition.* In the last number of this Journal, I announced this new form of magnet, as decidedly superior to the common solid electro-magnet, for exhibiting magnetic electrical phenomena. I have since performed a variety of experiments, with reference to the best mode of constructing these magnets, and arrived at some beautiful results and important conclusions. I find that the

* From Silliman's American Journal.

magnet made of flat plates of thin hoop iron, answers best
for lifting power and sparks; but those made of fine annealed
iron wire, answer nearly as well, and, (for reasons, by and
by to be mentioned) will in some cases be found preferable to
the flat bars. Sixteen magnets were made of soft iron wire,
in bundles of various lengths and diameters. Thirteen were
made of No. 26 wire, two of No. 16, and one of No. 8. For
lengths within ten inches and diameters within one inch, the
very fine wires answer best. But for longer and larger mag-
nets than these, the larger iron wires answer equally well.
Each of these magnets were tried with one, two, and three
coils of No. 16, copper wire (wound each the whole length of
the bars and superposed by a single pair of lead* and zinc
plates, eight inches single surface of zinc), kept constantly
in good action by the sulphate of copper. One of these mag-
nets weighing two ounces, with four coils of fine wire exterior
to three of large, gave a shock which could not be endured.
The two best magnets of the sixteen, were, one made of one
hundred fine wires six inches long, and one of five hundred
wires, ten inches long. This last furnished the most splendid
deflagrations, and by far the strongest shocks. Its decom-
posing power was very great, for the means used. One was
prepared of one thousand wires, and a foot long, but its power
was inferior to that of five hundred wires. This large com-
pound bar was ultimately sawed in halves, and each half in-
dicated as much power as the whole. This fact seems to prove
that the length of the coils of copper wire was too great for
the size. * I apprehend that a copper wire of a larger size†
would have made it superior to the other magnets. The ad-
vantage before alluded to, possessed by the magnets prepared
from bundles of fine wire, is, the facility of making a curved
or U magnet‡ wound accurately throughout its length. There
is little or no advantage in winding common magnets on the

* If properly prepared, lead for the negative plate, is much
superior to copper, where the sulphate of copper is used.

† I have universally found that large wire answers best for large
magnets, and small wire for small magnets. The larger the wire,
the more freely it conducts, but large wire cannot be used with
advantage on small magnets, as the coils or turns will not be suffi-
cient in number, and the axis of the wire will lie more oblique to
the axis of the magnet, than that of a smaller wire, or than upon
a larger magnet.

‡ The term, horse shoe, applied to magnets is inappropriate,
and has led many into the error of constructing magnets of this
awkward and disadvantageous form. The letter U would briefly
designate this species of magnet.

curved portion, owing to the difficulty of laying the wire on that part, at right angles to the axis of the bar. A very perfect U magnet covered accurately throughout its length, is readily made, by winding a straight bundle of fine iron wire, with as many coils of copper wire as desired, and bending them afterward. They are bent with ease, and the wire disposes itself on the bent portion in a beautiful and regular manner. All the magnets that were made of this description, were tried before and after they were bent, and no change in their properties could be observed. The best of the magnets, above alluded to, was covered with two hundred feet of fine wire, exterior to four coils of large wire. Combining the secondary currents of the large and small wire, the shock was so great as to render it difficult to keep even the tips of the fingers upon the wires. A single thermo-electric pair, heated and cooled, connected with the large wire, gave a bright spark and a shock, which could be felt as far as the wrist. The use of three pairs in sequence, enhanced the results. It must here be observed, that the shocks and sparks are not thermo-electric, but magneto-electric. The pure thermo-electric spark, I apprehend, has never yet been seen. I obtained, a long time since, sparks and shocks from a thermo-electric pair, in connexion with Henry's flat spiral. But in that case also, the results were purely secondary, or magneto-electric, the copper spiral while transmitting a current, in fact, representing a magnet with axial poles. I have always maintained the position, that a shock direct cannot be obtained from a single pair of plates, or any elementary current, under any arrangement whatever. Although recently, we have the high authority of Faraday, that iodide of potassium, and some other compounds may be decomposed, by a single galvanic pair, yet even admitting this to be fully established, I see no reason for retracting, and may continue safely to assert, that an elementary battery however large, cannot afford a direct appreciable shock. In Faraday's experiment, the decomposition was the result of uninterrupted action, but in all experiments hitherto, where shocks have been obtained by the aid of a single pair, they have been obtained as single impulses, immediately consequent to the completion of the circuit, as with the large magnet of Prof. Callan, or as in all other cases, to the interruption of the circuit. In a new instrument, shortly to be described, I have a singular instance of an electro-magnet, affording shocks, not only on the completion and breaking of the circuit, but even while the battery current is passing without interruption. It appears irrational to suppose for a moment, that the shock obtained by breaking the circuit with

coiled conductors, can receive any augmentation from the conjunction of the primitive and secondary currents, for the presumption is, and the fact itself seems sufficiently obvious, that the sparks and shocks indicating a new and secondary current are directly consequences of the dissolution of the primitive current. In other words, the effects produced by the breaking of a primitive elementary current, are due solely to magnetic excitation, and have no connexion with that primitive, except that of cause and effect. In fact, strictly speaking, the results thus observed are not secondary, but tertiary phenomena; the secondary production being the development or neutralization of magnetic forces. And, as Mr. Sturgeon has very ably set forth in his beautiful theory of electro-magnetic lines, in the present state of our knowledge, it is indispensable to the explication of the reciprocal action of magnetism and electricity, to *suppose* the existence of a secondary intervening medium, whether the coiled conductors act with or without the co-operation of ferruginous bodies. One singular fact which I noticed nearly two years since, remains to be reconciled with the postulates, that a direct shock cannot be appreciated from an elementary current, and that secondaries are strictly consequences remote from primitives. When two, three, or four pairs of plates, or any number below twelve, arranged as a compound series, are connected with coiled conductors, with or without soft iron enclosed, the sparks and deflagrations, are far more brilliant on breaking the circuit, than with the same plates used as a simple or elementary battery. This fact is readily accounted for. The battery current itself is capable of passing through perceptible space, with appreciable duration and as the secondary current (which is the natural electricity of the wire, set in motion by magnetic forces), returns to its equilibrium through the medium of the battery plates and its liquid, the two currents must here move in conjunction, and enhance the combustion of the metals used. I have strictly examined the power of the pure secondary developed in this way, and find it never to exceed the secondary from the same pairs arranged as an elementary series. In fact, the secondary begins to diminish, just when the magnetizing power of the compound battery begins to diminish, which is well known to occur, as the series extends. The properties of small and large compound batteries, have moreover been examined while the currents have been passing through circuits from twenty feet to half a mile, without any observable characteristic changes. But to return to the action of the compound electro-magnet. The superior value of these magnets,

whether made of flat plates or fine wire, may be traced to several causes. First, the homogeneous texture of the iron from which wires or flat plates are made, is favourable to the extensive development of magnetism. Secondly, the same quality favours its neutralization, when the exciting cause is withdrawn. Thirdly, the sum of the actions of so many highly charged magnets, and lastly, the mutual neutralizing influence of the individual magnets of a similar polar arrangement. This last is by far the most important consideration. The following experiments, throwing light on this point, will be regarded as novel, and have an important bearing on the subject of magnetism itself.

Take a piece of fine iron wire, carefully annealed, three or four inches in length, and touch it once with a common steel magnet. Being very soft, it yields readily to the inductive influence, and as its length is very great, compared with its breadth, if carefully handled, it will be found to retain sufficient power to hold more than its own weight. Hold it now by one end and snap the other with thumb and finger, and its magnetism will be instantly lost. The same quality of the iron which favoured the extensive development of magnetism, favoured also its neutralization on the disturbance of molecular forces. The transition here was so sudden and decided, that I was led to try the influence of a magnet thus operated upon, in developing electricity. A long piece of iron wire, retaining considerable magnetic power, was suspended so as to vibrate freely in a spiral of copper wire. A smart rap upon the suspended iron wire, determined a strong galvanic current through the copper spiral, as indicated by the galvanometer. The result, though readily anticipated, was nevertheless very striking. Again, take a number of pieces of fine iron wire, and magnetize them separately, so that each by itself will hold its own weight: combine them now in a bundle, and instead of the aggregate lifting power of the elements, you scarcely realize the power of one of those elements, owing to the neutralizing influence of the similar poles. It is the co-operating, neutralizing effort of similar poles, that chiefly determines the superior value of the compound electro-magnet for magnetic electrical experiments. The fact that very fine steel wires answer almost as well for these magnets as wires of soft iron, strongly corroborates this last position. For the knowledge of this curious fact I am indebted to Mr. Daniel Davies, philosophical instrument maker, of Boston. A bundle of fine steel wires when wound, give a bright spark and strong shock, whereas the same amount of steel in a solid bar, produces a hardly perceptible augmentation.

2. *New Magnetic Electrical Machine, or Magneto-Electric Multiplier, convertible into an Electro-Magnetic Engine.* In order that this machine may be readily comprehended, it will be necessary to make a few brief preliminary observations, and to advert to the discoveries which led to its invention. When a piece of soft iron enclosed in a helix of wire, is rendered a magnet, by the approach of a magnetized bar, a current of electricity flows through the helix (during the *development* of magnetism) in a direction contrary to that of a galvanic current, which would render the piece of soft iron a magnet of the same character. Withdraw the inducing magnet, and during its withdrawal, or the neutralizing process in the piece of soft iron, the direction of the electric current in the helix will be the *same* as that of a galvanic current which would render the soft iron a magnet of similar polar arrangement. Similar momentary currents are excited when the exciting cause is a galvanic current. *Completing* the galvanic circuit with the helix, determines a current flowing against the battery current. *Breaking the circuit*, determines a current flowing in the same direction with the battery current, which produces the bright spark. Any disturbance of magnetic forces is accompanied with a disturbance of electric equilibrium. It is well known, that an electro-magnet possessing an immense lifting power when its poles are joined by an armature, exerts a comparatively feeble action at a distance. The reason is this; the two magnetic forces or poles are tending constantly to neutralize or disguise each other, the softness of the iron favours this mutual action, and the whole amount of magnetism developed cannot be perceived, until the magnetic forces are insulated or determined towards the poles by the application of an armature. The application then of the armature must occasion a considerable disturbance or movement of magnetic forces, and give rise to an electrical current in the wires. The direction of this current can easily be predicted, from what has been before said. The application of the armature is equivalent to the further development or separation of magnetic forces : hence the new current will flow against the battery current. When the armature is pulled off, the new current is in the same direction as that of the battery. And if the magnet wires be so arranged that the galvanic circuit may be broken at the instant of pulling off the armature, it will be found that the magneto-electric spark and shock will be far greater, than can be obtained by simply breaking the circuit, without the aid of this operation. Thus we have a new method of augmenting to a great degree the magneto-electric currents

from an electro-magnet. The application of the armature
contributes to the development of magnetism, and on the
simultaneous withdrawal of the armature, and the galvanic
current, the whole amount is suddenly neutralized.* The
current developed by the *application* of the armature. I have
appreciated by the following experiments, and I find its con-
sideration to be of very great importance in the application of
electro-magnetism as a moving power. This subject will be
considered at some future time, and at present I shall be con-
tent with giving briefly the reasons why we cannot increase
the power with economy, where a great number of magnets
are charged by the same battery. 1. The motion of attract-
ing poles towards each other, actually diminishes the power
of each magnet, by determining a current against the current
of the battery. 2. The succession of similar or repelling
poles, determines a current also against that of the battery.
3. The withdrawal of attracting poles (which must be effected
by mechanical power, and is of course directly against an
independent movement) maintains the power of the magnets,
as the new current is then in favour of the battery current.

Exp.—To test the value of these secondary currents, I
included a galvanoscope in the circuit with an electro-magnet,
and placed the needle beyond the direct influence of the mag-
net. On bringing up the armature, the needle returned four
degrees. and on suddenly applying it, the needle swung nearly
back, but immediately returned to nearly its original deflec-
tion of 40°. Suddenly pulling off the armature, the needle
swung over 90°. The following experiment is •still more
striking. Connect two electro-magnets, so that they shall
be charged in sequence, but at the same time, and with the
same battery. Load one of them with about as much as it
will hold. Apply the armature to the other magnet, and the
weight of the first magnet will immediately drop. The same
may be repeated with either magnet. The power of this re-
acting current, as it may be called, is in proportion to the
number of magnets in use. charged by the same battery, the
number of coils of wire covering the magnets, and lastly in
proportion to the rapidity of motion.

The operation of the magneto-electric multiplier will now
be understood at a glance. It is simply an electro-magnet,

* A steel magnet gains power slowly when its poles are armed
and loaded simply by determination, or insulation of its poles.
Pulling off the armature again weakens its power. Jerking it sud-
denly off, weakens it still further. In this case the magnetic forces
seem to acquire momentum, and go beyond their original statical
development.

with a revolving armature and break-piece. The revolution of the armature is of course equivalent in its effect, or nearly so, to its rectilinear approach and abduction. *a* fig. 2, plate XIII. is a small compound electro-magnet, 12 inches in length before bent, composed of 500 fine iron wires, covered with 5 superposed coils of large copper wire, No. 16, and 7 layers of fine copper wire, No. 26. *b, c*, are the terminations of the small wire, *d, l*, the terminations of the large wires. 1, is a wooden strap, which, with a binding screw, secures the magnet to the wooden block *k*. *s* is a brass strap, to hold firmly the poles of the magnet. *e* is the armature of soft iron, which, with the small brass pulley, is fitted firmly to the shaft. *o*, the multiplying wheel. *n*, is the break-piece, which is merely a copper ferule dissected, and filled up with wood or ivory, as represented by the shaded spaces. *l*, one of the magnet wires bent up, plays upon the whole portion of the ferule; and *m*, one of the battery wires, plays upon the break-piece. The other battery wire is to be connected with the mercury cup *d*. Adjust now the break-piece, *n*, or the copper wire, *m*, so that the circuit shall be broken at the time or immediately after the armature leaves the magnet, and turn the multiplying wheel *o*. The sparks and shocks thus produced, will be found far greater than when the circuit is broken while the armature is approaching. The break-piece is readily adjusted, so that the armature revolves of itself with great rapidity; but in this case also, the sparks and shocks* are at once diminished, as the source of magnetism must be cut off before the armature arrives at its equilibrium. With things thus arranged, connect the two ends of the small wire *b, c*. (This wire is insulated, and entirely independent of the large wire.) The revolution of the armature is now suddenly stopped. The flowing of the secondary current after the galvanic is broken, keeps up or prolongs the power of the magnet so as to retain the armature. As a proof of this, set the spring and break-piece so that the magnetic power shall be cut off, some time before the armature arrives at equilibrium. It revolves of course slowly. Connect now the ends of the small wire *b, c*, and it immediately revolves with rapidity, the prolongation of magnetism by the secondary, contributing now to the motion of the bar. Here then the secondary becomes a new source of magnetic power. This instrument, when the armature is revolved mechanically, affords a shock from *b, c*, both when the circuit is completed and broken. The sparks are exceedingly brilliant, and the shocks so powerful that

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* The shocks are taken from the cups *b, c*.

they are sometimes felt by the bystanders through the floor. The light from charcoal points is intense. It decomposes pure water, and charges the Leyden jar. But its most remarkable and novel property is that of giving a slight shock without breaking the circuit, and while the galvanic current is flowing without obstruction. To effect this, place the spring-wire *m*, with *l*, on the whole portion of the ferule and turn the wheel. A slight shock will be felt at *b,c*, being simply the consequence of magnetic disturbance by the motion of the armature. A single thermo-electric pair of bismuth and antimony plates heated and cooled, affords a shock which is felt as far as the wrist.

Washington, D.C.,
    June 2, 1838.

LXIII. *On an electro-magnetic Indicator. By* J. T. Quekett, Esq. *In a letter to the Editor of the Annals of Electricity.*

    Sir,
Knowing that considerable difficulties are experienced by lecturers and others in the explaining to the uninitiated the action of electric currents on the magnetic needle, I have contrived a little piece of apparatus, which I call my electro-magnetic indicator, for the purpose of illustrating the suggestions of Ampère, and which I think will not only assist in the comprehension of his views, but tend to fix more permanently in the mind this important principle. The construction of it is as follows:—

    Let A B represent a piece of wood about eight inches long and four wide, into which is morticed, at a convenient distance from one end, an upright, C D, five or six inches in height, and of the shape represented in fig. 3, plate XIII ; on the inner face of this upright are fastened two concentric circles of brass, E F, also seen in fig. 4, and in section in fig. 4. These circles are about a quarter of an inch wide, and the larger is three inches, and the smaller two inches in diameter. To the larger or outermost of these circles is fastened a wire, P, for communication with the positive pole of a battery, and to the inner another wire, N, for the negative pole of the same battery. W W' is a piece of brass wire bent as represented, and its ends kept apart by a piece of ivory, I I', through the middle of which is a hole for receiving a screw, M, fig. 3. The end, W, of the wire is made quite smooth and moves on the outer circle, E, whilst the end, W', moves on the inner

circle, F. These ends are kept in perfect contact with the circles, E F, by means of a spring, G G', which is interposed between the head of the screw, M, and the ivory, I I'. The screw also forms an axis on which the frame, W W', is made to turn ; the pressure to ensure perfect contact being made by the nut, L, as seen in fig. 3. From the head of the screw, M, we have a smaller wire proceeding, and bent three times at right angles as seen at H, its free end being pointed for the purpose of allowing the magnetic needle, N S, to move on ; which needle, for the convenience of persons at a distance, has on its respective poles the cardboard letters N S. Attached to the wire frame at W, is a little wooden figure with its face fronting the needle, and in its right hand is placed a little staff to which another letter N is attached. On the wires, P and N, are fastened arrows, also cut out in cardboard, to show at a distance through which wire, and in what direction, the current is passing; the letters N and P being also used to denote which electricity is employed.

Such is the description of the apparatus. Now for its application.

We will suppose that the wire, P, is in communication with the positive pole of a battery, and N with the negative pole ; the arrows then will show that a positive current is passing from the head of the figure towards its feet over the needle, N S ; we shall then find that the north pole of the needle, on which the letter N is placed, will move towards the right hand of the figure. But the wire frame, W W', has a motion round M as a centre, and can be placed in any position, with respect to the needle, N S, that we may please : the contact with the circles, E F, being always preserved by the spring, G G'. If now the frame work be turned so that the figure be exactly under the needle, the north pole will still move towards the right hand of the figure, and it will be observed that the right hand is now in an opposite direction to what it was at first. Again, if the wire be turned a quarter of a circle either backwards or forwards, so that the plane of the wires corresponds with the plane of the needle, the north pole, N, will then dip up or down according to whether the right hand of the figure point upwards or downwards ; in fact, as long as the current remains the same, the north pole of the needle will always move to the right hand of the figure, in whatever position the frame, W W', be put. If a negative current be passing through the wire, P, the reverse law obtains. All these conditions with one current may be shown without removing the wires, P N, from the battery ; but the motions

of the needle will be more striking if they be taken out previous to the shifting of the frame work, W W'.

Should you think the above worthy of a place in your Annals, the insertion of it will oblige,

Your obedient servant,
50, *Wellclose Square*.   J. T. QUEKETT.

P. S.   Since the above was written I have made an improvement in the circles of brass, E F.   Instead of having on the outer one a continuous metallic surface, I propose to have at the four quadrants little pieces of brass soldered on, and the space between these pieces of brass to be filled up with wood, so that when the wire touches the wood the current may be stopped, and, consequently, when the wire frame, W W', is being moved on the wood, the needle will return to its original condition, and only be again acted on when the current is allowed to pass by means of the piece of brass at the quadrant; this contrivance will supersede the necessity of removing the battery wires when we wish to show the action of all the currents in succession.   This mode of making contact will probably be of some service to those engaged in telegraphic and other applications of electricity.

LXIV.   *Preliminary observations on an experiment on the action of Perchloride of Platinum (Platinchlorid) on Acetone.   By* WILLIAM C. ZEISE, *of Copenhagen.*[*]

When a solution of perchloride of platinum in about two and a half parts of acetone is distilled to a syrupy consistence, and the distillation of the product is once or twice repeated, we obtain a mixture of a variety of new combinations in the several chlorides thus produced.   The liquor which last distils over is rich in muriatic acid, and contains at least *one* ætherial body.   The different compounds thus mixed together, are difficult to obtain in a separate and pure state: and although I have already devoted much time to accomplish this object, I am, nevertheless, not certain that I have obtained them all in a pure condition.   Of these compounds, there appears one which deserves a particular description. It is a yellow crystallizable body whose elementary composition is
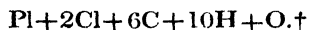
$$Pl + 2Cl + 6C + 10H + O.\dagger$$

[*] Poggendorff's Annalen der Physik und Chemie.   Band XLV Stück 2.   Translated by W.

[†] The original formula is $Pt + 2Cl + 6C + 10H + O$.   TRANS.

In order to obtain this compound, I agitate the brown acid, tar-like residue, with fresh portions of water, as often as that liquid assumes a brown yellowish colour, and then quickly filter the solution through a linen cloth, for the purpose of separating the undissolved resinous or pitch-like matter. The solution soon becomes turbid from the lower part upwards, and in the course of half an hour or an hour, a tolerable large quantity of small yellow crystals form and precipitate in the liquor. I now decant the mother-liquor and place it in a vacuum over sulphuric acid, either with lime, or hydrate of potassa, until it has evaporated to a brown crystalline mass. This crystalline substance I now treat with water in the same manner as i treated the tar-like substance in the first instance; by which means I obtain another portion of the new crystallizable compound: but this latter portion is generally of a, deep brown colour. In order to obtain it pure, I dissolve it in the acid liquor obtained from the first distillations of the perchloride with acetone, and distil this filtered solution down to a syrupy consistence, and treat this residue with repeated effusions of water in the manner already described. Finally, in order to remove every trace of brown colouring matter, I place the last obtained crystalline product between folds of blotting paper, and after its being well pressed and dried I dissolve it in acetone, filter the saturated hot solution into a wide-mouthed glass vessel; and when cooled, decant the liquid part from the deposited crystals. This liquid I cautiously distil down to that state which will ensure the greatest part of it to crystallize. The crystals, thus obtained, are washed in a small portion of acetone and then dried. I have also obtained a considerable quantity of this compound by placing a given quantity of acetone mixed with perchloride of platinum, in a well closed glass vessel: agitating the mixture, and afterwards permitting it to stand unmolested for twenty-four hours. I name this new compound *Metacechlorplatin.*

Metacechlorplatin is of a sulphur yellow colour; the crystals are small and difficult to describe with accuracy: it is almost devoid of all smell. When dried at the common temperature of the air, it loses nothing in weight by being placed in a vacuum over sulphuric acid; not even when the temperature is raised to above 100°. It inflames with facility, burns with a partially green flame and leaves a residue of platinum of a silvery white colour. Heated in a retort it blackens, and without swelling up, yields an abundance of odourous vapour, peculiar at the commencement, but which afterwards has a strong smell of muriatic acid: a part, at least, of which vapour is easily condensed to an oily substance.

The carbonaceous residue burns slowly in the open air, something like tinder, and leaves silvery white platinum behind. Scarcely any of it is dissolved by water at common temperatures, but when heat is applied, a portion becomes dissolved, and forms a yellow solution; which, however, holds but very little of the salt. By boiling, this solution yields a brown flocculent substance, whilst the undissolved portion becomes transformed into a brown slimy mass, without any observable appearance of metallic platinum. Æther does not appear to dissolve this body · and alcohol, at common temperatures, has but a feeble action on it, though, by heat, some little becomes dissolved in the latter medium, which assumes a yellow colour, and which. whilst cooling, deposits a yellow crystalline powder. Muriatic acid, even when concentrated, operates upon it only at a high temperature: the acid solution endures a boiling heat without any observable change. By a solution of potassa metacecchlorplatin is easily dissolved, forming with it a brown liquid. A solution of chloride of potassium, or chloride of sodium, also dissolves it by heating : and the yellow coloured solution denotes no change by submitting it to a boiling temperature.

The determination of the carbon and hydroygen was accomplished by burning a portion of the new compound, with oxide of copper, and another portion with chromate of lead.

The composition of metacecchlorplatin

$$(Cl.Cl^2 + C^6H^{10}O)$$

when compared with that of acetone $(C^6H^{12}O^2)$, indicates that (H^2O) has become displaced by the introduction of an atom of the chloride of platinum. There appears, however, to be several other chloro-combinations formed simultaneously with this one; and, therefore, the chemical interchanges may, perhaps, be of a somewhat more complicated nature. At all events, it is probable that there is formed a compound consisting of two atoms of chlorine and two atoms of hydrogen, in the same manner as by the action of perchloride of platinum with alcohol; and there is also formed, by the reaction of one atom of oxygen, one of the compounds of a corresponding character with aldehyde (Consult my essay on the inflammable chloride of platinum in this Annals,[*] Vol. XL. p. 251). This compound assimilates itself also with those products of acetone with sulphuric acid, muriatic acid, &c., described by Kane (See Transactions of the Royal Irish Academy). It also appears worthy of observation, that whilst by the action of perchloride of platinum on alcohol two atoms of chloride

combine with one atom of ætherin ($C^4.H^5$); but by allowing only one atom of chloride, still H^2O (and, perhaps, as the representative of the second atom of chloride), with C^6H^8 forms a distinct combination.

When the brown crystalline mass, obtained in vacuo, has yielded its last portion of metacechlorplatin, there remains a sour brown liquid behind. If this be heated in a retort, it becomes troubled and exhibits a tolerably brisk effervescence, yielding, at the same time, an oily-looking fluid which passes over; and in less than the course of an hour there forms in the, now almost colourless, fluid a great quantity of small flakes of a coal-black matter. Of this substance I shall make no farther remark in this place, than that, when gently heated, it inflames with explosion. For the present I name it *pyracechlorplatin*.

When the original syrupy product, of the distilled solution of the chloride, has yielded the whole of its soluble portion to the water, there remains a considerable quantity of a dark-brown pitchy matter; which for convenience I call *platinharz*. At common temperatures this substance is as hard and brittle as rosin, exhibiting a glassy fracture: and when very carefully cleansed with water, and then dried in a vacuum over sulphuric acid and hydrate of potassa, it becomes very easily pulverized. When something warm, it becomes as soft and pliable as wax, and is easily drawn out into threads. When inflamed, it emits a brilliant flame, which near the border is of a greenish tinge, and leaves metallic platinum. Heated in a retort it swells considerably and emits an abundance of fumes, some part of which very easily condenses; and the carbonaceous residue burns slowly in the air, leaving platinum. A solution of potassa dissolves the resin completely, acetone nearly all of it, and alcohol and æther a sensible part. That portion which does not dissolve in the two last-mentioned liquids, forms a solution with acetone, from which æther separates a dark brown substance, dissolvable only in acetone and solution of potassa. This substance, for the present, I call *chloraceplatin*. That part which is dissolved by alcohol and æther, appears still to contain two distinct bodies. These, as well as the metacechlorplatin, and the remaining primary and secondary products, I hope, soon to be able to give a more copious description. In connexion with this enquiry, I am also preparing for an examination of the action of metaceton, pyroligneous spirit and oil of turpentine with perchloride of platinum. I also think of examining some other metallic chlorides and haloides with reference to those I have already studied.

LXV.   *On some electrical experiments.   By* M. VOORT,
   *Director of the Department of Natural Science, in
   Felix Mertis.   In a letter addressed to the Editor of the
   Annals of Electricity, &c.*

Sir,

On reading your memoir on the electric fluid, which
appeared in the Annals for June last. I became desirous of
making known to you an experiment which I have made two
years ago and communicated to our Society, Felix Mertis, to
prove my conviction that there is a difference between the
force of the positive and negative electric fluids. if two such
forces do exist ; or that, if we were still to keep close to Dr.
Franklin's theory, which I always admired, its passing from
one place to another might be made visible.   For this pur-
pose I employed the following apparatus : —

Let *a* and *a'*. fig. 5, Plate XIII., be two small Leyden jars
of three inches high and without covers. having the **two con-
ducting** wires of thin brass fastened in the bottom.   Charge
them at the same moment in the usual way at the conductor
of an electrical machine, only to a low degree : then place
them on another table at a great distance from the machine
to avoid every influence.   Put one of these charged jars on a
stand of glass *g*, and the other on the table as shown in the
figure.   Between these jars place a glass pillar, *o,* having
a steel point on its top, and on that point, an index of
12 inches in length is placed. which may move freely in
any direction.   This index is better represented in fig. 6,
where *c'* is the agate cap which rests on the pivot ; *c g* a
capillary glass tube with a fine copper ring at its outer
extremity ; and *c c'* a thin copper wire with small coun-
terpoise.   In commencing the experiment the ring should
be held fast either with the finger or, which is better,
by a piece of glass. just in the middle between the *outside*
of the jar on the glass stand, and the *conducting rod* resting
in the inside of the other ; so that it has *plus* and *minus*
electricity on the opposite sides of it.   If there be no current
of air nor moving of persons to disturb its tranquillity in the
room, conditions highly essential to ensure exact results. you
will perceive that, on leaving the index to itself, exactly at
right angles to a right line joining the two jars, it will
instantly move first to the positive side, (towards the rod of
the jar *a'*, on the table) where it will take a certain quantity
of electricity : enough to cause it by repulsion, from thence
to go over to the negative side of the jar *a,* which is placed

on the glass stand; and in this manner it will play to and
fro for a few minutes, causing the charge of the two jars to
diminish sensibly. Out of a hundred experiments of this
kind, the first movement was eighty times towards the positive
surface; and whenever the first motion of the index was
towards the negative surface it was not difficult to see that
there was a defect in the manner of operating either by a
little motion of the hand whilst leaving the index, or from
some other cause. I need not observe to you, sir, how difficult
it is to proceed with accuracy in such extremely delicate
experiments. If the jars are loaded too *strong* the move-
ment of the index will be too swift and uncertain. There
must be just enough force, and no more than is wanted, to
produce a gentle movement.

With high regards, I am,

Amsterdam, Your obedient servant,
December 25th, 1838. A. VOORT.

LXVI. *Curious Voltaic Experiment.*

We are indebted to Mr. Porrett for the discovery of a dis-
turbance of the common hydrostatical level of water by voltaic
action. Mr. Porrett's interesting experiment is well described
in the 8th. volume of the Annals of Philosophy. The arrange-
ment consisted of a glass vessel divided into two compart-
ments by a bladder partition. " One of these compartments
having been filled with water, and left for several hours, was
found to have retained the water. The bladder, therefore,
was not sufficiently porous to allow the water to filtrate
through it. The cell filled with water, was now positively
electrified, with a (voltaic) battery of 80 pairs of $1\frac{1}{4}$ inch
double plates, and a few drops of water were put into the
empty cell, so as to cover the bottom of it. This small quan-
tity of water was then negatively electrified" by placing in
it the wire from the negative pole of the battery. " Indepen-
dently of the decomposition of a small part of the water,
which of course took place in the usual manner, the principal
part of it obeyed the impulse of the voltaic current, *from the
positive to the negative wire,* first overcoming the resistance
occasioned by the compact texture of the bladder, so as in
half an hour to have brought the water in both cells to the
same level, and afterwards overcoming the additional resist-
ance occasioned by the gravitation of the water, by continuing
to convey that fluid into the negative cell, until its surface in
that cell was upwards of $\frac{3}{4}$ of an inch higher than in the posi-
tive cell." This experiment has been repeated by M. De la

Rive, and Mr. Mullins, with similar results.* But the for-
mer gentleman has stated that he could not succeed unless
distilled or river water was placed in the cells, for when he
employed a saline solution of a proper strength no such effect
of impulsion was perceptible.†

Having ascertained the correctness of the statements of
Mr. Porrett, I became desirous of knowing whether or no a
feebler voltaic intensity would produce a similar effect by a
longer continued action: and as earthen-ware diaphragms are
now much employed for separating the copper from the zinc
of a voltaic pair, I employed an unglazed stone-ware jar,
which was kindly given to me by Mr. Gassiot, for a first trial.
This jar is about 6 inches high and 3½ diameter. It was
partly filled with a solution of carbonate of soda, and placed
in a larger jar of porcelain, which was afterwards filled about
half way up with a solution of sulphate of copper.   A cylin-
der of rolled copper, sufficiently large to envelope the diaph-
ragm, was placed in the sulphate of copper, and a small cylin-
der of zinc in metallic connexion with the former, was placed
in the solution of soda. The liquids inside and outside of
the stone-ware diaphragm were of precisely the same altitude.
In three days a difference of altitude in the two liquid columns
was very perceptible, and in about ten days from the com-
mencement of the experiment, nearly the whole of the liquid
in the zinc cell was driven through the stone-ware partition
into the outer compartment. The copper solution was so
completely decomposed, that scarcely a trace could be detected
by liquid ammonia : and but a very slight tinge was given
to litmus paper, showing that the acid liberated from the sul-
phate had been either neutralized by the soda, or decomposed
by acting on the zinc : or its disappearance was probably oc-
casioned on both these accounts.   A thin crust of salt, (pro-
bably sulphate of soda) covered the moisture which was left
in the bottom of the diaphragm, and the zinc was much cor-
roded and partially covered with a white matter, partly soluble
and partly insoluble in water, the latter portion being a car-
bonate of the oxide of zinc.   I have repeated the experiment
with precisely the same results, obtaining a difference of al-
titude of four inches in the two liquids, the filtration being
always in the direction of the electric current. I have also
obtained similar electro-transportations of liquid media
through diaphragms of common tile, and soft red brick, and
I have no doubt of their being obtainable, by sandstone and
even thick masses of granite.

WILLIAM STURGEON.

* Annals of Electricity, &c., Vol. I. p. 206.
† Ibid.

LXVII. *Description of a new Galvanic Battery.*

Mr. Grove has made a voltaic battery consisting of alternate plates of iron and unglazed porcelain, fixed in a mill-board box, at some distance from one another, like the plates of a Cruickshank's battery. When the alternate cells are filled with a solution of the sulphate of copper, one side of each iron plate soon becomes coated with copper. The battery now consists of copper and iron, as decidedly as the Cruikshanks consists of copper and zinc; so that when the other alternate cells are filled with any liquid which will not act on the other face of the iron plates, the battery is ready for experiment. This is an exceedingly ingenious contrivance, but can only be used to advantage when solution of copper is one of the liquids employed.

The battery I am about to describe will be found exceedingly useful either for display on the lecture table, or as an implement of research. It consists of a rectangular wooden box as represented at the bottom of Plate IX., in which are cemented a series of voltaic pairs of copper and zinc, in the Cruikshank form. Every cell formed by these metals is divided into two compartments by a diaphragm of mill-board, also cemented into the sides and bottom of the box so as to be completely water-tight. The figure represents six metallic pairs and five diaphragms, the former by the *light* partitions and the latter by the *shaded* ones. As the mill-board is pervious to aqueous liquids it is obvious that any two kinds may be employed which may be found most suitable to operate with, the one on the copper side and the other on the zinc side. Twelve plates, 6 inches square, answer very well for decomposing acidulated water when a solution of sulphate of copper occupies the copper cells, and a dilute solution of soda the zinc cells. Such a battery will be found to possess many recommendations.

<div align="right">WILLIAM STURGEON.</div>

LXVIII. *On Voltaic Decompositions. By* G. MACKRELL, Esq. *Communicated in a letter addressed to To J. P. Gassiot, Esq.*

Read before the London Electrical Society Jan. 15, 1839.*

<div align="right">Cloudesly Square,
December 27, 1838.</div>

Dear Sir,

The experiments I am about to detail, and the conclusions at which I have arrived from them, having originated from a

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* Communicated by the Author.

paper read by Mr. Walker, at the Electrical Society, detailing some very remarkable and valuable experiments made by yourself and other gentlemen, at Clapham, has induced me to place this communication in your hands, to be used, as in your discretion you may think proper.

In the paper I have alluded to, many anomalies were shown to exist, in decomposition, with batteries arranged both in series, and not in series ; and this led Mr. Pollock and myself, to undertake some experiments to endeavour, as far as possible, to elucidate their causes.

The galvanic arrangement we employed, consisted of 48 cells ; the copper cylinders were 9 inches in circumference and three inches in depth ; the zinc 6½ inches in circumference and three inches in depth, separated from each other by a diaphragm of brown paper ; the solutions were sulphate of copper and salt and water : a quantity of the salts were placed in the cells to keep the solutions saturated.*

The battery was arranged in the usual way, and after allowing it to remain a short time, for the diaphragms to be moistened through, we tried the decomposing power of the whole 48 cells in union ; and found that it produced one cubic inch of the gases in 59 seconds; considering this action to be very weak, we allowed the battery to remain about half an hour, when we again repeated the experiment, and found that its action had considerably improved : now evolving the same measure of the gases in 49¼ seconds, and it continued at this rate the whole period of our first experiments.

The arrangement was next formed into six batteries of eight cells each, so that the positive poles might be brought into one cup of mercury, and the negative poles into another cup. For convenience, each battery was designated by a letter, and the time occupied in producing a cubic inch of the gases was as follows:—

A—2 experiments, each		60	seconds.
B—2	,,	53	,,
C—2	,,	57	,,
D—2	,,	60	,,
E—2	,,	63	,,
F—2	,,	52	,,
A & B—2	,,	32	,,
A, B, & C—2	,,	24	,,
A, B, C, & D—2	,,	21	,,
A, B, C, D, & E—2	,,	19	,,
A, B, C, D, E, & F—2	,,	17	,,

* The solution in the voltameter was one sulphuric acid of commerce and eleven distilled water, by measure.

The view we entertained previous to obtaining these results was, that the quantity of the gases evolved by the several batteries when united, should be equivalent to the sum of the quantities evolved by the same batteries in a like period of time when acting separately; thus the batteries A and B should have produced the cubic inch of the gases in 28¼ seconds, but it will be seen that it required 32 seconds; clearly showing that considerable interference had taken place.

It must be confessed, that there are several causes in operation, while performing experiments, which prevent a perfect agreement, between observation and hypothesis. In the first place ; may not the wire itself in its employment be a cause of this disagreement, by introducing a difference of surface of copper in the cell to which it is attached ; and does it not also interfere in the essential regularity of the voltaic arrangement, by being fixed to the zinc plate of the other extremity of the battery ; and may not the state of the mercury be a cause of disagreement, by introducing a foreign and, therefore, an interfering action, by the admixture of the metals with which it becomes amalgamated during its employment ; to which causes may be added the unequal insulation of the different batteries.

Believing, however, that we had perceived a certain progressive degree of regularity, when the batteries were brought successively into union, we determined to extend our investigations.

The arrangement was again made in batteries of eight cells each ; the positive electrodes were all brought to one side, and the negative ones to the other. This mode was adopted to prevent any accidental contact with the wires, which we feared might have occurred, although every precaution was taken to prevent it : in every other respect the arrangement was as before, and the time in obtaining the cubic inch of the gases was, with battery

A—3	experiments, each	57	seconds.
B—2	,,	,,	58	,,
C—2	,,	,,	58	,,
D—2	,,	,,	61	,,
E—3	,,	,,	190	,,
F—2	,,	,,	60	,,

It will be perceived there was a considerable defect in battery E; it was therefore removed from the arrangement ;—the remaining batteries were again tested, and finding the results as before stated, we proceeded with batteries

A & F—2 experiments, each 32$\frac{1}{3}$ seconds.
B & F—2   ,,   ,,   35
C & F—2   ,,   ,,   32$\frac{1}{3}$
D & F—2   ,, .   ,,   32$\frac{1}{3}$
A, B, & C—2   ,,   ,,   25
A, C, & D—2   ,,   ,,   25$\frac{1}{4}$
A, B, & F—2   ,,   ,,   25$\frac{3}{4}$
A, B, D, & F—3 ,,   ,,   22
A, C, D, & F—2 ,,   ,,   21
A, B, C, D, & F—2 ,,   ,,   19$\frac{1}{2}$

Having obtained these additional results, we now proceeded to find the ratios of the differences, with a view of ascertaining if they were governed by any regular progression.

It will be found that the mean time required to produce a cubic inch of the gases, in the second set of experiments, was with

A single battery 58.8″
Two batteries   33.12″
Three   ,,   25.33″
Four   ,,   21.5″
Five   ,,   19.5″

but if the batteries were acting separately the results would be

For one battery   58.8″
Two batteries   29.4″
Three   ,,   19.6″
Four   ,,   14.7″
Five   ,,   11.76″

the difference, therefore, arisiring from interference, between the view we had taken and the experiments, will be

In two batteries 3.72″
Three   ,,   5.73″
Four   ,,   6.8″
Five   ,,   7.74″

From these ratios, it appears that the following formula will very nearly give the true result. a being taken for the excess of time required for two batteries in union, over the same two batteries when acting separately. In the foregoing case, a is seen to be 3. 72″; then $a + \frac{a}{2}$ will express the excess for three batteries; $a + \frac{a}{2} + \frac{a}{3}$ that for four batteries; $a + \frac{a}{2} + \frac{a}{3} + \frac{a}{4}$ that for five batteries; and so on; thus

$$a + \frac{a}{2} + \frac{a}{3} + \frac{a}{4} \ldots \ldots + \frac{a}{n-1} \text{ for } n \text{ batteries.}$$

The following table will show the result from our experiments.

	Mean time by experiment.	Mean time of batteries were acting separately.	Difference.	Difference by the proposed formula.
First set of experiments.				
1 battery.	57.5″	57.5″		
2 ..	32	28.75	3.25″	
3 ..	24	19.17	4.83	4.87″
4 ..	21	14.37	6.63	5.96
5 ..	19	11.5	7.5	6.78
6 ..	17	9 5	7.5	7.43
Second set of experiments				
1 battery.	58.8	58.8		
2 ..	33.12	29.4	3.72	
3 ..	25.33	19.6	5.73	5.58
4 ..	21.5	14.7	6.8	6.82
5 ..	19.5	11.7	7.8	7.75

The close approach of the proposed formula for the interference to the observations is remarkable, being in the second set of experiments within one seventh, one fiftieth, and one twentieth of a second; and may not even these trifling differences arise from the imperfect nature of the experiments? It is therefore desirable the subject should be investigated by more correct manipulation than can be expected from amateurs; and we trust that some gentlemen more competent to the undertaking will shortly perform this task, particularly as relates to the difference of the two first batteries, from which our standard data of interference are assumed; and the laws of which our time did not allow us to examine.

We are aware that this communication may be objected to, as it adds nothing to physical science; but it must be admitted, that if the interference of voltaic arrangements can be estimated from an examination of the two first batteries, one point at least is gained: and if electricity can be brought under mathematical laws, the benefits emanating from thence would be incalculable.

I remain, dear sir,
Your's very truly,
G. MACKRELL

LXIX. *Facts and observations for the purpose of illustrating a Theory intended to connect the Operations of Nature upon general principles.* By PAUL COOPER, ESQ.
(Continued from page 403.)

152. Having investigated the nature and characters of the resistance presented by the interposition of a reversed

pair of plates, I shall now endeavour to explain the cause of resistance when plates of platina only are interposed.

" When only one pair of zinc and platina plates was used, the current of electricity was entirely stopped to all practical purposes by interposing one platina plate, fig. 80, Plate X. *i. e.* by requiring of the current that it should decompose water, and evolve both its elements, before it should pass. This consequence is in perfect accordance with the views before given. For as the whole result depends upon the opposition of forces at the places of electric excitement and electro-decomposition, and as water is the substance to be decomposed at both before the current can move, it is not to be expected that the zinc should have such powerful attraction for oxygen, as not only to be able to take it from its associated hydrogen, but leave such a surplus of force as, passing to the second place of decomposition, should be there able to effect a second separation of the elements of water. Such an effect would require that the force of attraction between zinc and oxygen should under the circumstances be *at least* twice as great as the force of attraction between the oxygen and hydrogen." (Researches 1011.)

153.   Platina is naturally negative to hydrogen (87), and in this state the electro-motive force would be from the platina to the hydrogen ; but the inductive influence of the zinc, acting upon the end plate through the wire A B, produces the arrangement in this plate represented in the figure by the usual positive and negative signs : in this state the surface of the platina presented to the hydrogen is positive to it, and the electro-motive force is from the hydrogen to the platina. Now, this arrangement is quite as necessary in the interposed platina plate as in the end plate, not only to bring the particles of water into the electrolytic order described in the figure, but also to enable the current to take the proper direction ; it being essential to the production of electrolytic effects in the second cell that it should enter this cell from a negative surface of the platina (140). But the inductive forces of the connected zinc and platina plates can only be conveyed to the interposed platina plate through the medium of the electrolyte ; and these forces, thus conducted, are unable to control the electro-motive force of the platina to the hydrogen, and give a negative surface to it upon the opposite side of the plate to that which presents it under the inductive influence of the hydrogen. The polar arrangement in the interposed plate, as it is represented in the figure, which is essential to electrolytic action, does not, in fact, take place under these circumstances ; and the current as far as this principle is concerned is entirely stopped. The latter part of the para-

graph quoted (152), clearly shows that Dr. Faraday attributes the electro-motive force to the greater attraction of zinc for oxygen than hydrogen for oxygen (80); but I would with great deference suggest to him, that this conclusion is in direct opposition to his own more general views. (Researches 669.)

154. " When two pair of zinc and platina exciting plates were used, the current was also practically stopped by one interposed platina plate." (Researches 1012.)

" Three pairs of platina plates, fig. 81, were able to produce a current which could pass an interposed platina plate, and effect the electrolyzation of water in cell 4." (Researches 1013.)

In this experiment the electrolytic arrangement described in figs. 80 and 81, was first actually made; the feeble current observed in the other experiments (Researches 1017) must have proceeded from a partial arrangement in cell 1, fig. 80, and a current through the other parts of the circuit conducted upon the same principle as common electricity (110).

155. " The three pairs of platina plates were now opposed by two intervening plates. In this case the current was stopped." (Researches 1014.)

" Four pairs of zinc and platina plates were also neutralized by two interposed platina plates." (Researches 1015.)

" Five pairs of zinc and platina, with two interposed platina plates, gave a feeble current, with decomposition in all the cells. (Researches 1016.)

These experiments may, perhaps, be made more instructive by putting them in a point of view in which they will be more familiar to us. A little consideration will render it evident, that if A B, fig. 81, be a platina wire, the end platina plate will form only a continuation of it ; and that if the end of the wire be presented to the electrolyte in cell 4, the plate may be removed without affecting the experiment. The circuit in this case will be completed by means of the interposed platina plate, through the medium of the electrolyte ; but it will be precisely the same thing if we break the wire A B, and instead of completing the circuit through the medium of the electrolyte in cell 4, complete it by bringing one end of the wire B into contact with the interposed plate, placing the electrolyte between its other end and the end of the wire A, from which it was separated. In fact the experiment is in no respect different in principle from that with which we are so well acquainted ; in which we connect a platina wire with the end zinc plate, and a similar wire with the end platina plate ; the interposed plate being removed and the opposite ends of

the wires connected through the medium of an electrolyte in the experimental cell.   In the experiments with three, four, and five pairs of zinc and platina plates, and two intervening platina plates; the experiments would be the same in principle if we were to remove the interposed plates, break the wire A B at the points A and B, and complete the circuit by interposing, at both these points, an electrolyte similar to that in the cells of the battery.   It will be observed, "that variation in the size of the electrodes causes no variation in the chemical action."   (Researches 722.)

156.   When a zinc plate was substituted for the interposed platina plate in fig. 80, there was as powerful a current, apparently, as if the interposed zinc plate was away."   (Researches 1027.)

In this experiment, zinc being positive to hydrogen (86), the electro-motive force of the interposed plate was from the hydrogen to the zinc and from the zinc to the oxygen, in concurrence with the direction of the current; it must, therefore, have had a tendency to increase its force, rather than to obstruct it.   When the experiment was varied, by interposing two and three zinc plates, the electric current passed as freely as if there had been no such plates in the way.   (Researches 1029.)

157.   "The effects of retardation were altered altogether when changes were made in the *nature of the liquid* used between the plates, either in what may be called the *exciting* or the *retarding* cells.   Thus, retaining the exciting force the same, by still using pure dilute sulphuric acid for that purpose, if a little nitric acid were added to the liquid in the *retarding* cells, then the transmission of the current was very much facilitated.   For instance, in the experiment with one pair of exciting plates and one intervening plate (152), fig. 80 when a few drops of nitric acid were added to the contents of cell 2, then the current of electricity passed with considerable strength, and the same good effect was produced by the nitric acid when many interposed plates were used."   (Researches 1020.)

"When a little nitric acid was put into the exciting cells, then again the circumstances favouring the transmission of the current were strengthened, for the *intensity* of the current itself was increased by the addition.   When, therefore, a little nitric acid was added to both the *exciting* and the *retarding* cells, the current of electricity passed with very considerable freedom.   (Researches 1022.)

LXX. *The London Electrical Society.*

Tuesday, January 15*th.* A paper on Voltaic Decomposi-
tions was read by G. Mackrell Esq. This paper is given
entire at page 495.

Tuesday, February 5th. A paper was read by C. V. Walker
Esq. describing a series of experiments on the decomposing
power of a voltaic battery belonging to T. Mason, Esq. The
experiments were made at Mr. Mason's house and the battery
arrangements were entirely his own. Ninety-nine porcelain
jars, each furnished with a copper and zinc cylinder, and fitted
up in a similar manner to those used in the previous experi-
ments made at the house of J. P. Gassiot, Esq. and described
at page 420 of this volume, were arranged in nine groups in
series of eleven jars in each group. The groups were first tried
separately in order to ascertain the mean power of each ; and
afterwards they were combined *laterally,* commencing with
two groups, and after ascertaining their united power pro-
ceeding with *three* groups, and so on, adding another group to
every preceding combination, until the whole of the groups
were brought into play in eight distinct combinations, each of
which had all its *positive* poles in connexion with one of the
metals of the gasometer, and all its negative poles in connexion
with the other; the *series*, of course, in all cases being eleven
pairs. If now we represent the number of groups in each com-
bination by the digits 2, 3, 4, 5, 6, 7, 8, 9, and the quantity of
gas liberated in one second of time, by each individual group
by g, then the gas liberated by each combination of groups
would be $2g$, $3g$, $4g$, $5g$, $6g$, $7g$, $8g$, $9g$, respectively, or the
quantity of gas liberated in these experiments was exactly
proportional to the quantity of metallic surface in the battery,
when the time is constant, a result never before verified ex-
perimentally. And what was still more interesting, similar
results were obtained with two gasometers whose platinum
terminals exposed very different extents of surface to the acid-
ulated water in the instruments, only one of those instruments
being used in each series of experiments; for although the
larger platinum terminals liberated more gas than the small
ones, the quantities liberated by each gasometer were propor-
tional to the voltaic surfaces employed. The batteries were
brought into play at 3 o'clock in the afternoon of the 26th
Dec., 1838, and were kept in action till 3 o'clock the following
morning.* The experiments were conducted by Mr. Mason,

* From these results it would appear that a series of *eleven* pairs
of this kind is very near the proper unit of battery intensity for this
class of decompositions. EDIT.

Mr. Gassiot, Mr. Sturgeon, and Mr. Walker (the same party who carried on the experiments with Mr. Gassiot's battery at Clapham). The whole of Mr. Mason's battery was put in series of 100 pairs for the purpose of ascertaining its *striking distance*, but none could be discovered though sought for with great care. Whether the polar wires were furnished with charcoal points or not, the force of the battery would not strike through the thinnest stratum of air, though a flame of considerable length could be kept in play after once the points had been in close contact and removed slowly from one another.

Read also an extract from a letter from W. L. Wharton, Esq., describing some singular effects of a thunder storm.

Tuesday, February 19th. Read an extract of a letter from S. Samo, Esq., dated Surinam, December 31, 1838, addressed to Walter Hawkins, Esq. The Gymnotus Electricus is found in the rivers of Surinam, but the writer states that it is very difficult to select the true from the fabulous accounts of the habits of this creature. It is generally found in shallow rivulets of fresh water, having a rocky uneven bottom, and principally in those parts which are shaded by high trees. Mr. Samo describes the manner in which the eels are caught, and states that one of the three which he had waiting for an opportunity to forward to the Electrical Society, had been attacked and destroyed by water rats; which animals he concludes must be insensible to the electric shock of the gymnotus, although horses can be paralyzed by them.

A paper was read by Mr. Pollock, on the occurrence of the striking distance in frictional, but not voltaic electricity.

Mr. Pollock considers one of the most common of the phenomena attendant upon the action of the electrical machine is the production of the spark upon presenting any body, such as the knuckle of the hand, to the conductor; as this occurs when the body is at a certain distance, this has been termed the striking distance; sparks also occur in the phenomena of voltaic electricity, and it has been very generally supposed, that in this respect there existed no difference in the two actions; but recent investigations, made with most minute attention to accuracy, throw considerable doubt upon this conclusion, showing that it has been too hastily made.

It has been shown on bringing the two terminal wires of a voltaic battery so near each other that the distance between them has been so small as to be scarcely appreciable, yet no spark has occurred; this circumstance compels us to admit, that although the transmission of the spark may not be impossible, yet a most marked distinction between these two kinds of electricity is manifest.

The object of Mr. Pollock's paper is to demonstrate that by the changes in the relations between the rubber and cylinder of the electrical machine, and between the metal zinc and its oxide of the voltaic battery, and their respective latent heats while each is in action, must be of an opposite kind, and, therefore, this distinction between common and voltaic electricity might have been predicted.

Mr. Sturgeon exhibited to the Society the results of his repetition of Mr. Fox's experiments on the conversion of copper ore from one kind to another by the influence of voltaic electricity. (See Vol. I. p. 133.)

The copper ore, in this experiment, formed one side of a voltaic pair, and a slip of zinc the other, being attached to each other by copper wire, and placed in a porcelain pot with a partition of clay between them. A solution of sulphate of copper covered the ore, and water with a few drops of sulphuric acid covered the zinc. The action was kept up for *five* successive months, during which time several new pieces of zinc had to be soldered to the end of the connecting wire, to replace their dissolved predecessors: so that much sulphate of zinc was formed in the zinc cell, the sulphuric acid being supplied from the decomposed sulphate of copper in the other cell; and the liberated copper attached itself to the copper ore and wire which surrounded it. Fresh supplies of bruised crystals of the sulphate were put into the liquid when found necessary.

When the particulars of the process had been described, Mr. Sturgeon broke the galvanized ore into several fragments before those who attended the meeting, and it was examined and compared with another specimen, also broken at the time, but not galvanized. The two specimens exhibited very different fractures, the ungalvanized being of a brilliant yellow, and the galvanized a dull blackish appearance, something resembling the colour of plumbago. The galvanized ore was completely cased with the precipitated copper, and every fracture presented nearly the same dull colour. It is to be understood that both specimens were taken from the same mass of copper ore prior to the experiment. Because of some difference of opinion existing respecting Mr. Fox's experiment, Mr. Sturgeon did not think proper to give any opinion regarding the nature of change which had taken place in the ore on which his experiment had been made. He brought the specimen without knowing whether any change had or had not taken place; and having broken it in presence of the Society, and submitted it to their inspection, he considered he had done his duty to all parties, and left the chemical analysis of

the result of this exceedingly tedious experiment, to be undertaken by some other person. J. P. Gassiot, Esq., Treasurer to the Society, took two fragments, one of the galvanized and the other of the ungalvanized ore, which he has put into the hands of an eminent chemist for analysis. The clay partition was not laminated.*

LXXI. MISCELLANEOUS ARTICLES.

Mr. Harris's Unit Jar is represented by fig. 7, Plate XIII. It is formed of a small inverted Leyden jar, supported and insulated by a slender glass rod, covered with lac varnish, and fixed into a wooden sole as seen in the figure. The inner coating of the jar is in metallic contact with a brass ball and wire *d*, and *a*, which wire communicates with a prime conductor. Another ball, above the former, is in contact with the outer coating of the jar by means of a metallic frame and sliding wire. The brass ball *g*, and wire *b*, are also in connexion with the outer coating. If the wire *b*, be connected with the ground whilst the machine is in motion, the jar is charged in the usual manner and is discharged between the ball *d*, and the one above it : and in this way a succession of sparks may be produced by *equal quantities* of the electric fluid. The use of this instrument is to charge other jars with known proportions of the electric matter, which is done by placing the wire *b*, in connexion with the inside of the other jar or jars until a certain number of sparks have been discharged between the inside and outside of the unit jar, and so on to the inside of the charging jars on the table.

To the Editor of the Annals of Electricity, &c.

Dundanion, Cork, Nov. 5, 1838.

Sir,

The Annals of Electricity published by you have lately directed my attention to that very interesting subject. Aware that the success of many experiments depends upon the intensity and permanency of magnetism that can be given to

* Having failed in two instances, in laminating clay by the action of a single pair, I have placed another portion in the circuit of a series of six porcelain cells, with their copper and zinc cylinders; but as I find that the experiment will be exceedingly tedious, and require attention and time which I cannot well spare, I am not certain that I shall continue it. W. S.

steel magnets, I had 10 of equal size from the same bar of cast steel formed and numbered from 1 to 10. No. 1, 2, 3, 4, 5, 6, 7, and 8, I made as hard as possible, 9 and 10 I left as they came from the forge ; all were their ground and polished ; 1 and 2 I left perfectly hard ; 3, 4, 5, and 6, I tempered down to different shades ; 7 and 8 I laid across a bar of red hot iron until the blue shade appeared in the centre, they were then removed and allowed to cool gradually and *when cooling* they were touched with a powerful magnet. All were touched with the same magnet, then tried, No. 7 and 8 raised 20 per cent more than those reduced to a cherry red, and 42 per cent more than No. 1 and 2, and No. 7 was somewhat more powerful than No. 8 which I attribute to its having been touched while in a warmer state.

<div align="center">I have the honour to be,

Your most obedient servant,

R. NEWENHAM.</div>

The experienced electrician knows well that if he requires a long spark from the prime conductor of a machine he must commence by taking short ones, and gradually lengthen them to their maximum. A corresponding fact is found in the action of a voltaic battery. No appreciable *striking distance* is passed through by a series of 300 pairs, until the polar wires have first been in contact ; this done, however, the fluid may be made to pass through a considerable space by withdrawing the wires gradually from one another. In both cases the short sparks prepare the way for the longer ones, probably by an attenuation of the air, or by a stream of infinitely small metallic particles which are carried between the wires : probably on both these accounts. If the polar wires of a voltaic battery be placed at a small distance from one another, and a Leyden jar be discharged through the intervening space, the voltaic discharge also takes place, and continues as if the wires had previously touched one another. This last fact we believe is due to Sir John Herschel, who produced it a few days ago. EDIT.

"I have just learned that Professor Jacobi is occupying himself with a discovery which may, if in the end successful, prove of far more use than Daguerre's. He observed that the copper deposited by galvanic action on his plates of copper could, by certain precautions, be removed from those plates in

perfect sheets which presented in relief most accurately every accidental indentation on the original plate. Following up this remark, he employed an engraved copper plate for his battery, caused the deposit to be formed on it, removed it by some means or other, he found that the engraving was printed thereon in relief (like a wood cut), and sharp enough to print from. Whether a repetition of the process from this galvanically formed block will furnish, in its turn, a copper plate from which impressions can be thrown off is not yet established."

JULIAN GUGGSWORTH.

Wormwood Scrubs,
   Feb. 5, 1839.

To the Editor of the Annals of Electricity, &c.

Manchester, September 17, 1838.

Dear Sir,

I have often during thunder storms noticed that the flashes of lightning do not always appear momentary, but frequently as if there were two distinct flashes, one succeeding the other at an interval of about $\frac{1}{4}''$ of time.

And indeed, this fact cannot have escaped the observation of any attentive person, who has seen the lightning of a distant storm at night; then, the flame of light is in general seen to shine brightly at first, then partly to die away, and at last to end with about the same lustre with which it began; the whole scene taking up in some cases a length of time so palpable as one second. In this case, the lightning has lost so much of its dazzling brilliancy, that the evidence of our senses may safely be trusted.

For my own part I feel quite unable to furnish any hypothesis at all calculated to account for the above phenomenon; I present it therefore to you and your many scientific readers for explanation.

               I am, dear sir,
                   Your obedient servant,
                         P.

THE ANNALS

OF

ELECTRICITY, MAGNETISM,

AND CHEMISTRY;

AND

Guardian of Experimental Science.

OCTOBER, 1839.

XVI. *Experimental and Theoretical Researches in Elec-
tricity, Magnetism, &c. Fourth Memoir. By* WILLIAM
STURGEON, Lecturer on Experimental Philosophy at the
Honourable East India Company's Military Seminary,
Addiscombe, &c.

On Marine Lightning Conductors.

The proper adaptation of lightning conductors to shipping being a subject of vital
inportance to every maritime community, both British and Foreign, the author
has been induced, respectfully, to submit this memoir to the consideration of the
following scientific bodies.

British Dominions. The Royal Societies of London and Edinburgh.—The
Royal Irish Academy.—The Cambridge University Philosophical Society.—The
Philosophical Societies of Newcastle-upon-Tyne, Bristol, Manchester, Hull,
Liverpool, and Yorkshire.

Foreign. The Royal Society of Sciences at Copenhagen.—The Royal Academy
of Sciences at Paris.—The Royal Academy of Sciences at Thoulouse.—The
Royal Academy of Sciences at Berlin.—The Italian Society of Sciences at
Modena —The Royal Academy of Sciences at Turin.—The Royal Academy of
Sciences at Brussels —The Royal Institute of Amsterdam.—The Royal Academy
of Sciences at Lisbon.—The Imperial Academy of Sciences at St. Petersburgh.
—The Royal Academy of Sciences at Stockholm.—The Royal Society of
Sciences at Dronthem —The American Philosophical Society at Philadelphia.—
The New York Philosophical Society.—The American Academy of Sciences at
Boston.

172. In addressing this memoir to the British Association
for the Promotion of Science, I am far from supposing that there
can exist a necessity for entering into a routine of details in
order that the subject may appear sufficiently important for
its consideration. It is one well known to be, at all times,
interesting to the philosopher, and, at the present moment,

when the British fleets are about to be furnished with light-
ning conductors, it has become a subject of high national
importance, and demands the most profound consideration of
every experienced electrician in the land, in order that the
most efficient and economical plan may be adopted and
carried into effect.

173. Without any pretensions to a knowledge of the motives
for not making so momentous and unsettled a topic open to fair
scientific discussion, there can be no doubt of the scantiness
of publicity being the sole cause of so *few plans** being
brought before the notice of the Committee appointed to
enquire into the best means of protecting shipping from the
effects of lightning. I am not aware of the objections of the
Committee to the plan proposed by Mr. Martyn Roberts nor
of the reasons for giving a preference to that so long proposed
by Mr. W. S. Harris: but as the House of Commons has
been officially informed that the Committee have determined
in favour of the conductors of the latter gentleman, the sub-
ject is now open to impartial discussion ; and as it is possible
that, unless some timely council interpose, the recommenda-
tions of the Committee may induce the Admiralty to adopt
Mr. Harris's conductors, and, without further enquiry respect-
ing the efficiency, or inefficiency, in parrying the effects of
lightning, give immediate orders for their application to Her
Majesty's fleets, no time ought to be lost in placing Mr.
Harris's system of conductors in a *proper light* before the
Government and maritime community of these realms.

174. There certainly cannot be a finer opportunity offered
to the British Association for exercising its almost boundless
influence in eliciting important philosophical truths, and ex-
tending the benefits of experimental and practical science,
than that now presented by this momentous topic ; nor can
electricians of every country have a more favourable opportu-
nity of becoming benefactors to mankind, than by giving this
universally important subject their profoundest contemplation;
and, as soon as possible, making known the results of their

* Only two plans were brought before the Committee, the one
by Mr. Martyn Roberts, and the other by Mr. W. S. Harris. The
plan of the former gentleman consists of a rope of metallic wire
one end of which is hoisted to the mast head, and the other thrown
over the side of the vessel. See Annals of Electricity, &c., Vol. I.
p. 468, Vol. II. p. 241.

The plan of the latter gentleman consists of strips of sheet copper,
one to each mast, let into a groove the whole length of the after-side,
and passing through the keel of the vessel, and that belonging to
the mizen mast passes either through or close to the after powder
magazine.

experimental investigations in this branch of electricity, and their uncompromising opinions on Mr. Harris's system of conductors, on which so much deep interest is now involved: or by proposing any better plan of protection to the thousands of lives and millions of property continually exposed to the most formidable and destructive of nature's elements.

175. The present memoir contains:— *First*, an examination of those experiments which Mr. Harris has exhibited on various occasions, and at various places, in illustration of the supposed superiority of his system of conductors. *Second*, an examination of the *observed* effects produced on shipping by lightning. *Third*, a statement of electrical phenomena similar to those effects, and probably productive of others no less destructive. *Fourth*, a comparison of the *observed* effects of lightning and the *probable* effects which lightning would produce by the adoption of Mr. Harris's system of conductors. *Fifth*, a description of a new system of conductors for the protection of shipping from lightning.

Examination of Mr. W. Snow Harris's experiments.

176. *Experiment* 1. Let *j*, fig. 1, Plate IV., represent the electrical jar on board the Caledonia; *c* the Louisa cutter, with a strip of sheet copper let into the after side of her mast from top to bottom, and continued, through her keelson, to the sea, as represented by the strong black line. By means of a chain *o o o*, the lower extremity of this conductor is brought into metallic connexion with a loaded brass howitzer in the boat B, moored at some distance from the cutter. A wire *w w w*, proceeds from the outside of the jar to the boat, and terminates above the vent of the howitzer, leaving a small interruption for the introduction of a priming of *detonating* powder. By means of another wire *w' w'*, one end of which is fastened to the conductor at the mast head of the cutter, the jar is discharged in the direction indicated by the arrows; and the spark which passes between the howitzer and the end of the wire *w w w*, ignites the *detonating* powder, and the piece thus becomes discharged: whilst a portion of *common gunpowder*, placed in contact with the *uninterrupted* conductor at *p* in the cutter, *is not exploded*.

Experiment 2. The only difference in the arrangement from that in the last experiment, is simply that of interrupting the metallic connexion between the lower extremity of the cutter's conductor, and the howitzer in the boat B. and permitting the sea to form part of the circuit. The effects were the same as in the first experiment.

Experiment 3.   The arrangement of apparatus the same as before, with the exception of an interruption in the cutter's conductor at *p* by passing a saw through it.   No gunpowder nor *detonating* powder at *p*.   The howitzer was discharged as in the preceding experiments.

177.   These three experiments were made in Plymouth harbour, in September, 1822, "in presence of the Navy Board, Sir Alexander Cochrane, Commissioner Schield several Captains of the Navy, and the principal Officers of the Dockyard," with the intention of convincing them of the efficacy and superiority of Mr. Harris's conductors: although it must be obvious to every electrician that these experiments had no peculiar bearing on any peculiar plan of conductors whatever: for they are just as applicable to the illustration of the efficacy of the *present chain conductors* or to the *metallic rope* proposed by Mr. Roberts, as to those proposed by Mr. Harris.

178.   The first experiment showed that copper is a conductor of electricity : that *detonating* powder can be ignited by an electric spark : and that *common gunpowder* will *not* ignite by the application of cold copper, though a feeble momentary electric current traverses the metal at the same time.   The second experiment, in addition to the facts shown by the first, shows that sea water is a conductor of electricity.   The third experiment, in addition to the facts shown by the first and second, shows that a *slight* interruption in the metallic part of the circuit did not prevent a spark, from the jar employed, from igniting *detonating* powder at another *slight* interruption of that circuit.

179.   These, I believe, are the only facts which Mr. Harris's experiments, exhibited in presence of the Navy Board, are calculated to illustrate ; and as they prove nothing novel, nor anything peculiar to that gentleman's system of conductors, they not only fall short of their object, but are perfectly inapplicable to the great question at issue.

180.   There are, however, circumstances connected with these experiments, which, though not noticed by Mr. Harris, would have been quite as interesting to the Navy Board as the results which they witnessed.   Mr. Harris could easily have shown the officers of the Navy Board, that a moderate   electrical discharge would render a **copper** conductor *red hot*, and that gunpowder in contact **with** this electro-heated metal would explode as decidedly as **if** heated by any other means : and it would have been no **more** than a just inference to have added, that, a lightning **conductor** may be sufficiently heated to fire gunpowder, by a **discharge** of lightning from the clouds.   It would have

been well, also, to have shown that *detonating* powder would be as sure to ignite by the spark at the interruption made by the saw in the cutter's conductor, as by a spark at the howitzer. From the first of these facts, those officers might probably have inferred that a lightning conductor passing through the powder magazine would be the most dangerous appendage in the ship: and from the latter they would have implied that the most trifling interruption in the conductor, from accident or otherwise, might be productive of serious evils. [*]

181. In a lecture delivered before the members of the British Association at the Liverpool meeting, Mr. Harris brought forward the following additional experiment in illustration of the advantages of his system of lightning conductors.

Experiment 4. In fig. 2, *a b* and *c d* represent the two halves of a long tapering round pole, cut along its axis from end to end, having the flat sides of both halves towards the spectator. In the axis of *a b* are embedded three pieces of metallic wire *a n*, *n m*, and *m b*. At the interruptions *n* and *m*, are small cavities in the wood for the lodgment of gunpowder and *detonating* powder ; and when charged with these combustibles the flat side of *c d* is placed against the flat side of *a b*, the two being slightly kept together by a suitable contrivance. The outer surface of this compound piece is furnished with a strip of tinfoil which reaches from top to bottom: and is then represented as a perfect model of a ship's mast furnished with Mr. Harris's conductors.

When a jar is discharged from the top to the bottom of this mast, the electric fluid traverses the tinfoil on the outside, and the mast is protected. This done, a portion of the tinfoil conductor, reaching from above the opening at *n*, down to below the other opening at *m*, is taken away : and the next discharge of the jar necessarily passes through the axial wires, and ignites the powder in the two cavities *n* and *m* ; and the explosive force of the gunpowder blows off the moveable half *c d* of the mast. This effect was held forth as an exemplification of the effects of lightning on a ship's mast.

182. Whatever may be the merits or demerits of Mr. Harris's system of conductors, it is very remarkable, that the experiments exhibited by that gentleman in its favour, before the Navy Board, at Plymouth, in 1822, and repeated in his lectures before the British Association, at Liverpool, and other places, happen to have no bearing whatever on the subject.

* I believe, however, that Mr. Harris has taken every precaution to prevent interruptions in the conductors which are applied to the masts: but at the steps of the masts, and between the copper bands and bolts in the keelson, it will always be difficult to keep metallic contact so as to prevent explosions

Those experiments are no more illustrative of the efficacy of
Mr. Harris's system than of any other system ever yet offered
to public notice: and as I am not aware that any other
experiments have been attempted for demonstration, I have
no means of knowing how that gentleman has been led to
neglect some of those considerations, which, next to the
selection of the conducting material, are certainly the most
important in the equipment of shipping with lightning con-
ductors.

Examination of the observed effects produced on shipping by lightning.

183.   A general outline of the effects of lightning on ship-
ping cannot be better pourtrayed than by the following des-
cription of those produced on the Rodney, on the 7th of
December last.

FATAL EFFECTS OF LIGHTNING ON H.M.S. RODNEY, IN THE MEDITERRANEAN.

H.M.S. RODNEY, 7th December, 1838, was 8 or 10 leagues
to the eastward of Cape Passaro, blowing strong with squally
overcast and rainy weather, under close reefed fore and mizen
and treble reefed main top sail.   At 9 A.M., in a heavy squall
attended with hail and hard rain, the ship was struck with
lightning.   Three or four flashes of angry looking forked
lightning had been seen before in the northern horizon, not
attaining an altitude of apparently more than 10 or 15
degrees, and the writer thinks it here necessary to mention
that this statement is drawn up entirely from his own ideas,
sensations, and observations on the occasion, and not from
any hearsay reports, because many on board saw and heard
things differently to himself.   He was standing with Captain
Parker close to the wheel at the time.   No very particularly
vivid flash or unusually great glare of light was observed at
the time, although people are commonly blinded for some
seconds on such occasions, but the first thing heard was the
burst or explosion, which was louder than one of the ship's 32
pounders, but not exactly like it, it being sharper and more
piercing, something like the bursting of a bomb shell; and
then the iron hoops of the main mast came rattling down the
mast, being burst asunder.   The captain and the writer looked
at each other in silent astonishment, then forward and aloft,
when the hoops were seen tumbling down the main mast, and
splinters flying about in all directions and to leeward to an
immense extent.   The main topgallant mast was shivered to

atoms, leaving the main topgallant yard across the cap, and the royal pole from the hounds of the topgallant rigging to the truck perfect, which was left sticking up above the topgallant yard, having slipped itself on the upper part of the topmast rigging. In perhaps about a minute or so, flames were seen issuing from the bunt of the topsail yard, the electric fluid having ignited the paunch mat and gear thereabouts; it burnt some minutes before it could be extinguished by wet swabs, water, &c., &c.; fortunately from the heavy rain every thing was perfectly saturated, or no one knows where the mischief would have ended. The electric fluid was seen to pass from the mast about 7 feet above the deck over the starboard hammock netting to leeward (right over a gun) like a ball of fire. Luckily all the people on deck had been ordered to take shelter out of the rain, consequently nobody was near the main mast at the time, and nobody suffered excepting the unfortunate men who were at the mast head.

Progressive course of the Electric Fluid.—The vane staff which is 6 feet long with a copper spindle (on which the vane traverses) of about 10 inches in length, surmounted by a gilt wooden ball, the size of an orange, shows its first effect (the ball and spindle were never seen after the shock), being split but not broken, and one side of it blackened; the copper binder round the truck was burst asunder, a small piece broken out of the truck, and one of the metal sheaves for signal halyards slightly fused. From this, after leaving the royal pole uninjured, it appears to have passed inside the copper funnel for topgallant rigging and iron hoop of the hounds of the mast, shivering the topgallant mast to atoms, from thence to the topmast cap, not a piece having been seen the size of a common walking cane, and the sea was literally covered with its splinters to a considerable extent; its marks are now lost for many feet, notwithstanding the shock about this spot must have been most terrific, as it was in the topmast crosstrees where the poor fellows who suffered were at the time, and also the heel of the topgallant mast (which was not at all injured below the cap) was forced upwards into the cap, the fid being raised about 8 inches above the trussel trees with such force that the top burton block strop was carried away in trying to house it down again, and after all we were obliged to cut it out, not being able to clear it in any other way. Its next appearance is on the main topmast, 10 feet above the cap, seemingly attracted by the iron-bound tye blocks and iron hoops on the topsail yard (being under a treble reefed topsail), from whence it rent an immense splinter out of the mast down to the lower cap, going nearly into the

core of the mast and set fire to the tarry and greasy gear
about the bunt of the topsail yard, after taking this large
splinter of nearly one quarter of the substance of the mast
away. Its next positive mark is on the starboard lower trussel
tree, the lower cap, head of lower mast, and heel of topmast
(both iron hooped), having escaped unhurt. It shook and
blackened the trussel tree, rendering it unserviceable, and
then must have entered the main mast, spreading and passing
down both sides, bursting 13 of the large iron hoops in its
course, and knocking out pieces of the side trees and main
stick in several places, and escaped from the mast in the shape
of a fire ball, 7 feet above the deck, and was seen to go over
the starboard (leeward) netting right over the gun abreast of
the main mast rending the hammock cloth in several places,
carrying away one rattlin and stranding another; its exit
although fiery in appearance was harmless in effect, merely
injuring the cloth over a space of about a foot, and breaking
the two rattlins, when it was seen to strike the water a short
distance from the ship.

Effects of the Electric fluid in its course.—Knocked over-
board (at least they were never picked up or seen) the gilt
ball, copper spindle, and calico vane from the top of the vane
staff—split the vane staff—broke the copper binder round the
truck—broke a piece out of truck and slightly fused one of
the metal sheaves for signal halyards—cleared away the whole
of the main topgallant mast from the hounds of topgallant
rigging to the topmast cap, not leaving a fragment aloft.
Four men who had been sent aloft to unbend topgallant gear
and prepare for sending the yard down, were in the crosstrees
at the time ; John Rowe was struck dead as he was moving
from the weather to the lee side of the mast for shelter from
the rain, he was just on the aft side of the mast at the moment
and fell astride the after crosstree, where he was held by some
ropes falling round him—he never spoke. Thomas Hollings-
worth was standing on the after shroud of topgallant rigging
to leeward of the mast and holding on by the after crosstree—
he was so seriously injured as to be sent down in a chair and
died in 7 hours after.• Hugh Wilson was standing on the
foremost shroud of topgallant rigging, holding on by foremost
crosstree and close to Hollingsworth—he states that the
shock threw him forward and Hollingsworth aft, he was only
slightly hurt, and only two or three days in the doctor's list ;
the other man Charles Prynn was to windward, standing on
the crosstree. holding on by the foremost shroud of topgallant
rigging and received so slight a shock that he did not even
apply to the doctor. Wilson heard no thunder. The first

named two men had every stitch of clothes burnt from their bodies, excepting just the wrist bands and lower parts of the trousers which was left about the wrists and ancles. They presented a shocking spectacle, their bodies discoloured and hair singed from their persons. The next place is a large splinter out of the main topmast, from ten feet above down to the cap, setting fire to the gear about the topsail yard and then commences its destructive force about the main mast, first of all giving a severe shake to the starboard lower trussel tree ; it is hardly possible to give a description of its effects on the main mast, the mast should be seen fully to understand it, but some idea may be formed when it is stated that out of 28 large iron hoops 5 inches wide and half an inch thick between the deck and trussel trees, 13 were burst asunder and that for a space of 53 feet its ravaging effects can be traced the whole way, and the spot whence it made its final escape is several inches deep in the mast—on the starboard side a large piece of the mast is broken out (6 inches deep) from the third to the sixth hoop above the deck, and from the eighth to the ninth hoops. The cheek or side tree several feet of the lower part gone altogether and the other part nearly shook all to pieces. The larboard side—ekin piece gone from the sixth to the eleventh hoop and the mast burst out from the ninth to the eleventh, and from the thirteenth to the fifteenth, and the cheek very much shook. The hoops carried away were mostly the clasp hoops of side trees, but some of the body hoops were also burst asunder, and strange to say the awning hoop on which the main trysail mast steps and mizen stays reeve, lost one of its fore-locks, notwithstanding a piece of copper had been nailed over the clasp part—the forelock which was driven downwards was gone with a piece of copper and never seen, while the one which drives upwards was left in its place and held the hoop together. There were eighteen body hoops between the deck and trussel trees, and ten clasp hoops round side trees—four of the body hoops below side trees were broken, none of the hoops on the head of lower mast or on the heel of the topmast were touched.

Several men assert that balls of fire were running about the lower deck, and that they ran after them to throw them overboard; this seems strange, but if so, and it is hardly possible several could be deceived, it could be nothing more than flashes or rather sparks passing down the different hatchways after the explosion and less active than in the first descent, at all events it is certain there was a strong sulphurous smell below, particularly in the pump well, and sparks seen by many of the officers. It is remarkable that the

electric fluid seems to have jumped from metal to metal—first the copper spindle, then the copper funnel of topgallant rigging. and iron hoop round the mast to the head of the top-mast, from thence to the iron bound blocks and hoops on the top-sail yard to the main cap, and then to the lower trussel-trees, taking all the hoops downwards, passing over a gun into the sea.

The mast has since been taken to pieces at the naval yard at Malta, and its interior shows no defect, in fact, not the slightest injury appears about the mast, except what was exteriorly displayed; it is marked in some places, even on the spindle (centre piece) as if a train of powder had been flashed on it, but nothing more.

The outside crippled state of the mast led us to fear the worst and to secure it as effectually as possible for carrying sail.   This was done by getting the spare fore top-mast up and down. filling up with studding sail booms and clapping the iron fishes over the worst part, the main top-mast being lowered sufficiently to bring its wound below the cap.   All was well woulded together, and by degrees we felt confidence in the strength of the mast, so that at last we got a fore-top gallant mast, fitted on it and carried the main-sail, treble reefed topsail, topgallant sail and royal, working to windward for three or four days.

<div style="text-align:right">G. B. H.</div>

Magnetism and Electricity.—(Extract of a letter from Malta)—After the mainmast of H.M.S. *Rodney* was struck by lightning during her late passage from Athens to this place, the broken hoops surrounding it, were all found to be magnetised in the same uniformity of direction as if they had been operated on in one direction by the galvanic helix. Thus in a hoop broken in two athwart ships (speaking with reference to the ship's head) the larboard end of the foremost portion was a south and its starboard end a north pole : the end of the aftermost portion in contact with the south pole of the foremost portion being consequently a north pole, and the other end thereof a south, and so uniformly with all the other hoops at whatever part they were; similar poles in each hoop always pointing in similar directions in the circumference of the respective circles.

<div style="text-align:right">Nautical Mag.</div>

184.   There are five remarkable facts stated in the above account from the Rodney, which will be occasionally alluded to in this discussion : viz. The bursting of the metallic hoops of the main mast, copper binder of the truck, &c.—The fusion of the metallic sheave.—Wilson and Holingsworth being thrown in opposite directions.—The flashes or sparks seen

below deck:—and, The manner in which the broken hoops were magnetised.

185. The two following are cases in which ships, furnished with Mr. Harris's conductors, are supposed to be struck with lightning.

Extract from a Report on the Lightning Conductors of H.M.S. Beagle. 1831-6

" Previous to leaving England in 1831, the Beagle was fitted with the permanent lightning conductors invented by Mr. W. Snow Harris, F.R.S.

" During the five years occupied in her voyage she was frequently exposed to lightning, but never received the slightest damage, although supposed to have been struck on at least two occasions. At each of these times, at the instant of a vivid flash of lightning, accompanied by a crashing peal of thunder, a hissing sound was heard distinctly on the masts, and a strange, though very slightly tremulous, motion in the ship herself, indicated that something unusual had happened, &c.

(Signed)   " ROBERT FITZ-ROY,
" Late Captain of H.M.S Beagle."

Extract from a Copy of a Letter from Captain Turner, late of H.M.S. Dryad.

" H.M.S. Dryad, Sierra Leone, Feb. 13th, 1831.
" My Dear Sir,

" I write to inform you that we have had a trial of your conductors, and most excellent things they are. During our last cruise we had a great deal of lightning, but in one afternoon in particular, we had a tornado, which is always attended with a great deal of thunder and lightning; whilst standing on the quarter deck during one of the flashes, I distinctly saw the lightning run down the conductor on the foremast, and the officer of the forecastle came and told me he heard a whizzing noise resembling water boiling. All the men that were there heard it also; a short time afterwards several of the officers were standing abaft, and saw it during another flash go down the mizen mast with the same whizzing noise. It may be necessary to tell you that lightning on this coast appears to remain longer in the air than any other place I was ever at.

" I am &c. &c.
"WILLIAM TURNER."

186.   Mr. Harris observes, that, there is "a remarkable agreement" in these two cases, especially in "the hissing sound &c." On this phenomenon I shall remark, that the hissing sound proceeds from no other parts of a conductor than those which deliver the fluid into the air or other inferior conducting medium; for no such noise is ever produced by the fluid *entering* a metallic conductor. Hence it is obvious that if these hissing sounds proceeded from Mr. Harris's conductors, they were occasioned by the electric fluid rushing from the sharp edges of them, and from numberless asperities on the surface of the metal. To those accustomed to making electric-kite experiments during thunder storms, the hissing noise must be a familiar phenomenon. It proceeds from the broken parts of the metallic strand and other asperities of the string, which are discharging fluid from *upper* to *lower* strata of the air, or to the ground; and not from any flash of lightning striking either the kite or its string. Hence the hissing noise heard on board the Beagle and Dryad was no indication of either of the vessels receiving a flash of lightning at the time; but rather that their conductors, and other parts of the rigging, were discharging into the air about the *lower* part of the masts, a quantity of fluid communicated to their *upper* parts by a *wave* of the electric fluid, produced by a neighbouring flash of lightning. There is certainly something remarkable in the appearance of lightning running down the masts of the Dryad, which I shall endeavour to explain under another head (208, 209, 210, 211, 212, 213, 214); but whether they emanated from direct discharges of lightning on the masts or otherwise, they could be no very welcome visitors in the powder magazine.

187.   Mr. Harris has stated that "from about 100 cases, the particulars of which have been ascertained, it appears that about one half of the ships struck by lightning, are struck in the main mast: one quarter on the fore mast; one twentieth on the mizen mast and not more than one in a hundred on the bowsprit. About one ship in six is set on fire in some part of the masts, sails, or rigging. In these 100 cases there are destroyed or damaged 93 lower masts, principally line of battle ships and frigates, 83 top masts, and 60 topgallant masts."[*]

188.   By looking over the particulars of 174 cases,[†] which Mr. Harris has collected, I find only 44 in which the top-

* Harris's " state of the question relating to the protection of the British navy from lightning."
† In several of these cases the particulars of the damage is not specified.

gallant masts appear to have been injured : and as out of these 44 cases there are 13 in which the topgallant masts were lost, broken, or damaged, accidents probably occasioned by the mere falling of those masts when the others below them were struck, there would appear to be only about 31 cases out of the 174 in which the topgallant masts have been absolutely struck by the lightning. It is probable, indeed, that the proportion is even less than this ; because of these 31 cases there are 15 in which the topgallant masts were shivered only, a species of damage which, if occurring near to the heel of the masts, might easily arise from lightning striking the ship no higher than the topmast head. Lightning striking the topsail yard arm, when that sail is set, or the crosstrees at other times, would be very likely to damage the lower part of the topgallant mast.

189. An accurate statement of the *highest point* which lightning has struck each vessel, and the cases in which yard arms have been struck, would afford important data ; for showing the necessary height to which conductors ought to ascend in the rigging, and the comparatively uselessness of continuing them higher. Also, how far it would be advisable to protect the yard arms for the purpose of preventing lightning entering the vessel by their instrumentality.

Electrical phenomena similar to those produced by lightning.

190. The figure, dimensions, and character of the metal being determined on, the next most important consideration in the application of lightning conductors to shipping, are their most eligible situations, both as respects the working of the sails and avoiding the probable disasters by lightning entering the rigging and hull of the vessel ; and to determine which, it becomes necessary to study the effects of both the *direct* and the *lateral* discharges.

191. By the *direct* or *primitive* discharge, we know that men can be killed, inflammable bodies ignited, and metals even fused. Hence our first object should be to keep the *direct* discharge as clear of the ship as possible*; especially from the masts, deck, and hull; and as it is possible that the most spacious conductor that can conveniently be applied to a ship may be rendered sufficiently hot, by a flash of lightning, to ignite gunpowder,* we ought never to be induced, under

* Instance the fusion of the metal sheave of a block on board the Rodney (183, 184). Some few years ago the brig, Jane, from New York to Liverpool, had some part of her *chain conductor* fused by a flash of lightning ; the lower part of it fell overboard. It is said

any circumstances whatever, to lead a lightning conductor through, or near to, the powder magazine, at the risk of blowing up the ship.

192.   The *lateral discharges* are of three distinct kinds, which, for convenience of reference, I will denote by the *first, second,* and *third* kind.

193.   The first kind of lateral discharge, was, by Priestly, called the *lateral explosion.* It takes place at every interruption of a metallic circuit, or, wherever the electric fluid is exhibited in the shape of a spark or sudden flash of light in the common atmosphere or other inferior conducting medium.   By this kind of lateral discharge, the air is suddenly displaced: loose bodies are scattered and thrown from the axis of the circuit, and solid inferior conducting bodies are shattered or torn to pieces: *waves* of electricity are also produced in the neighbouring atmosphere and the bodies within it, by this kind of lateral discharge.   Hence the probability of ships' masts and other masses of wood being shivered, split, &c.; their hoops burst asunder, and other similar effects being produced from the same cause: even men may be killed by these lateral forces (183, 184).[*]

194.   The *second* kind of lateral discharge is a species of radiation, of the electric matter, from the surface of good conductors carrying a *direct* or primitive discharge.   It takes place most copiously from the edges of strips of metal, or from the surfaces of ragged or asperous wires; and to a greater extent in rare than in dense air.   Hence sharp edged strips of metal carrying a heavy flash of lightning would necessarily discharge a great quantity of fluid from both edges to the neighbouring objects or into the air.

195.   The *third* kind of lateral discharge is a displacement of the electric fluid natural to those bodies which are vicinal to a continuous conductor carrying the primitive discharge. The following experiment will serve as an illustration.

Experiment.   Let the Leyden jar *j* fig. 3, Plate IV, be discharged through the good conducting metallic rod *c c*,

that the lightning rod passing through the Nelson monument, at Edinburgh, became so hot, by a flash of lightning, that it could not be touched by the hand, by the first person who visited it afterwards.   Allowing only a few minutes to have elapsed between the flash and the person entering the monument, the probability would be that the conductor had been rendered red-hot by the lightning.

[*] The *principal* discharge obviously passed through and over the surface of Hollingsworth's body; whilst Wilson was probably hurt, and thrown in the opposite direction, by the lateral explosion.

which stands on the same metallic plate as the jar is placed on.' At the same moment a spark will appear at the opening *o*, between the insulated metallic body B and the conductor *c c*. This is a lateral discharge of the *third kind*.

196. If, instead of the metallic body B, the knuckle were to be presented to the conducting rod *c c*, a pungent spark would be felt at every discharge of the jar through the circuit represented by the figure.

197. If, instead of discharging the jar through the conductor *c c*, it were to be discharged by a direct application of the discharging rod to the coating or metallic plate on which the jar stands, as represented by fig. 4; the lateral discharge would still occur at *o* between the body B and the conducting rod *c c*.

198. By this kind of lateral discharge a dense spark may be produced when the bodies B and *c c*, are half an inch asunder, though the jar employed be only of the capacity of a quart. Chemical decompositions are easily performed by this lateral discharge, even with the small jar I have just mentioned, and every other class of electric phenomena may be exhibited by it, when the apparatus are sufficiently large, as decidedly as by the primitive discharge itself.

199. The effect is much increased by connecting the body B with the ground; and diminished, to a certain extent, by connecting the outside of the jar with the ground. I have produced the spark between *c c* and the body B, when placed 50 feet from the *direct* discharge. The arrangement is shown in fig. 5, where *c c* is connected with the outside of the jar by a copper wire 50 feet in length.

200. In fig. 6. *c c* may represent the conductor of a ship's mast, connected with the iron knee *k* by means of the copper strap *s*, as proposed by Mr. Harris. Then a discharge from the jar *j* through the conductor *c c* would imitate a flash of lightning striking a similar conductor in a mast; and it will be found that this arrangement is as productive of the lateral discharge at *o* between the body B and the iron knee *k*, as any other arrangement whatever; and as similar lateral discharges would take place from any part of the copper strap *s*, or from any metallic appendage or branch of the conductor, it is obvious that men who happened to be near to any of these conductors, straps, knees, &c. would experience all the effects of these lateral discharges.

201. When the body B is connected with the conductor *c c*, by a wire *w w*, 20 feet long, as represented in fig. 7, only a very short lateral spark is obtained at *o* between the conductor and body B; but as the wire is lengthened the lateral spark becomes longer and denser. I find, also, that the

lateral spark is increased by an increase in the quantity of metal between the body B and the jar, without lengthening the conducting distance.

202. I have united, by metal, the outside of the jar *j* and conductor *c c* with a metallic pump, which descends into a deep well copiously supplied with water, in order that the conductor *c c* might be as uninsulated as I could possibly make it; still, however, the lateral spark appeared between the conducting rod *c c* and the body B, whether the latter was metallically united with the same pump, or with a kitchen range at some distance from it. I have made a number of experiments of this kind, and have varied the arrangement in many ways, and from the results which have been obtained, I am fully of opinion that this kind of lateral discharge will always take place when the vicinal bodies are sufficiently capacious, and near to the principal conductor which carries the primitive discharge, or to any of its metallic appendages.

203. Such, in fact, appears to be the case when only the small jar, already mentioned, is used: and as the extent of electro-displacement, in vicinal bodies, depends upon the magnitude and intensity of the primitive discharge or *main stroke*;* the electro-displacement and consequent lateral discharge occasioned by a heavy flash of lightning striking a conductor which passes through the body of a ship, must necessarily be exceedingly dangerous ; for although this kind of lateral discharge is never so powerful as the primitive discharge, nor so great amongst inferior conductors as amongst those which conduct freely; the *magnitude* and *intensity* of a flash of lightning being infinitely greater than any thing which can be produced artificially, even by the most stupendous apparatus that can be employed; the effects of lateral discharges from lightning would also be proportionally greater than any ever produced, or possible to produce, experimentally: and their consequences always to be dreaded.   The "balls of fire" seen "running about the deck" of the Rodney, with the flashes and sparks (183, 184), were more likely to be a series of lateral discharges of this kind, or by *waves* from the first kind (193), amongst the articles below, than any part of the lightning which struck the ship.   Electrical waves produced by a discharge through a conductor situated close to the powder magazine would produce intense sparks amongst the powder barrels, whose metallic linings and hoops would reciprocally exchange them.

* *Main stroke* is the name given to this phenomenon, by Viscount Mahon, who studied this kind of lateral discharge very extensively.

A comparison of the observed effects of Lightning, and the probable effects which Lightning would produce by the application of Mr. Harris's system of conductors to shipping.

204. Were there no other data than those afforded by the *fusion* of the metallic sheave, belonging to the Rodney (183, 184) : and the fusion of the chain conductor belonging to the brig Jane (191 note), we should have ample demonstration of the super-eminent calorific powers which lightning exercises on metallic bodies of considerable dimensions : and of the probable effects producible on masses of still greater magnitude, by the same agency. These specimens of electric action when exhibited on the grand scale of nature, and which mock every human effort at competition, ought to be regarded as monitors of inestimable value, prominently and opportunely placed before us, as if commissioned by an all-merciful Providence, to warn us of the imminent danger, or certain destruction, to which thousands of mortals may be exposed by the misplacing of lightning conductors in ships containing combustible materials. Let us, then, avail ourselves of these well authenticated and most important facts, and endeavour to profit by the inestimable examples which they afford for our instruction and guidance. The impressions which these facts convey to the mind are too forcible and too definite to be easily misunderstood : they clearly imply that either of the discharges of lightning which struck the Rodney, or the Jane, would have been powerful enough to have rendered even the thickest part of Mr. Harris's conductors *sufficiently hot to ignite gunpowder ;* and that a similar discharge of lightning striking the mizen mast conductor, should it pass through the powder magazine, would probably be the means of the ship being blown to atoms. Hence it becomes obvious, that the calorific effects alone, by a *direct* discharge of lightning, would be sufficient to put in the most imminent jeopardy every vessel which is furnished with conductors that pass through, or near to, the powder magazine.

205. It appears from Mr. Harris's list of cases (187), that the lower masts are more frequently injured than the top-masts, and the top-masts more frequently than the top-gallant masts : hence, although the Rodney and some other ships have been struck *above* the top-mast, it is obvious that lightning more frequently strikes the rigging below the top-mast-head than above it ; and by taking into account the damage done by the mere falling of the top-gallant mast, as a consequence of the masts below it being struck and injured, it is highly probable that the cases in which lightning strikes

the spindle at, or above, the top-gallant mast head, bear a very small proportion to the cases in which lightning strikes the sides of the masts and yard-arms. From this inference, it will be obvious that an *oblique* flash of lightning striking the body of the rigging, could not arrive at a conductor, let into the wood on the *after-side* of a mast, without damaging the mast itself; unless it came from a cloud astern the vessel. Were the lightning to strike any of the yard-arms in order to arrive at a conductor, that yard-arm would receive as much damage as if no conductor had been there: and it is even possible that the conductor would be the means of increasing the damage, by causing the lightning to run along the whole length of the yard-arm to the mast; and the mast itself might then be traversed by the lightning and shattered between the yard and the conductor. The sails, ropes, spars, and every article which the lightning traversed on its way to a mast, would receive precisely the same extent of damage as if no conductor were attached to it. And men placed in, or near, the track of the lightning would be as sure to receive a death-blow as under any other circumstance in which lightning entered the rigging. Moreover, as these *central* conductors would offer increased facilities for lightning to strike the masts, all the evils usually attending oblique discharges through the rigging to them, would necessarily be increased in like proportion.

206.   To whatever species of force we may be disposed to allude, the bursting of the massy iron hoops on the main-mast of the Rodney (183), we are compelled to infer that a similar force operating on a mast with its conductor, would be productive of similar effects, to a certain extent. If we are to suppose that the hoops were destroyed by the force of a lateral explosion of the first kind (193), then a similar lateral explosion between the mast and its conductor, would shatter both of them about the place where the lightning struck the mast: and it is not overstepping the boundaries of electrical evidence to infer, that the *closeness* of the conductor to the mast, would be one circumstance at least, conducive to the destruction of the latter. The iron hoops on the main-mast of the Rodney were close jambed to the wood, and situated under the best circumstances to be split open from a sudden expansive force within. Upon the same principle a conducting strip of copper, close jambed to the wood within a groove in the mast, might probably, not only be burst asunder, but peeled from the wood for many feet upwards and downwards, from the point where the lightning struck the mast. No circumstance could better substantiate the veracity of this inference than

the following, which is so clearly described in the accounts from the Rodney. " The copper binder round the truck was burst asunder, a small piece broken out of the truck, and one of the metal sheaves for signal halyards slightly fused." In this case the fused sheave was *within* the boundaries of the " copper binder," and as it is highly probable that the lightning was divided between the two masses of metal, in this part of its course, the force of the lateral explosion occasioned by that portion only, which passed through the sheave, was sufficient to burst asunder the exterior copper binder. If it should be discovered that the copper binder of the truck was sufficiently heated to render it brittle, such circumstance would certainly facilitate the rupture of the metal; but by no means invalidate the inference I have drawn respecting the effects of the lateral force : whilst at the same time it would be an additional proof of the astonishing calorific effects which lightning is capable of producing on the best metallic conductors. The manner in which the fragments of the iron hoops were magnetized, if we consider the lightning to have *descended*, would prove that it traversed the mast *within* the hoops, and its lateral force the cause of their being torn asunder (183, 184).

207. The electro-magnetic effects occasioned by a flash of lightning traversing conductors which pass through the hull of the vessel would be exceedingly injurious to the chronometers on board; by magnetizing every piece of ferruginous matter which enters into their construction ; and thus deranging their performance to an irremediable extent, whilst those pieces remained in the instruments. Not only would the *principal* conductor in the mast, be productive of these effects, but every strap, knee, and other metallic appendage to that conductor, would communicate *permanent* magnetism to all those morsels of steel which form so essential a part to those valuable horological instruments. The chronometers on board the Beagle, surveying ship, to whose safety Mr. Harris ascribes the instrumentality of his conductors ;* are,

* "The report from the Beagle shows that the conductors had performed their office and defended the ship; this, it must be allowed, was of the utmost, perhaps vital, importance to the survey on which she was employed ; since, if the vessel *had been struck* by lightning, it is more than probable that the many valuable chronometers, compasses, and philosophical instruments of various kinds, necessary to the survey, would have been seriously damaged : the cabin set apart for the charts not being far from the main mast." *Harris's State of the Question &c. Section E. p.* 7.

The above paragraph would imply that the Beagle was *not* struck by lightning : but as Mr. Harris has elsewhere given a different

by the circumstance of their not being injured, almost certain evidence that the ship was *not* struck by lightning at those times when the "hissing sound was heard on the masts" (185). Had a discharge of lightning traversed the main-mast conductor, every chronometer in the cabin would have suffered from its electro-magnetic influence ; and no plan or invention whatever, excepting such as would distribute the electric influence on every side alike, could possibly prevent chronometers from being subject to these magnetic effects.

208.   Mr. Harris is of opinion that the light which appeared to run down the masts of the Dryad (185, 186), was a decisive evidence of the ship being "struck by lightning in the com-mon way, but without the ordinary ill consequences :" and says, "the electrical agency seems to have fallen so powerfully on the masts, that it produced about them a luminous at-mosphere ; a phenomenon not uncommon when large quanti-ties of electricity traverse conducting bodies, in a heated or rare state of the air, such as that usually found on the coast of Africa."*

209.   It is true that rarefied air, whether hot or cold, will facilitate the production of a luminous atmosphere around a conductor carrying an electric discharge; but this is not the case with *unattenuated* air, though of a high temperature : but on the contrary, heat offers a decided resistance to the display of the phenomenon and confines it within narrower limits than it would otherwise expand to and occupy. Unless, therefore, it can be shown that the barometeric column was exceedingly low indeed at the time of the occurrence, Mr. Harris's explanation is by no means applicable to the case. Neither would a luminous atmosphere, such as Mr. Harris calculates on, have any resemblance to the phenomenon described by Captain Turner : for instead of its appearing like lightning running down the mast, the phenomenon would have been a momentary glowing column of electric light. We cannot produce anything like a *running light* when the conductors are sufficiently good and capacious to conceal the motion of the fluid, though such a phenomenon may easily be produced by the employment of inferior conductors.

210.   I have seen a globe of electrical light traverse the surface of a wet silken cord, upwards of three feet long.   One

opinion (186), it is possible that this passage was intended to state that those instruments would have been damaged "if the vessel had been struck by lightning " *and not furnished with his conductors* at the time.

   * "State of the Question relating to the protection of the British Navy with Lightning Conductors."

end of this cord was tied to the lower end of a kite string, in which a strand of copper wire was laid, and the other end was tied to a young tree. Some non-commissioned officers of the Royal Artillery, who were looking on, thought that the electrical globe was about the size of a musket ball. The lightning was very heavy at the time; but neither the kite, which was only about fifty yards high, nor the string, were ever struck by it. The hissing noise in the string could be heard at the distance of a hundred yards.

211. On one occasion, at Maidstone, a continuous stream of dense sparks traversed the same silken cord when quite dry, for several successive minutes, whilst a cloud passed over the kite. The string was cut, or burnt, at about three hundred yards from the ground, and the kite lost: but no lightning was seen, nor thunder heard, at the time. More than fifty persons witnessed this fact. We heard thunder at a distance about some ten minutes after the cloud had passed over the field where we were experimenting.

212. At Kirby Lonsdale, in the spring of 1834, a man named William Croft, was severely struck by a discharge from my kite string, when no lightning was present. The electric fluid discharged itself over a four-foot long stick, which Croft held in his hand, pointing the farthest end to the string. A hail shower was falling at the time. Several hail showers fell the same day from well defined insulated clouds, leaving the sky quite clear after the transit of each. Long dense sparks could always be had at the kite string on the approach of a cloud, and during its transit over the kite: but only very feeble electrical indications could be obtained when the sky was clear for some distance about the kite.

213. Under similar circumstances, at Addiscombe, I have had rapid *spontaneous* discharges from a two-gallon jar, whose inside was connected with the kite string.

214. I once received a severe blow from a kite string, when no visible cloud was within a mile of the kite, although the string, containing a wire its whole length, was uninsulated (tied to a tree) at the time, and at the distance of three feet from the place where I stood. The discharge took place over a dry silken ribband with which I was attempting to lower the kite. I had received several shocks before this heavy blow which was so severe on my chest, thigh, and shin bones, that I was deterred from taking down the kite, until a cloud, which I observed to windward, had passed over to a great distance on the leeward side. It was a thin ragged cloud, and did not discharge any lightning. This fact occurred in the Royal Artillery Barrack field, at Woolwich.

215.   I have been particular in describing these facts, because they show that those phenomena which Mr. Harris considers as sure indications of his conductors being struck by lightning on board the Beagle, and Dryad, are more likely to have been the mere consequences of *electrical waves* produced by clouds, or neighbouring lightning.   Lightning invariably causes electrical waves, but is not absolutely necessary to their production, nor to the production of the phenomena in question : and as Mr. Harris has not taken any such facts into consideration, they will appear more important in this discussion : because if he has not been aware of them before, they may now possibly have a tendency to give him very different views to those which he has hitherto taken respecting the *cause* of the phenomena which were observed on board the above named two vessels.

216.   It is highly honourable to Mr. Harris, however, that he has, in the most liberal and open manner, given a fair and ample invitation to investigate " the state of the question relating to the protection of the British Navy from lightning" by that system of conductors which he has proposed.*   At the present crisis, a more important scientific topic could not well be imagined : nor can there be one which has a greater claim upon the serious consideration of every experienced electrician.   I have been induced to respond to Mr. Harris's solicitation in the hope that, from my humble example, others, more competent than myself, and with more ample means at their disposal, will be induced to take up the subject.   I hope, however, that I have succeeded in pointing out some important truths which, previously, were either not known, or unaccountably neglected : and that I have been enabled to place the true character of Mr. Harris's plan of conductors, and the experiments intended for the illustration of their superiority over others, in a much clearer point of view than any in which they have hitherto appeared,

217.   Having no motive, beyond that of the public good, for introducing his conductors to the Navy in preference to other systems, Mr. Harris will feel a gratification at his solicitations to enquiry being thus promptly and diligently attended to : and that this great question has been fairly and impartially discussed ; that mere speculation has been stu-

* " It is therefore further submitted, that Mr. Harris may justly claim to have these circumstances fairly considered.   In seeking for an enquiry, he aims at nothing which may not come openly and fairly before the country, without any kind of reservation whatever." *State of the question, &c.*"

diously avoided; and that no inferences have been drawn but such as either rest on incontrovertible data, or have ample probability in their favour.

Description of a new system of Lightning Conductors for Shipping.

218. By the preceding investigation we are enabled to understand that narrow strips of copper let into the masts afford but a very partial protection to a great part of the rigging, and are liable to serious injury from oblique flashes of lightning striking the masts (205, 206); and that, in order to prevent all hazard of leading destruction into the very vitals of the ship, it is more prudent to conduct the lightning entirely away from the interior of the hull than directly through it: and as clear from the plane of the masts and as exterior to the principal parts of the rigging as circumstances will allow. Hence, therefore, had the frequent hauling up of chain or other flexible conductors to the masts' heads not been a serious inconvenience, those metallic chains, or ropes, if sufficiently numerous, would probably have been as secure a protection to vessels as any that could easily be devised: for instead of permitting an oblique discharge of lightning to traverse a portion of the rigging with the probability of splintering a yard arm, or a mast before it arrived at them, they would, by their *exterior* position, envelop within their boundaries, not only the masts but the principal rigging of the vessel, and shroud them from the effects of the discharge: and no plan of protection to the masts, from a *perpendicular* discharge of lightning, could be more perfect than a system of continuous conductors hanging from the masts' heads, and passing over the sides of the vessel into the sea. As, however, the hauling up of such systems of conductors, and the liability of their being an obstruction to the working of the sails, &c., are strongly urged as objections to their general use: and that *fixed* conductors are absolutely desirable, not only to prevent occupying the time of the men when wanted for other duties of the ship, but also to be in constant readiness to parry the effects of sudden or unexpected electrical storms, we should endeavour to employ all such means as are available to accomplish this great object: and as simplicity and security ought to be the leading features of every plan, and economy always kept in view, I am not aware of any plan so likely to meet the demands of the circumstances to so great an extent as the simple system of conductors which I now propose to describe.

219.   Nothing can appear more simple than to attach fixed conductors to the lower rigging, because the lower masts are always standing under every circumstance of weather.   I therefore propose to protect the lower masts and rigging by cylindrical rods of copper; four to each mast, and situated exterior to the shrouds, having one *before* each fore-shroud, and one *aft* each after-shroud.   The upper extremities of these conductors to be attached to the fore, main, and mizen tops, as distant from the masts as circumstances will allow : and in any manner most secure and convenient.   The lower ends of these copper rods to be fixed to the chains on the outside of the fore and aft shroud of each mast : and continued, by broad and stout straps of copper, to the copper sheathing of the vessel.   By these means both the *starboard* and *larboard* shrouds of every mast would each be flanked by two conductors, which would be always at their posts ready to receive any flash of lightning tending towards the masts, from whichever side of the ship it might approach.   As these conductors might be made to extend to some distance *fore* and *aft* of their respective shrouds, without obstructing the working of the lower sails, they would form an alinement on both sides of the vessel so as to protect each other's masts as well as their own.   To prevent lightning from entering the lower rigging from ahead, I propose a conductor on each side of the fore-stay, their upper ends to be united with the conductors of the foremast, and their lower ends with the sea in the most convenient way.   The whole of these conductors would be as permanent, and as unobstructive to the working of the vessel, as any part of the standing rigging ; and, with the exception of the after part, would so completely enshield the lower rigging, as to parry any flash of lightning tending towards it. The lower yard arms would however still be unprotected though not so liable to receive injury from without, as by Mr. Harris's system ; because, in every position, they would be near to some of the conductors, which would relieve them of any discharge they might receive before it arrived at the mast.   Yard arms would be very easily protected by flexible conductors uniting them with those which are permanently fixed.   By such a simple system of conductors the whole of the lower rigging would be as completely protected from lightning as any system of *fixed* conductors are ever likely to protect it : and certainly without the slightest chance of injury to the lower masts or to the hull of the vessel.   For though there were no conductors in the upper rigging, and that lightning were to strike an upper mast, it would do no injury below the lower mast head ; as it would there enter the system of con-

ductors, which would convey it over the edges of the vessel to
the sea.

220. The topmasts and rigging are easily protected by a
system of conductors similar to those already described for
the lower rigging, and may remain permanently fixed as long
as the topmasts are standing. They may consist either of
inflexible rods or of flexible metallic ropes of sufficient dimen-
sions, four to each topmast. The lower extremities of these
conductors are to be united with the upper extremities of the
lower ones; and their upper extremities to the cross-trees.
The upper and lower systems of conductors may be so con-
tinuously united, by the former sliding on the latter, as to
present no inconvenience whatever, even under circumstances
necessary to strike or to remove the topmast. By these sys-
tems of conductors every part of the ship, below the topmasts'
heads, would be well guarded; and without the slightest risk
of danger from the presence of the conductor; and as light-
ning but seldom strikes the rigging higher than the cross-
trees, it would not be of much consequence to carry the con-
ductors higher up. Oblique flashes of lightning which might
otherwise strike the top gallant mast, would be directed to the
topmast head when furnished with these conductors.

221. As, however, every chance of danger to the men,
and every species of damage to the vessel ought strictly to
be avoided as far as practicability will admit, it still appears
desirable to furnish the top-gallant rigging with conductors :
and perhaps those which would give the least trouble to the
men, would be strips of copper let into groves in the masts,
according to the plan proposed by Mr. Harris; but instead
of *one* strip only to each mast, I should propose *three* in each,
at equal distances from each other ; which, by having an ex-
posure of metal on every side, would be a greater security to
the mast than by its having one strip only. These strips of
copper need only be narrow, and of moderate thickness ; as
they would be made to act in concert by uniting them at, or
near to, the truck, with a copper band. A similar band
would unite them at the lower end of the mast. Precautions
similar to those so admirably contrived* by Mr. Harris, for
maintaining metallic contact between the top-gallant conduc-
tors and the system next below, must, necessarily, be attended
to. The strips of copper should be continued to the vane
spindle.

222. Four cylindrical copper rods, or four flexible metallic
ropes, stretched from the cross-trees to the truck, parallel to
the top-gallant shrouds, would afford a much better protection
to the top-gallant rigging, than any conductors let into the

mast.   Flexible conductors would be as easily set up, or taken down, as the shrouds themselves.

223.   The bowsprit would be very well protected by the conductors accompanying the fore stay (219), they being continued on each side near its lower surface, to the sheathing at the bows ; and the jib-boom would be protected by three copper ropes, or chains, reaching from its extremity to the conductors of the bowsprit.   The jib-boom, however, being so little liable to be struck by lightning, presents no very strong claims to attention.

224.   Let fig. 8, Plate IV, represent a transverse section of a vessel through the plane of a mast.   Then the lines c c, c c, will represent the conductors on the *after-side* of lower shrouds ; and the inner lines c' c' and c' c' the conductors on the *fore-side* of the lower shrouds.   The lines t t and t t will represent the conductors on the *after-side* of the topmast shrouds ; and the lines t' t' and t' t' will represent the conductors on the *fore-side* of the topmast shrouds.   The conductors of the top-gallant masts are too easily understood to need further illustration.

225.   Having thus described a system of marine conductors which, to me at least, appear more efficient and less objectionable than any other hitherto offered to public notice ; the next consideration would be the expense of equipment : which I will compare with the expense of equipment by Mr. Harris's plan.   The following official document, which is copied from Mr. Harris's pamphlet, is highly valuable on this point ; coming from the best authority it furnishes unexceptionable data.

226.   *Documents relating to expense.*

The following are extracts from a report by a gentleman who was sent from his majesty's dockyard at Chatham, by order of the commissioners of the navy, to estimate and inspect the work ; and who, being a naval engineer, and otherwise a person of great intelligence, was considered by them equal to the task.

" His Majesty's Dockyard, Chatham, July 9, 1834."

" Sir,—In obedience to instructions received from the principal officers of this yard, in conformity with your official communication of the 1st instant, I herewith enclose a statement (in a condensed form) of what would be the expense, in labour and materials, of applying Mr. Harris's lightning conductors to each class of ships, supposing them fitted at the most eligible time.   I have likewise ventured to append a few cursory remarks, which seemed necessary in order to afford a criterion of what may probably be the *ultimate expense* to

the public, in the event of the plan being generally adopted in the service. With this view, I beg further to transmit schemes of prices for labour, for different artificers employed on the work, upon which it should be added the accompanying estimate has been made."

<div align="right">" I have the honour to be, &c., &c.,</div>

<div align="right">W. M. RICE."</div>

" To G. J. Smith, Esq., Secretary of H.M. Navy, &c."

Mr. Rice then proceeds to give a complete table of particulars in the fitting each part of a ship, according to the plan above mentioned, the whole of which it is not necessary to detail here. It will be sufficient to state the general results as they appear in the last columns of his table.

<div align="center">TABLE 5.</div>

Class of Ships.	Total of Masts and Hull.				
	Labour, &c			Reconvertible Copper Materials	
	£.	s.	d.	£.	s.	d.
120 guns	60	18	0	305	12	2
84 ..	56	18	0	292	19	11
74 ..	54	17	0	263	2	3
50 ..	50	16	0	235	17	3
46 ..	45	15	0	190	16	4
28 ..	38	14	0	123	13	3
18 ..	29	12	6	89	14	8
10 ..	24	10	0	77	16	9

83.   Upon this report Mr Rice offers the following remarks:—

" This estimate is grounded on the supposition that Mr. Harris's plan be applied to ships at the most eligible time, viz : during the progress of building or repairing, when the essential or original fastenings of the hull may be made to subserve as conductors: much delay will be thus prevented, and many contingent expenses saved."

"As the prompt execution of work much depends upon the facilities afforded to the workmen, I beg, without further recapitulation, to refer particularly to the arrangements proposed in the letters and drawings which I had the honour, on a former occasion, to submit for the consideration of the commissioners of his majesty's navy ; humbly conceiving that they will greatly assist the desirable object contemplated by Mr. Harris, inasmuch, as by adopting them, the practical process will be greatly simplified, and the general introduction of the plan much facilitated."

" It may be proper to make a few observations on the subject of expense.   Referring to the accompanying condensed statement, the last column *but one* denotes the *money absolutely sunk* upon the first ship of each class fitted with lightning conductors, and the *last* column, the outlay upon *reconvertible materials*, which must be

viewed as other articles of a ship's furniture for the protection of lives and property, such as *life-buoys*, &c. &c., with due allowance for wear and tear.   In taking, therefore, a prospective view of the expense, very considerable abatement will be made upon the first cost by return of copper into store, which it is presumed will have suffered very little deterioration after many years' service; more-over, the straps over the heads and heels of masts, nuts, and screws, vanes, and vane-spindles, and the branch conductors under the beams, when removed from any ship, may all be placed in store, to be again replaced in other ships of the same class; the plates taken out of masts may probably be made serviceable by redressing, or, at most, will only suffer a loss at the rate of one penny per pound for remanufacturing : the plates removed from ships' hulls may certainly serve for many ships successively. Hence, should the plan be generally adopted, the amount of labour will be much re-duced by the appropriation of serviceable articles to several ships, so that the loss alone upon the material *must not be charged* again upon a second or third ship."

"The absolute expense to the public is really the aggregate of the amount of labour, &c., as money sunk, with interest, together with the interest upon the first cost for copper for a given time. It would be difficult to assign any scale of per centage for the pro-tection of ships by lightning conductors; but, in order to form some notion, within certain limitations, let the value of a first-rate, when fully equipped, be assumed in round numbers as worth £110.000, the value of property simply considered, neglecting the consideration of lives, and the dangers and disasters arising from a disabled ship, and taking the average of five years' run only upon the principal and interest, the annual premium would be less than £30., making the per centage under *sixpence halfpenny.*"

"If we extend the term of years, the aliquot part of the money sunk will of course be less, and therefore the per centage will be reduced in like proportion ; but, if this view of the expense be carried forward to a second, or a third ship, the *money sunk* will be very considerably less, on account of the diminution of labour, &c., whilst the term of years is still further increased ; and *therefore the premium or per centage, would be ultimately trifling.*"

<div align="right">"(Signed) W. M. RICE."</div>

"Chatham Yard, July 9th 1831."

227.   Mr. Harris' has given the following dimensions of his conductors for "one mast of a frigate of fifty guns."[*]

	Length.	Mean Width.	Cubic Inches.
On the Royal Pole	18ft. 3in.	2in.	82
On the Top-gallant Mast.	17 0	2·5	95
On the Topmast	50 0	4	450
On the Lower Mast . . .	93 0	6	1255

<div align="right">Total copper 1882</div>

[*] Nautical Magazine for November 1837.

228. By taking the specific gravity of copper at 9000; the weight of 1882 cubic inches will be 613lb nearly ; which, at one shilling per pound (and it may be had at a lower price), would cost £30 13s. This average for each mast, would give £92. nearly : the cost for copper on the the three masts of a fifty-gun frigate.

By referring to Mr. Rice's estimate for a similar fifty-gun ship, we find that the copper alone, costs nearly £236. Then 236—92=£144, is the cost of the appendages, mostly below deck, to Mr. Harris's conductors.

There is still, however, another consideration in these estimates. Mr. Harris's conductors proceed to the bottoms of the masts, and are the broadest below deck ; therefore the quantity of copper *above deck*, in each mast, is considerably less than 1882 cubic inches : and its cost proportionably less than £30. 13s.

229. Now the quantity of copper on the lower mast is about two-thirds of the whole (227) ; and by allowing about one-third of that on the lower masts to be below deck, we have $(2 \times 92) \div 9 = £20$. 9s. for the cost of the copper in the masts below deck. Rejecting the nine shillings, we have 92—20=£72. for the cost of that portion of the copper conductors which are above deck.

230. Now this seventy-two pounds-worth of copper (229) is, of course, supposed by Mr. Harris, to conduct lightning safely to the deck. Then, as my system of conductors, on each mast, would act in concert as decidedly as if in one piece of metal ; they, with the *same quantity* of copper, would stand the same chance of being efficient to the *chains* as Mr. Harris's to the deck : and consequently, the cost of the metal for my conductors, would be £72. with the additional cost of about £15. for the fore-stay conductors, and fastenings to the whole.

231. The quantity of copper above deck in Mr. Harris's system, does not appear to me to be sufficient, requiring at least one-half more. Hence the cost of the copper for the equipment of a 50-gun frigate, with my system of conductors, would be £130., calculating it at the rate of one shilling per pound, which is, perhaps, about one sixth too much ; and consequently, the material for Mr. Harris's system, would cost more than double the sum required for mine.

232. The next thing to be considered is the expense of workmanship ; which, for my system, could not possibly exceed £20.: which, being deducted from Mr. Rice's estimate of £50. 16s., would leave a saving of £30. 16s. But Mr. Rice's estimate is "grounded on the supposition that Mr.

Harris's plan be applied to ships at the most eligible time, viz., during the progress of building or repairing &c." (226); and, consequently, applicable under those circumstances only. Under all other circumstances, the expense would be enormous: the ship having to be completely dismantied above deck, and the lower masts taken out and removed from her. It appears therefore, from the whole, that under the best circumstances in which Mr. Harris's conductors could be applied to a 50-gun frigate the expense would be £286. 13s.   To equip a similar vessel with the system which I have proposed would never exceed £150., reckoning the copper at one shilling per pound.   At tenpence per pound the expense would be £128.   Taking the mean of these prices, the expense of the two plans would stand as below.

$$\begin{array}{ll} \text{Harris's system} \ldots\ldots & \text{£286.} \\ \text{Sturgeon's ditto} \ldots\ldots & \text{£139.} \\ \hline \text{Difference} \ldots & \text{£147.} \end{array}$$

For a 120-gun ship the difference
would be nearly .... £200.

233.   The *time* necessary to equip any ship with Mr. Harris's system of conductors, would be ten times that necessary to equip a similar ship with the conductors which I have proposed. Moreover, the latter system could be fitted to any ship, on any station or on any service, either at sea or in harbour. I do not, however, advocate the cause of economy as a primary object; as the only gratification which I can experience must emanate from a consciousness of having made a proper application of the principles of science to the protection and welfare of my fellow men.

Westmoreland Cottage. Pomeroy St.,
     Old Kent Road, London.   Sept. 8th, 1839.

P.S.   The readers of the Annals will perceive that this is not the precise subject intended to form the fourth memoir. The subjects are similar, and this being the more important one I have given it the precedency.   W. S.

Westmorland Cottage, Pomeroy Street,
Old Kent Road, London, Sept. 12th, 1839.
     My dear Sir,
     You are well aware that the subject of electricity has for a long series of years, occupied a great deal of my attention: and *marine lightning conductors* have been amongst my most careful studies,

though I have not published any of my ideas respecting them till very lately. The now apparent intention of introducing your system of conductors into the navy has induced me, however, to arrange the results of my investigations on this topic in a somewhat regular order, and as I was aware of your being at the Birmingham Meeting of the British Association, I was desirous of bringing my views to light under circumstances which would give you the best possible opportunity of correcting any errors which, by possibility, I might have happened to have fallen into. My paper would not reach Birmingham till Thursday evening, the 29th ult. but as you were on the committee, I have no doubt of its being immediately placed in your hands, and in sufficient time to peruse it, before it would be read.

You will have perceived that the results of my investigation are not favourable to your plan: they point to several objections which you do not seem to have been aware of, which, as they are plainly described in the memoir, do not require to be particularized in this place. If I have fallen into any error respecting the probable effects of lightning through the instrumentality of your conductors, it is in consequence of the supposition of the mizen conductor passing through the powder magazine. Although I have great authority on that point: yet as you have ample means of avoiding so dangerous a situation, I would not urge that objection; though I cannot find any data to conduct my reasonings to any other conclusion than that, *danger is always to be apprehended from lightning being led into the body of the ship*, not only from the calorific effects of the *main discharge* alone, but from the effects of the lateral discharges, which your system of conductors does not provide against.

Truth, and the welfare of our fellow men, being the only motives which either you or I can have in this undertaking, I have been particularly desirous that the results of my enquiries should appear in the most prominent and simple form; and I think you will acknowledge with pleasure that the candid and open manner in which I have freely discussed every point, can lead no one into error respecting the inferences I have drawn.

The subject is of such paramount importance to every maritime country, and requires such general attention and rigorous investigation, that I mean to submit the substance of my memoir to the consideration of the principal scientific bodies in Europe and America, in order that the subject may be fully sifted and clearly and satisfactorily explained by the ablest electricians which the world can at this time produce. I shall publish it entire in the October number of the "Annals of Electricity &c." and shall feel much pleasure in publishing any remarks you may wish to accompany it, either in favour of your own conductors, or any you wish to make on that system which I have proposed.

I am, my dear Sir,
Yours very truly,
W. STURGEON.

To W. Snow Harris, Esq.
&c. &c. &c.

XVII. *Memoir on the employment of the oxy-hydrogen gas in its applications to the purposes of Chemistry and the Arts, by means of the Silex-safety Blowpipe, an instrument adapted to the perfectly secure combustion of explosive gaseous mixtures in any continuous quantity, and with jets of extraordinary dimensions.*   By W. H. Weekes, Esq.,[*] *Surgeon, Lecturer on Philosophical and Operative Chemistry, &c. &c. &c.*

Fertile in expedient and powerful in agency as the combined efforts of genius have contributed to render modern chemistry, it may, perhaps, be fairly doubted, when we except the mighty voltaic arrangements which enabled our celebrated Davy to decompose the alkalies and elucidate the basis of the earths, if, in the appliances of this almost universal science, we can rank one superior, or even equal in its general operation, to that which uniformly obtains from the use of the oxyhydrogen blowpipe, when its operations are guaranteed by safety, and its management is skilfully directed.   Our present purpose is not designed to trace the history and progress of this astonishing instrument, which, in the hands of our chemists of the nineteenth century, and with the several improvements it has gradually received, exhibits such small comparison with its ancient form of construction and degree of effect, as scarcely to be recognized as belonging to the same class of philosophical apparatus.   The daring essays, however, which have paved the way to this most important change, have too frequently been marked by the occurrence of alarming and serious accidents to the enterprising experimentalists, while engaged in their researches with those tremendous agents, the "mixed gases," as they have usually been denominated.   Foremost in this little phalanx of bold and ingenious master spirits appears the honoured name of Dr. E. D. Clarke, whose thirst for chemical and mineralogical knowledge led him, while thus engaged, repeatedly to the detriment of his health and the imminent hazard of his life.   Many persons as well as myself, I doubt not, will recollect an engraved representation of the Doctor's blowpipe, placed on one side of a thick deal wall or screen, with the jet pipe (the orifice of which seldom exceeded one fortieth of an inch in diameter) passing through the said wall into an adjoining apartment, where the operator directed its action in comparative security.   Prudent and even necessary as this precaution must have been originally, future chemists will probably view the engraving to which I have

* Communicated by the Author.

alluded with a smile combined with feelings of wonder and congratulation at the march of scientific acquirement. The extremely hazardous character of Dr. Clarke's blowpipe, yet notwithstanding his brilliant experiments and discoveries therewith, speedily turned the attention of eminent scientific chemists to its adoption and improvement. It would be not only tedious, but, at this day, equally unnecessary to recapitulate all the ingenious expedients which have in turn been rendered available, with various degrees of success, for the purpose of guarding against and subduing the explosive properties of oxyhydrogen gas in its applications as an agent of chemical research. Our brief consideration of the principal methods hitherto in use, may be reduced mainly under two heads. First, the employment of separate reservoirs to contain the respective gases (subject to condensation or otherwise) which are made to unite in, as near as possible, the component proportion for forming water, immediately previous to their exit from the extremity of the jet pipe and their consequent ignition. This method has doubtless the merit of perfect security, but, though I have devoted no small share of attention to experiments of this description during the last twenty years, I have never been able to obtain upon this principle so great or so uniform degree of heat, as when I have employed the gases in a state of previous admixture; nor does it appear practicable thus to unite them, however ingenious the means, with sufficient accuracy to ensure the fullest effect to which they are competent when burnt from a single gas holder. The second class of securities to which I have alluded embraces the object of combining the gases in due proportion, previously to their being conveyed into the blowpipe, and then preventing a retrocession of the oxyhydrogen flame, by the interposition of a safety medium. Now it is a well known fact among chemists that this gas will not inflame below a certain degree of temperature; the heat yielded by a piece of common touchwood in a state of glowing ignition, for instance, will not set fire to a stream of this gas, unless the wood can first be excited into a flame; therefore whatever substance will allow of the free passage of the gas through its interstices, yet at the same time invariably reduce the temperature of the burning jet below the ordinary point at which its combustion obtains, will necessarily preclude its return in an ignited state to the gas reservoir, and thus constitute a security against all explosions. To accomplish this desideratum, series of wire gauze, tubes charged with iron or steel filings, and, above all, a congeries of small wires wedged longitudinally into a brass tube, have repeatedly been adopted. I may permit myself to remark,

and it will certainly be without a shadow of invidious feeling, that several years prior to these several modes becoming public, they had a fair trial in the course of my own experiments, but in my hands they were not invariably successful in preventing explosions. With the congeries of longitudinal wires, for a time, all went on well; but if by any chance moisture found its way among them, or they became oxidated, a consequence which assuredly followed by frequent use, then the certain finale was a "blow up," and the dispersion of my apparatus in fragments throughout the laboratory.

Now, as all projectors and inventors doubtless feel a much greater degree of confidence in their own schemes than can be immediately appreciated by other people, I shall forbear to expatiate on what I conceive to be the merits of an apparatus about to be submitted to intelligent and unbiassed men of science; contenting myself with simply remarking that I have successfully employed it during many years past and in some thousands of instances. Its security I have found *absolute*, and its powers such as I would rather leave to be decided by experience than resting on my own assertion.

Description of the Silex-safety Blowpipe.

Fig. 1, Plate V, represents a central perpendicular section of the instrument and its immediate appendages, the precise nature of which, I think, may be rendered perfectly intelligible without the aid of letters of reference, provided the reader will place the figure before him while he peruses the subjoined particulars. The body of the apparatus consists of a cylindrical vessel, eleven inches in height and four and a half in diameter, formed from sheet copper about one-sixteenth of an inch in thickness, and is firmly supported by a circular foot or base of the same material, projecting to the extent of one and a quarter inch from the perpendicular line of the cylindrical vessel, the bottom whereof terminates the horizontal plane from whence the projecting base commences. The top of the instrument, or superior circular disc of the cylinder supporting the tubular appendages to be next described, is not permanently soldered into its place, but is provided with a rim which descends within side of the cylinder to the depth of three-eighths of an inch; and, by accurate grinding of the two surfaces in contact, is made to fit air-tight, or may be easily so rendered by the occasional use of any of the cements which become liquid at a low temperature. This arrangement is found advantageous, as, by the application of a spirit lamp flame for the space of a few seconds, the top

of the instrument may be removed and replaced with facility, and thus give free access to the interior works of the cylinder, though such approach can rarely be required. Consecutively with each other in a line drawn across the centre of the copper disc forming the top of the instrument, and with their lower rims permanently soldered therein, arise the three brass tubes seen in the sectional figure, surrounded concentrically at their respective bases by a second piece of tube which has the effect of rendering them severally secure and firm in their required positions. We will explain the object of each in succession, beginning with the one most conspicuous on the right hand. This tube is two and three quarter inches in height, and one inch and a quarter in diameter, furnished at the top with a milled-edge screw cap and a collar of leather for the purpose of closing it air-tight. From the side of this upright tube projects a second, three-fourths of an inch in length, and at right angles with the first, containing the socket which receives the screw of the accompanying stop-cock ; the opposite end of the latter being in like manner attached to the base or commencement of the annexed safety-tube, three inches in length, and seven-eighths of an inch in diameter, from the right hand end whereof proceeds, to the distance of five-eighths of an inch, a stout brass projection properly perforated and communicating with the main tube, and into the cavity of this connecting piece are carefully ground the several jet pipes, straight and curved ; a method whereby they are much more easily and expeditiously shifted for use, during the various operations to be carried on, than they can possibly be when attached in the ordinary way by means of screws. The extreme ends of the jet pipes (of brass and neatly turned) are made to terminate in a globular enlargement, as seen in the figure, whereby they are preserved from overheating and destruction when the gas flame is long continued upon refractory bodies placed upon charcoal or other supports. We come now to describe the central tube of the three, seen in the section before referred to : it is one and three quarter inch in height, and one inch and an eighth in diameter, covered by a brass plate at top, through the centre of which is drilled a circular aperture, three-eighths of an inch diameter at its superior edge, and decreasing downwards, so as to receive an inverted cone of solid pewter, carefully turned to fit perfectly air-tight when the requisite degree of pressure is applied. The mode of obtaining this pressure is simple and effective : from the inverted base of the pewter cone, and secured thereto by means of solder, ascends a small brass wire until it reaches an inch or little more above

the horizontal cross piece of the right angled vertical stays,
which are attached, by means of a small screw, on each side
of the main tube, a little below the edge of its circular top.
Over this wire, to the height of three quarters of an inch, and
resting loosely upon it as well as the inverted cone, is placed
a helix consisting in five or six spiral turns of very elastic
steel wire, about one-sixteenth of an inch in thickness. Again,
upon the upper extremity of the helix rests, also loosely, a
circular disc of brass, three-eighths of an inch in diameter,
and one-eighth in thickness, its upper surface being a perfect
plane, while underneath it is shaped somewhat concave with
a depending rim, to retain in its proper position the upper
termination of the helix before mentioned. Upon the upper
surface of this small brass disc is resting the end of a screw
which is hollowed longitudinally, and passes freely over the
perpendicular wire arising from the inverted conical valve.
This hollow screw has a delicately cut thread on its external
surface, and, when turned by its milled-edge nut on the top,
revolves about the perpendicular wire, and works into a cor-
responding socket in the horizontal cross piece of the vertical
stays; and thus, by means of the spring helix, regulates with
perfect facility any degree of pressure which it may be
desirable to put on the conical valve. The third or small
tube on the left hand may be one and a half inch in height,
and about five-eighths of an inch internal diameter; it is
simply closed by a projecting milled-edge screw cap and a
collar of leather, for the purpose of ensuring it air-tight, when
the apparatus is used. From the left-hand side of the copper
cylinder, and at the distance of one and a half inch from the
top thereof, proceeds a substantially made stop-cock, ter-
minating outwardly with a screw in the usual way, or, at
pleasure, with a hemispherical cap, having a smooth cylin-
drical cavity in its centre, for the reception of a corresponding
tube, ground to fit air-tight. This stop-cock communicates,
by means of the usual socket, with a small circular copper
box, about one inch and a quarter in diameter, well secured
by solder on the inside of the cylinder. From the back part
of this box, and communicating with its cavity, descends,
diagonally across the middle of the cylinder, a copper tube,
three-eighths of an inch in bore, the lower end whereof ter-
minates in a plane parallel with the base of the instrument;
which plane is perforated with four or five holes the size of a
large knitting needle, and reaches to within half an inch of
the bottom of the cylinder; the tube being securely held in
its diagonal position by an effective soldering to the back of
the copper box above mentioned. To the distance of three-

fourths of an inch from underneath the centre of the bottom of the copper cylinder proceeds a brass tube of about five-eighths of an inch bore, as seen in the sectional figure, but necessarily concealed in perspective by the projecting base of the instrument. This tube in ordinary is securely closed with a good cock. and serves, upon the removal of the latter, as an outlet for the water charge employed when the apparatus is brought into operation in connexion with a reservoir of the mixed gases.

The instrument as hitherto described is, perhaps, as portable as need be desired, being wholly of metal, and weighing only fifty-three ounces, including stop-cocks, safety-tube, &c., charged for use, together with half a dozen jet pipes perforated in progressive degrees from one-twenty-fifth to one-tenth and one-eighth of an inch in bore, to suit the various purposes of the operator.

Preparation of Blowpipe and adaptation of Apparatus generally for use. (See fig. 1.)

Having closed the lower orifice of the copper vessel with a good elastic cork. the cap at the top of the left-hand tube is to be unscrewed, and, by means of a small funnel, the cylinder charged with water until its surface reaches to within three-fourths of an inch of the bottom of the tubes, leaving a circular area or space as denoted by the unshaded portion of the sectional figure. The funnel being removed and the left-hand tube again closed, the cap of the principal tube on the right is now to be removed, incidental to its being charged (not over tightly) with a piece of good soft and *slightly* moistened sponge of uniform texture, which being effected, the cap of this tube is also to be replaced and rendered airtight by means of its leather collar.

In order to procure the granulated silex for charging the safety tube already described, a quantity of what is commonly called "drift sand," a substance now found adundantly on all our Macadamized roads, should be subjected to repeated washings with clear water, until nothing but the silicious granules are found remaining. These, being first dried, are to be separated, the large and irregular from those appropriate to the purpose, by the use of a proper sieve which will allow of the passage of none but the finer and most uniform particles ; with these the safety tube is to be *accurately filled,* so that no motion or change of place can occur among the granulations, when the tube is screwed to its connecting stop-cock, which being done, the jet pipe selected for the occasion may now be attached to the silex tube ready for use.

What I have next to communicate relates to the valve surmounting the central brass tube, and will, I anticipate, produce a smile from our scientific friends, when I inform them, as I now seriously do, that *it is a work of supererogation ;* and, notwithstanding the pains I have taken in detail to render its construction familiar. I regard it as being utterly useless to the main intention of the instrument. The question will most naturally arise—Why then has it been adopted, and for what reason should it be retained? I answer that, though it can never be directly necessary to complete the security furnished by this form of blowpipe, it is, nevertheless, of considerable *indirect importance.* I am myself convinced to demonstration of the absolute security afforded by the interposition of the granulated silex, nor should I, for one moment, hesitate to discard even the water cylinder, and without any intervening medium whatever, attach my silex tube at once to a gas holder containing a thousand cubic feet of the explosive mixture, by the side whereof I should work with the composure which arises from implicit confidence, that dependence being not merely a theoretical consequence, but the result of more than twelve years' practical assurance necessarily begotten by experiments repeated almost ad infinitum. But I have no right to expect that my assertions alone, or the evidence growing out of my individual experience, can, in the absence of ocular proofs, produce conviction to the minds of others, especially in a pursuit fraught from its earliest origin throughout with a frequency of the most tremendous explosions and their usual alarming train of effects. Mr. Hemmings, several years since, in speaking to a scientific friend, of his apparatus for burning the mixed gases, said "his friends were afraid of him, and, although *he* felt quite safe in using his tube, even with a Pepy's gas holder, yet those near to him always made a simultaneous movement when he began to operate." Many a time and oft, not only in the lecture room but during the delightful hour of association with a knot of friends in the privacy of the laboratory, spite of theory, demonstrations of security, and the exhibition of cool confidence on my own part, have I witnessed this "simultaneous movement," when about to employ my silex tube for the deflagration of the combustible gases.* It is, therefore, not only

* Among the personal friends whom I have the pleasure to recognise as having devoted their talents to the advancement of this department of chemical science, I cannot but recall to my mind, with a feeling of extreme satisfaction, the name of Mr. T. O. N. Rutter, of Lymington, Hants. This gentleman is the inventor of a very improved species of oxyhydrogen blowpipe, whereby the

essential to provide an absolute security but also absolutely
essential to produce an ocular means of conviction that such
security exists, guaranteed even to a threefold degree, ere we
can hope to render the oxyhydrogen blowpipe extensively em-
ployed, and while we disarm the natural timidity of inexpe-
rience, to place it beyond the power of the inadvertent or
careless operator to compromise himself and his friends in any
more than a harmless pop-gun explosion. These, then, are my
reasons for retaining the valve already described, and which
may be adjusted with remarkable facility to any degree of
pressure, simply by turning the milled-edge nut of the vertical
screw previously explained in reference to fig. 1. We shall
presently have occasion to recur to the subject of this valve,
when treating of the operation of the instrument; but I will
avail myself of the present opportunity to suggest to my scien-
tific friends, that they may find this description of safety valve
extremely simple in its action and effective in practice, as
applied to philosophical apparatus on many occasions, wherein
its *safety* properties will be more expressly operative and de-
sirable than in the present instance of its application.

The reader is now referred to fig 2, Plate V. The arrange-
ments hitherto prescribed having been carried into effect with

respective gases are condensed into separate copper reservoirs of
large dimensions, and ignited at the instant of their union in a com-
mon orifice analogous to the principle some years since adopted by
Professor Hare, of Philadelphia. One of these instruments con-
structed to order by Messrs. Jones of London, weighed, indepen-
dent of the condensing syringe, 53 pounds; thus providing for long
continued action, certainly, in numerous instances, a most important
consideration, without even the shadow of danger from explosions.
In reference to the point of my remarks in the text, I am sure my
friend Rutter will excuse me, if I quote a few lines from one of his
communications to me a few years since, as nothing can more for-
cibly illustrate the frequent danger, as well as the general feeling
of apprehension necessarily connected in ordinary with gas-blow-
pipe experiments. I must premise that, as a mere memento of
esteem, I had forwarded to Mr. Rutter one of my silex safety tubes
for his acceptance and trial. He says, "Although I have your assur-
ance that your silex tube is *safe*, I cannot conceal from you that I shall
use it with 'fear and trembling.' An accident that happened to
me in May, 1832, makes me very careful of explosive mixtures. I
had a 'blow up' that left me bleeding out of about forty different
places, spoiled an entire suit of good clothes, disfigured about fifty
volumes of books, stripped the ceiling of some of its plastering down
to the bare laths, and left other marks that will not be easily obli-
terated, to say nothing of a glass receiver, a porcelain ditto, and
various other articles of apparatus which were totally destroyed."

the requisite degree of precision, the blowpipe is to be placed towards the left-hand end of a strong and rather heavily formed table of convenient height, which, from its weight and resistance of casual disturbance, will constitute a material advantage to the operator. The necessary connexion, between the blowpipe and the gas holder to be employed on the occasion, is now to be established by the intervention of an elastic gum tube from three to four feet in length, or of greater extent, at pleasure ; one end of the tube being attached by an ordinary screw-socket to the extremity of the stopcock from the gas holder, and the other, by means of a *union joint*, to that of the blowpipe. In working I prefer to place the gas holder completely within reach of my left hand, as being thereby more under my immediate command and proportionately conducive to convenience. The various gas holders acting upon the hydrostatic principle—such, for instance, as the ingenious and (generally) valuable invention of Pepy's—I find are by no means well adapted to the purposes of operation with the oxyhydrogen blowpipe. Their use not only involves the necessity of an assistant. but it is utterly impossible to obtain by their means an equable and uniform degree of pressure; both of which objects are secured to a single operator by the use of the gas holder and gasometer shown in the figure ; and, though the one I am in the habit of employing (capable of containing several cubic feet of the explosive mixture) differs in some few respects from those in general use among chemists, I am sure its operative principle is too obvious and generally known to require from me any especial description. The arrangement of apparatus herein suggested is, I believe, as compact and commodious as can well be desired, either for the purposes of the public lecturer, or those of private manipulation ; the blowpipe, owing to its perfectly flexible medium of communication with the gas holder, being readily brought into any required position, or placed at the most desirable degree of elevation as respects both operator and observers.

Operative management of Apparatus.

The preliminary arrangements being completed, let the operator place himself alongside the table and right abreast of the blowpipe ; he will then with his right hand have entire command over the ignited jet, his several supports, and the substances to be reduced or otherwise acted upon ; while with his left hand he will secure the management of the gas holder and regulate his supplies therefrom. Let him commence his

operations by loading the inverted gas bell with the requisite degree of weight; this done the stopcock next the safety tube is to be turned open, either wholly or in so much as may be deemed adequate to the especial occasion, subject to subsequent correction in working the instrument. The communication with the gas holder should now be established by the operator opening the other two stopcocks in succession with his left hand, and he will find there will immediately follow a continuous low gurgling sound, occasioned by the passage of the gas down the interior copper pipe, formerly described, and its re-ascent through the water of the cylinder. He may, without further delay, apply a taper or lighted match to the extremity or knob of the jet pipe, when a cone of flame (regulated by the degree of pressure on the inverted bell, the opening in the airway of the stopcocks, and the orifice of the jet employed) from two to ten or twelve inches in length, and of proportionate diameter, will be brought into instantaneous play upon the subject of his experiments.* So long as it is intended to continue the instrument in action, notwithstanding a temporary suspension of the gas flame may occasionally prove to be requisite, it will be unnecessary to interfere with the stopcock proceeding from the gas holder, and communicating directly with the elastic tube. The left hand stopcock of the blowpipe being closed—and it will be infinitely preferable to cut off and regulate the supply to the jet pipe principally by means of this stopcock—the gas flame will in the next instant become extinguished, and its disappearance be accompanied by a faint and harmless report only, proceeding from an explosion of the last portions of gas remaining in the tubular cavity between the knob of the jet pipe and the anterior extremity of the silex safety tube.

* These lengths of flame and even greater are safely attainable from the use of this instrument; but it is a mistake to suppose that this circumstance is of any practical importance. The point of greatest heat is in fact to be found near the orifice of the jet pipe from which the gas issues. But though the length of the gas flame be of secondary consideration to the operator, whatever tends to increase its diameter is of the first-rate advantage, as it will enable him not only to bring much larger bodies within the sphere of its action, but he thereby obtains a very great augmentation to the degree of heat. In this particular respect the blowpipe now in question affords far greater facilities than any other form with which I have the pleasure to be acquainted, wherein a single gas holder is used, especially when the gas flame is directed downwards upon any substance rather than in a horizontal line.

Observations on the principle of safety.

In the sectional representation, fig. 1, it will be perceived, as we have already had occasion to mention, that the water line terminates at the distance of three-fourths of an inch from the top of the copper cylinder, the intervening space being occupied by atmospheric air, until that is displaced by the introduction of the mixed gases a result which speedily follows when the apparatus is put into action. This provision is necessary in order to accommodate an increase in the volume of water which immediately obtains from the continuous ascent of gas bubbles as they emerge from the orifices of the diagonal tube, and which water would otherwise rise into the main upright tube charged with sponge ; the latter substance effectually prevents any moisture whatever, occasioned by the breaking and splashing of the bubbles as they rise to the surface, from finding its way into the air passage of the stopcock, or insinuating itself, in consequence, among the granular silex of the safety tube. Now, *supposing,* for the sake of illustration, that the ignited gas jet could from any circumstance recede and burn backwards through the silex tube, the sponge medium (though not here designed for such purpose*) is amply capable of extinguishing it ; or, allowing a second supposition, that the sponge should fail in arresting the course of the fiery renegade, the only mischief he could possibly effect would be the explosion of an insignificant quantity of gas, from three to four cubic inches in amount, by the force of which the conical valve in the central tube would be instantly raised, and the final consequence a harmless report. I presume it would be superfluous to enter on any argument in order to show that such explosion, even if it were possible in itself, could in any way extend to where the only source of real danger resides, viz. in the gas holder or reservoir connected with

* Many years since I placed at the disposal of the talented editor of the " Mechanics' Magazine," J. C. Robertson, Esq., an essay on the combustion of the mixed gases, with the description of an apparatus for their secure deflagration, in which the safety medium is constituted solely by sponge. I believe this essay appeared in Vol. VII. of the popular and valuable work to which I have referred. Years of subsequent experience have only served to strengthen the confidence I then expressed relative to my sponge safety tube, provided the rules I have prescribed for its management be *implicitly* followed; otherwise, through the sponge becoming dry and shrunken, &c. &c., explosions may and will happen, but the most incorrigibly careless operator might be defied to render inefficient the successive securities afforded by the silex safety blowpipe.

the blowpipe, as the flame would have in the first place to dive through a column of water nearly a foot in height, and then, further suppositions are obviously out of the question.

During a very considerable portion of my life, the oxyhydrogen blow pipe, in its various forms, has been to me a source of intense interest, both in reflection and practice ; and few persons, it is probable, have ventured on a course of more hazardous experiments unattended by personal injuries or other serious contingencies. These fortunate and, in latter years, entire exemptions from accident, I certainly owe to the adoption of the silex medium, a substance possessing, in my estimation, the true character requisite for such a purpose, that of absolute *immutability*, whereby, if once brought into operation with suitable effect, the employment of the mixed gases is rendered an agency of really astonishing, if not unlimited, power in chemical researches and many purposes of the arts. With the identical instrument furnishing the descriptive portion of this memoir, the reduction of the several earths to their respective bases, and the consequent production of barium, calcium, &c., has not unfrequently been, as it were, the magical conversion wrought by a few seconds' application.

XVIII. *Description of an Electro-magnetic Engine. By* JAMES P. JOULE, ESQ. *In a letter to the Editor.*

Broom Hill, near Manchester, 30th August, 1839.

Dear Sir,

I hasten to forward to you the description of my new electromagnetic engine. I am particularly desirous of communicating any new arrangement, in order, if possible, to forestall the monopolizing designs of those who seem to regard this most interesting subject merely in the light of a pecuniary speculation.

Fig. 3, Plate V, represents a perspective view of the engine, in what may be termed its " quadratures." A, B, C, D, is a frame of wood made very firm, and strengthened by a board fixed at the back ; the top E may, by means of thumb screws, be taken off at pleasure ; the inside measure of the frame is $4\frac{1}{2}$ feet long, $1\frac{1}{2}$ feet high, and 1 foot broad. F, G, is the axle of cast steel, three quarters of an inch square, and turned to half an inch at the working parts. *a, a*, are holes by means of which the axle may with expedition be let into the brass steps *b, b*. H, H, are holders for the revolving electromagnets *m, m* : there is another holder between these, which

is omitted in the figure, but which contributes very materially to the stability of the apparatus. *n, n,* are the stationary electro-magnets, firmly secured to the top and bottom by the brass clamps *k, k. s, s,* are silver springs which enter through the frame and press gently on the brass half discs of the commutator *c.* The commutator, figs. 3 and 4, consists of two half discs of brass, inlaid in hard wood: the silver springs being of the same size at the ends as the distances between the half discs; the electric current is instantaneously reversed. *r, r,* are copper wires, soldered to the brass half discs, and connected with the revolving electro-magnets. *x, x.* are wires let into the frame, and connected with the stationary electro-magnets. *z, z,* are wires to connect the coils of the two systems.

Fig. 5 will more clearly represent the plan of my holders; they are made of hard wood, and brass plates are screwed to the sides and faces; *b, b,* are brass screws, which work upon wedges, so that I can adjust the magnets at different distances from the axle; *a* is a screw which secures the holder to the axle.

The iron of the revolving electro-magnets is thirty-six inches long, three inches broad, and half an inch thick; that of the stationary electro-magnets is of the same breadth and thickness, but their length is such that their extremities when bent are $36\frac{3}{4}$ inches asunder; 3-8ths* of an inch is therefore the working distance.

I shall provide myself with four of the straight electro-magnets, two of hard and soft iron, and two of hard and soft rectangular iron wire: the hard iron and wire magnets are completed and fixed on the engine. Fig. 6 represents the position and exact size of the wires, where *a, b,* are planed iron rods to secure them with certainty.

Each electro-magnet is furnished with a strip of thick calico next to its iron, and with three separate layers of well covered copper wire, 1-24th† of an inch in diameter, each 106 yards long. I always take care to ascertain that each coil is properly insulated by experiment, and the method which I employ in winding the wire ensures regularity.‡

My engine consists of only four electro-magnets, but it will at once be seen that the principle admits of the use of any superior even number; I do not however think that the mere

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* 1-8th would be better.

† I have reason to think that this is too thin.

‡ A heavy weight is fixed to the wire, and wound up as the magnet is covered.

increase of number is attended by advantage, unless the friction of the axle is thereby diminished.

As the form of this engine enables me with ease to place the electro-magnets in different positions, and as the several coils are insulated, and I am thereby enabled to use the electric current in quantity and intensity arrangements, this engine is well adapted for experiment. It works beautifully, but as I have not as yet carried my experiments with it very far, I prefer to postpone for the present any account of its duty.

Vol. IV. page 132. line 23. read ·62 instead of 62.

<div align="center">

I remain, Dear Sir,

Yours most respectfully,

JAMES P. JOULE.
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XIX. *Report on a Memoir, by* M. MASSON, *relative to the action exercised by chloride of zinc on alcohol.**

For some years past the question of the formation of ethers has been studied by chemists with so much care, and under so many different aspects, that it might seem difficult to find anything new in so simple a reaction as that which forms the subject of the present memoir ; yet nothing was less expected than the results obtained by M. Masson which we are about to mention.

The author dissolved chloride of zinc in alcohol, and submitted the liquid to distillation, taking care to break the products, and keep exact account of their nature ; but he found that in proportion as the liquid boils he at first lost alcohol, but when his point of ebullition, which was raised gradually, arrived at 130°, or rather 160°, it produced sulphuric ether.

Thus chloride of zinc acts on alcohol exactly like concentrated sulphuric acid ; and what is very remarkable, these two bodies determine the production of sulphuric ether at precisely the same temperature.

Carrying the experiment further we discover an oil, which, in its character, exactly represents the oil known by the name of *sweet oil of wine.* It is formed at about 160°, i. e. nearly under the same circumstances as when we operate with sulphuric acid and alcohol.

We observe, further, that the ether disengaged is always accompanied by a certain quantity of water, and that it is

also the same with the sweet oil. These phenomena are also remarked in the reaction of sulphuric acid on alcohol. M. Masson is also assured that hydro-chloric ether is not produced, which fact he but little expected.

Hence M. Masson has perfectly proved that chloride of zinc acts like sulphuric acid. There now remains to consider a certain number of phenomena which the author has hitherto thought proper to neglect, and which take a great part in the reciprocal action of the sulphuric acid and alcohol. In fact, the analogy observed by M. Masson between chloride of zinc and sulphuric acid is so perfect, that it is difficult to believe that chloride of zinc does not furnish some product corresponding to sulpho-vinic acid. It is this that M. Masson has not sought to verify, and which we recommend to his attention.

Hitherto we have admitted that the author had obtained sweet oil perfectly similar to that obtained by the assistance of concentrated sulphuric acid. However, M. Masson has not confined himself to the establishment of this identity : he has considered the oil that he has obtained, and is satisfied, by attentive distillations, that it contains two very different products.

The first, the most volatile, is carburet of hydrogen, the most hydrogenated liquid known : it contains more hydrogen than olefiant gas, and is represented by $C^{..} H^{..}$; it boils at about 30 or 40°.

The second, the less volatile, contains, on the contrary, less hydrogen than olefiant gas ; and is represented by $C^{.} H^{.}$, and boils only at about 30 0°.

These results joined to those by which M. Regnault has demonstrated the absorption of oxygen gas by sweet oil of light wine, will perfectly explain why some chemists have obtained from its analysis more carbon than is contained in olefiant gas ; and others, on the contrary, have hit on the composition of olefiant gas itself.

These facts which appear to us well founded, would have led your Commissioners to regard M. Masson's work as of a nature to settle the discussions relative to the sweet oil of wine ; but M. Marchand, a German chemist, who has been much engaged with sulpho-vinates, has recently published some analyses of the oil of strong wine, as well as some analyses of the oil of light wine, or some crystals which it furnishes. His results agree perfectly with those of Serullas, and, consequently, differ from those obtained by M. Masson under the inspection, and in the laboratory, of your reporter.

Considering that among the chemists who engaged in this subject, some have operated on the oil obtained by sulphuric

acid and alcohol; others on the oil of sulpho-vinates; and that M. Masson has procured his by alcohol and chloride of zinc, some might perhaps imagine that these several oils differ among themselves.

So much the more, since M. Masson has never been able to extract from his oil the crystals obtained by Hennell, Serullas, and Marchand, from theirs; and that, on the contrary, he has extracted a very volatile product unknown to the chemists who have preceded him.

But M. Marchand has undertaken to remove this last difference; for he distinguishes among the products of the distillation of sulpho-vinates, the existence of a very volatile product which he has not analyzed, but which appears to have the greatest relationship to that which M. Masson has discovered some time ago.

Hence, it is evident that the history of the sweet oil of wine is not yet terminated; but M. Masson has done a great deal in clearing from it the existence of a very volatile carburate of hydrogen $C^4 H^9$.

The reciprocal action of chloride of zinc and alcohol has been so well studied by the author of the memoir which engages our attention; it has been on his part the object of experiment so worthy the attention of chemists; he has so well proved that it gives rise to sulphuric and not hydrochloric ether, that we do not hesitate to request the insertion of his memoir in the " *Recueil des savants étrangers.*"

The conclusions of this report have been adopted.

XX. *Employment of Gelatine for Food.**

M. Arago mentions that during his last visit to Metz, he received a letter from M. Darcet inviting him to visit the hospital of St. Nicholas, where they used gelatine for food: and he wished on his return to state to the Academy what he had observed. M. Arago subscribed at the desire of his professional friend, fearing, that during the examination of facts, he should be led astray by the influence of those prejudices which he had formerly entertained against the elementary regimen, the subject of so lively and prolonged a debate.

The hospital of St. Nicholas at Metz, contains more than five hundred persons, men, women, and children. The men and women are all very old. Each individual receives twice

* From the Comptes Rendus, 24th December, 1838. Translated by Mr. J. H. Lang.

a day, five days in the week, a soup containing *a quarter of a litre* (a little more than half a pint) *of broth*, which, for a thousand rations, is prepared with the gelatine produced from twenty-five kilogrammes (about 52¼ pounds) of bone and ten (22 pounds) of meat.

After the soup in the morning each person receives a ration of dry or fresh vegetables cooked with bacon.

After the evening soup, they receive the bacon which was used for cooking the vegetables in the morning.

The rations of fresh vegetables, such as potatoes, cabbages, carrots, and turnips weigh 37¹ grammes (1¹⁄₃oz). The rations of cooked vegetables, such as haricots, peas, and lentils, 12¹⁄₂ grammes. The rations of rice and millet 5 grammes.

The bones from which the gelatine is extracted come from the military hospital, the college, and the school. All the operations relative to this extraction are made in a room, separated from that in which the old men are, only by a wooden gate.

Before the introduction of the gelatine the regimen of St. Nicholas was exactly as at present; only the soup was prepared with hog's lard, salt, and spices.

It must be well understood that the new rule was not introduced for the sake of economy, the desire of improving the soup of the paupers was the only motive of the administrators. Every quarter of a litre of hog's lard broth comes to 0·92 centime; every quarter of a litre of animalized gelatine broth costs 1·25. centime.

The preceding details sufficiently show that the observations collected from the hospital, *St. Nicholas de Metz*, cannot decide *whether pure gelatine* is nutritive, but they may serve to appreciate the influence this substance exercises over animal economy, when it is mixed with bread, vegetables, and a very light meat broth.

The animalized gelatine broth has been *more than four years in use at the hospital St. Nicholas de Metz.* In these four years, from the unanimous testimony of the honourable directors of this establishment, the state of health of the five hundred persons it contains has evidently improved. The increase is found to be more than compensated by the decrease of expense for the infirmary.

M. Arago has received this information from M. Pedancet, counsellor to the royal court; from M. Prost, director of the fortifications of Metz, second commandant of the school of application, &c. &c.; and from M. Frecot.

The declarations that M. Arago has collected, by visiting the different halls of the hospital, have completely confirmed

the statement of the directors. With two or three exceptions in the section of old women, every one is pleased with the new regimen, and all say it is very superior to the old one, both as to taste and salubrity, and all wish it to be continued.

The military hospital at Metz used lately for the employés a preparation of gelatine which is now done away with. But M. Arago is assured by Dr. Scoutteten, that particular circumstances totally unconnected with the value of M. Darcet's process, are the only reasons for its temporary suspension. The employés are very well satisfied with the use of animalized gelatine broth, and would be glad to see it re-established.

M. Silvester wishes the communication of M. Arago to be inserted with detail in the *Compte Rendu.*

M. Magendie, on the contrary, regrets it has been made in the public session. He thinks M. Arago ought to have confined himself to sending *his conclusions* to the gelatine commission. This commission pursues with zeal and perseverance the duty that has been assigned to it, and in a short time will present its report. We shall then appreciate all the efforts it has made to overcome the difficulties of the question, and to escape the uncertainty with which complex experiments, such as M. Arago speaks of, abound. M. Magendie is of opinion that the gelatine might be abolished in the hospital *St. Nicholas de Metz* without the old men and children to whom it is given perceiving it, and certainly without their feeling any harm from it.

M. Arago, answers that he has not presented *conclusions,* but only facts. The experiment at Metz, *considered,* if not physiologically, at least in an economical point of view, appears excellent even after the observations of M. Magendie. He doubts the commission having had the means of undertaking any thing of this sort so extensive, either as to duration or with regard to the number and diversity of the persons submitted to the gelatine regimen. In reply to the reproach of having addressed the academy in preference to the commission, M. Arago says he did it for the satisfaction of M. Darcet, who for the last seven years has been wanting to be relieved from the most uncomfortable position. The perpetual secretary adds that he will never hesitate, within the limits of right, justice, and truth, to render his professional friends all the services which are in his power.

M. Magendie declares the experiments on the nutritive properties of gelatine to have been commenced only two, and not seven years, ago.

M. Dumas adds that facts analogous to those which M. Arago has laid before the academy have not been disregarded

by the commission : that he, for example, has had an oppor-
tunity of personally considering, on the spot, the results
obtained at *the hospital St. Nicholas de Metz.*

XXI. *Note on the paralysis and nevralgie of the face.* *By* M. MAGENDIE.*

The discussion which took place at the last session has
reminded me of some medicinal facts, which, having a ten-
dency to throw light upon the functions of the nerves of the
face, I think worthy to be laid before the Academy.

I have lately paid attention to two young persons affected
with hemiplegia of the face, a disease which at the same time
compromises the receiving of the food, mastication, speech,
the action of whistling, &c.; but which, above all, raising the
expression of the figure, and rendering it completely immove-
able, particularly when the passions are the most animated,
renders the countenance something monstrous.   If this para-
lysis were shown at the same time on both sides, the figure,
that moving picture of our thoughts, would assume by its
immoveableness a frightful aspect : it would be an inanimate
mask on a living head.

This state is fortunately very rare, and not even met with
in the case of general paralysis, where every kind of motion
of the body and limbs is stopped : by the goodness of provi-
dence there remains to the patient his phisiognomy to express
his sufferings or desires.

I have had the good fortune to cure these paralyses of the
face by means of electricity applied to the nerves themselves,
with the assistance of platina needles.   In the course of these
applications I had the opportunity of remarking that the
nerve became perfectly incapable of exciting the contractions
of the muscles, preserving however a sensibility which
apparently differed nothing from the sensibility it possessed in
its healthy condition, which supports the results of my last
experiments ; proving that the sensibility of the facial nerve
is only apparent, being no other than that of the threads of
the nerves of the fifth pair which intermix with the threads of
the facial nerve.

There is nothing, in this sensibility, existing in a paralyzed
motive nerve which ought to surprise us now, though for a
long time I was much perplexed by it.

* From the Comptes Rendus, June 17, 1839. Translated by
Mr. J. H. Lang.   ●

It ought, theoretically, not only to exist in the trunk, but in all the branches. It is this also which exists, and which I have several times mentioned, in the two diseases I have just spoken of.

M. Roux, mentioning a nevralgie case in which he cut all the facial branches in succession, and perceived the pain take refuge successively in the uncut branches, has turned my attention to these very mysterious relations which are seen among the branches of the same nerve.

I saw a very remarkable instance of this last month.

A middle aged woman came to me to be cured of a most intense nevralgie which for five years had given her no relaxation, and had entirely deprived her of sleep.

This nevralgie did not occupy every branch of the fifth pair at once, but was sometimes in one, sometimes in the other, and always with equal violence ; the place varied but the pain was the same.

The day she came to me the disease was seated in the right lower maxillary ; I applied the electric current and the pain immediately passed to the tongue, leaving the maxillary nerve. I forced a needle in the right side of the tongue, and caused the current to pass over it ; the pain entered the under-orbitary. I also followed it there. It went at last to the frontal nerve, at which it was not difficult to arrive : I attacked it there in the same manner, and it disappeared ; fortunately not having taken, as a last resource, the nasal thread of the opthalmic, as in the case quoted by M. Roux ; but which, however, would not have entirely discouraged me, for, by the assistance of a fine platina needle, I do not consider impossible to have got at it ; and, consequently, to have directed an electric current to it.

My patient, to her great satisfaction, was thus relieved from a disease which had been the torture of her existence. The disease has since made some attempts to return ; but weak and very tolerable : above all, never preventing sleep : and a single application of the electricity causes it immediately to disappear.

For some days I have had an opportunity of seeing a Parisian lady, who has also been troubled for some time with a nevralgie ; but nearly always situated in the right lingual nerve. I applied the electricity, and the pain passed into the upper maxillary nerve ; from which I immediately removed it. It went no further, and thus saved me the trouble of pursuing it.

How are these transmissions made? Is it by the branches themselves ? or is it by those numerous anastomoses ; little

known to any but professional anatomists? I am not at present able to obtain any well founded opinion as to this curious question.

XXII. *Observations on the means to be adopted for ascertaining the temperature of vegetables. By* M. Becquerel.*

As soon as I had applied thermo-electric effects to determine the temperature of the interior parts of men and animals, I tried the same sort of experiment to ascertain that of vegetables. The general process consists in using two perfectly similar needles, each composed of one steel and one copper needle soldered together at one end, while the free ends are thus connected: the steel ends with a wire of the same metal, and the copper with the two extremities of the wire which forms the circuit of a multiplier. When the temperature is the same at the two soldered parts, the magnetized needle remains in equilibrio; but if there be only the difference of a tenth of a degree, it is indicated by the deviation of the needle. Hence, by keeping one of the soldered parts at a constant and known temperature, we may, by means of a table which gives the relations between the deviation of the needle and the difference of temperature, ascertain that of the other soldered part which is variable.

In men and animals, whose interior temperature is much above that of the surrounding air, we place one of the soldered parts in an apparatus having a constant temperature of 38 degrees, and the other by means of puncturation is admitted to the part we wish to explore.

When I wished to apply this process to vegetables, I immediately saw the impossibility of using an apparatus of a constant temperature, on account of the slight difference existing between the temperature of the air and that of vegetables: the apparatus used for animals requiring a difference of a certain number of degrees to be employed to advantage.

Being anxious, however, to resolve the question with regard to vegetables, as I had done in conjunction with M. Breschet that respecting animals, I requested M. de Mirbel, two years ago, to join me in making experiments at the " Jardin des Plants." He accepted my proposition, and immediately placed at my disposal those shrubs which were likely to be

* From the Comptes Rendus, June 17, 1839. Translated by Mr. J. H. Lang.

most useful to us. I soon perceived the difficulties I had to overcome in attaining the desired end. We commenced by piercing one of the shrubs with a very delicate gimlet, so as to introduce one of the solders. The needle introduced was soon changed, which produced an electro-chemical current. To remedy this evil the needles were covered with several coats of gum-lac varnish ; the other solder remained in the air, where the temperature was, to all appearances, constant ; but the radiation not being the same at the two extremities, the one being covered with the ligneous tissue, and the other in the open air, there resulted very complicated effects which required to be resolved, as, otherwise, we could have no means of ascertaining the temperature of the vegetables. M. de Mirbel proposed that I should operate in the middle of the garden, placing the apparatus in one of the gardeners' cabins. I accepted his offer, and on entering perceived a tree in full vegetation (an acacia I believe), and at the side a branch of the same tree. It immediately struck me, that to avoid the difference of radiation which was an obstacle to the success of our experiments, we might put one of the solders in the living and the other in the dead branch of the same tree, both having apparently the same diameter. This experiment which was indicated by theory succeeded perfectly ; and we soon perceived a difference between the temperature of the living and that of the dead tree. The gardener was commissioned to note the deviations every two hours ; but I perceived the next day that notwithstanding his intelligence he had made so many errors in the observations, that I was obliged to give it up ; intending to return to it when I had finished some labours of another kind which I had already begun.

The next year M. Dutrochet asked me for some instructions on the means to be employed for determining the temperature of vegetables : I told him all I had done on this subject, requesting him to make use of my process which might conduct him to the solution of the question. It is with the greatest pleasure I see that he has made use of them, and the observations already collected will assist the process of vegetable physiology. I do not doubt but the memoir he is about to publish will contain only the details I have just communicated to the Academy ; and for which there was no room in the short paper read during the last session.

XXIII. *Electricity. Extract of a letter from* M. Peltier, *in reply to an objection of* M. Parrot.[*]

The objection of M. Parrot having been only indicated in the *Compte Rendu,* we shall only announce the reply of M. Peltier. The letter of this latter philosopher also contains some considerations, independent of this discussion, and these we shall here mention.

I have shown in former communications, says M. Peltier, that the *quantity* of dynamic electricity is proportional to the quantity of molecules whose equilibrium has been disturbed. I add that the duration of the electric phenomena is equal to that of the passage from the old to the new equilibrium, and that there is a connexion and dependence between these two states.

I have also stated, that the currents of induction increase as the magnetic intensity of the bars; I have since found that it was the same with the caloric, when placed in the shade of the neutralization in return for the quantities we wish to measure. It now remains to discover whether the augmentation of the disturbing force, that is to say, whether a more intense magnetism or a greater elevation of temperature, would really give a more copious current; or whether, on the contrary, the phenomenon of *quantity* was not the product of the electric *intensity,* from this power overcoming resistances, which otherwise would oblige more electricity to be neutralized by the direct circuit. To arrive at the complete solution of this question, we must have measuring arcs, of a perfect conductibility, which it is not possible for us to obtain; but by employing very short circuits, three or four decimeters at most, we can show that the first degree of heat gives a greater deviation than the second, the second than the third, and so on; that the difference in favour of the first degree is as much greater as the resistance of the circuit is less; and, consequently, if the conductibility be perfect, the first degree will give the maximum current in the thermo-electric circuit. This proves also that the intensity of the disturbing force does not increase the *quantity* of dynamic electricity, but gives to the quantity produced more of that power which we call *intensity.* The result of this experiment is, that the causes which do not change the nature of the bodies, as heat and induction, act like piles, in which the *intensity* increases as the number of pairs, while the *quantity* remains the same.

[*] From the Comptes Rendus, June 17, 1837. Translated by Mr. J. H. Lang.

This similitude of effect gives the idea, that there is between the atoms thus disturbed, as between the pairs of a pile, a continuation of the neutralization of the two electric states; and which leave free only the two extreme states, positive on the one side, and negative on the other, which are neutralized over the interposed conductor.

This analogy does not exist when the force of preturbation changes the nature of the substances, as, for example, chemical action. The atom which combines immediately, ceases to form part of the body; there is no longer solidity between them; each molecule produces its electric phenomenon insulated and complete, which has no influence over the phenomenon produced by the neighbouring molecule; thus the intensity remains the same with a feeble or strong reaction, the *quantity* varies only because there is a greater transformation in a given time.

It is a very remarkable particularity, that there is no corresponding ratio between the temperature and the current. We have just said that a double temperature gives the dynamic electricity a double *intensity* of power; but when a current traverses a metallic wire, and it is made to increase as two to one of its powers, the temperature of the wire increases as three to one of its powers.

XXIV. *Letter from* M. BREGNOT, *inspector of buildings to the " Hôtel des Invalides," to* M. ARAGO, *on the thunder clap which struck the dome, June 8th,* 1839.*

I have the honour to enclose you some particulars, corrected from those very inexact ones, sent to the Academie de Sciences, by M. Leymerie, of the effects of the storm, of Saturday, June 8, on the Royal " Hôtel des Invalides."

It is true the thunderbolt fell on the dome des Invalides, and that in consequence of the fracture of one of the metallic chains serving as a conductor, it was precipitated into a small adjoining court: but here stops the correctness of the details forwarded by M. Leymerie.

The chain was perfectly whole and very solid, six days before the event. It was visited on the 2d of June, by the officers of the buildings; and the fracture being about the height of the hand, could not have missed being seen.

I am certain that it was broken by the thunderbolt itself, both from the attentive examination made on the 2d of June, and from the observation made immediately after the storm. We discovered that it was not a simple solution of continuity,

* From the Comptes Rendus, June 17, 1839. Translated by Mr. J. H. Lang.

which would have been the result of the oxidation of the metal, but that a length of at least thirty centimeters (about 12 inches) had been broken and dispersed into an infinite number of pieces of equal lengths, not exeeeding forty millimeters (1½ inch).

This chain is composed of about twenty iron wires twisted together. At the parts in which it was broken it passes through an iron ring or collar, fixed in the wall ; and, without doubt, to diminish the effort of its weight, it was twisted twice round this collar. This arrangement which I think bad, added to the angular direction the chain takes at this place, appears to me to be the cause of the fracture. However, these things have been the same from the establishment of the lightning conductors ; and nothing of the kind has ever happened before.

The following are the other effects of the electric discharge :—

1.   About sixty nails, fixing the gilt lead about the lantern of the dome, on a circumference of about twenty meters,* and at least fifty meters above the spot where the chain was broken, were violently torn out and dispersed in all directions.

2.   The lead was raised by the displacement of these nails, and formed projections from thirty to forty millimeters.†

3.   The lead surrounding the base of one of the columns of the lantern was so torn off that we could not find the least vestage of it ; yet there was no sign of fusion or vitrification.

4.   After the rupture of the conducting chain, the thunderbolt struck a small building in which are the bath rooms ; it broke into four pieces, a stone, about 1^m,25 high, 0.30 wide, and weighing 77 kilogrammes (190lbs.), which it hurled 4 meters from the wall against which it was leaning.

It is remarkable that this stone was not attached to the wall by a metallic connexion, but it hid the orifice of a leaden pipe, used for discharging the water from the reservoir of the baths.

5.   On the right, as you face this stone, is a door leading into the bath rooms.   This door was open at the time the thunderbolt fell ; and although the place contained twenty copper bath tubs, cocks, pipes, and pump apparatus, the lightning did not enter it.   The bath keeper was standing at this door and felt nothing but a very natural fright, as he was not 50 centimeters from the stone which was struck.

6.   On the right (looking at the door), is a window defended by a square iron grating.   The four great squares of this window were broken in pieces ; the largest of which was scarcely two centimeters wide.

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* Each metre is upwards of 39 inches.
† Each millimeter is upwards of ·0397 of an inch.

7. Several other squares were broken; but I think this ought to be attributed more to the shock than any regular action.

8. Finally, on the roof of this building, above the entrance door of the bath room, several of the slates were pierced with holes as if there had been a discharge of grape shot.

These are the details of the effects produced by this storm. My relation, though rather long, if it have no other merits, has that of being exact and conscientious. The assertion of a solution in the continuity of the conductor previous to the storm, and the testimony brought to support it, are not worthy of belief.

We shall return to the thunderbolt of the Invalides, and the consequences likely to flow from it, as soon as we have received different documents of which we are in expectation.

XXV. *Account of a Tornado, which, towards the end of August, 1838, passed over the city of Providence, in the state of Rhode Island, and afterwards over a part of the Village of Somerset. Also an Extract of a Letter on the same subject from Zachariah Allen, Esq., of the city of Providence.**

Communicated by Robert Hare, M. D., Professor of Chemistry in the University of Pennsylvania. Read October 28, 1838.

I propose to lay before the Society, for a place in their Transactions, an account of a tornado which occurred in the state of Rhode Island, towards the end of August last.

This phenomenon was first observed near Providence, over the south western suburbs which it passed in a course generally from west by north, to south by east. Only a few days subsequently I visited some of the most remarkable scenes of its ravages.

The characteristics of this tornado, from all that I could see or hear, are quite similar to those of the tornado which occurred at New Brunswick, New Jersey, in June, 1835, and to which I referred in my paper upon the causes of tornadoes and water-spouts, published in the sixth volume of the Society's Transactions.†

This recent tornado was advantageously seen by J. L. Tillinghast, Esq., from a window of his mansion, which is so situated, on the brow of a hill on the eastern side of the city of Providence, as to afford an unobstructed view of the country opposite. Mr. Tillinghast alleges that his attention was

* From the Transactions of the American Philosophical Society at Philadelphia.
+ The New Brunswick Tornado is described at p. 195, Vol. II. of these Annals.

at first attracted by seeing to the westward a huge inverted cone, of extremely dark vapour, which extended from the clouds to the earth. In the contortions and spiral movements of its lower extremity, this cone was conceived to resemble the proboscis of an enormous elephant, moving about in search of food. Sometimes it was elongated so as to reach the ground; at others it skipped over the intervening space without touching it; but at each contact with the terrestrial surface, or bodies resting thereon, a cloud of dust, intermingled with their fragments, was seen to rise within the vortex. To those who were sufficiently near to the meteor, a fearful explanation of these appearances was simultaneously evident. Ponds were partially exhausted. Trees uprooted or deprived of their leaves or branches. Houses were unroofed, or uplifted and then dashed to pieces. Farms were robbed of their grain, potatoes, fruit-trees, or poultry: nor were human beings secure from being carried aloft, and more or less injured by subsequent descent. It was alleged that at Somerset two women were carried from a waggon over a wall, into an adjoining field. Within the same village a cellar door frame, with its doors bolted, was lifted, and then deposited on one side of its previous position; although situated to windward of the mansion to which it belonged. This result was the more striking, because, in consequence of their presenting an inclined plane to the blast, the doors and their frames would have been pressed more firmly upon their foundation by an ordinary wind. In consequence of the same dilatation of the air within the house, which lifted the cellar door, the weather-boarding on the leeward side was burst open, while that to the windward was undisturbed.

About four o'clock on the afternoon during which this tornado passed near Providence, there was heard at the farm at which I resided, twenty-five miles south of Providence and about fifteen miles from Somerset, the loudest thunder which I ever heard. It made the house in which I was tremble sensibly.

I have received from an estimable friend, Mr. Allen, a most interesting account of this tornado, which passed over the river, and there produced the appearance of a water-spout, while he was sufficiently near for accurate observation. In one respect his narrative tends to justify my opinion, that the exciting cause of tornadoes is electrical attraction. In two instances in which flashes of lightning proceeded from the water, Mr. Allen remarked that the effervescence produced by the tornado in the water very perceptibly subsided.[*]

[*] See Essay on the Cause of Tornadoes or Water-spouts in sixth vol. American Philosophical Transactions, or in Silliman's Journal, vol. 32, for 1837, and at p. 195, Vol. II. of these Annals.

" It was about three o'clock, P. M., during a violent shower, that I observed a peculiarly black cloud to form in the midst of light, fleecy clouds, and to assume a portentous appearance in the heavens, having a long, dark, tapering cone of vapour extending from it to the surface of the earth. The form of this black cloud, and of the cone of vapour depending from it so nearly resembled the engraved pictures of ' water-spouts' above the ocean, which I had frequently seen, that I should have come speedily to the conclusion that one of these ' water-spouts' was approaching, had I not been aware that this phenomenon occupied a space in the heavens directly above a dry plain of land. Whilst attentively watching the progress of the cloud, with its portentous dark cone trailing its point in contact with the surface of the earth, I noticed numerous black specks, resembling flocks of blackbirds on the wing, diverging from the under surface of the clouds, at a great elevation in the air, and falling to the ground. Among these were some objects of larger size, which I could discern to be fragments of boards, sailing off obliquely in their descent. This alarming indication left no room for doubt that a violent tornado was fast approaching, and that these distant, dark specks were fragments of shingles and boards uplifted high in the air, and left to fall, from the outer edge of the black conical cloud. This fearful appearance was repeatedly exhibited, as often as the tornado passed over buildings.

" The whirlwind soon swept towards an extensive range of buildings, within a few yards of me, the roof of which appeared to open at the top, and to be uplifted for a moment. The whole fabric then sunk into a confused mass of moving rubbish, and became indistinctly visible amid the cloud that overspread it, as with a mantle of mist.

" The destructive force of the tornado now became not only apparent to the eye, but also fearfully terrific, from the deafening crash of breaking boards and timbers, startling the amazed spectator in alarm for his personal safety, amid the roar of the whirlwind, and the shattered fragments flying like deadly missiles near him. At one instant, when the point of the dark cone of cloud passed over the prostrate wreck of the building, the fragments seemed to be upheaved, as if by the explosion of gunpowder, and I actually became intensely excited with the fear that the moving mass might direct its march toward the open area of the yard, to which I had resorted, after abandoning a building in which I had previously found shelter.

" Fortunately the course of the tornado was not over the building used as a depot by the Stonington Railroad Company in Providence, where there was a numerous assemblage of passengers awaiting the departure of the cars; otherwise several lives might have been lost.

" The most interesting appearance was exhibited when the tornado left the shore, and struck the surface of the adjacent river.   Being within a few yards of this spot, I had an opportunity of accurately noting the effects produced on the surface of the water.

" The circle formed by the tornado on the foaming water was about three hundred feet in diameter.   Within this circle the water appeared to be in commotion, like that in a huge boiling cauldron; and misty vapours, resembling steam, rapidly arose from the surface, and entering the whirling vortex, at times veiled from sight the centre of the circle, and the lower extremity of the overhanging cone of dark vapour. Amid all the agitation of the water and the air about it, this cone continued unbroken, although it swerved and swung around, with a movement resembling that of the trunk of an elephant whilst that animal is in the act of depressing it to the ground to pick up some minute object.   In truth, the tapering form, as well as the vibrating movements of the extremity of this cone of vapour, bore a striking resemblance to those of the trunk of that great animal.

" Whilst passing off over the water, a distant view of the cloud might have induced the spectator to compare its form to that of a huge umbrella suspended in the heavens, with the column of vapour representing the handle, descending and dipping into the foam of the billows.   The waves heaved and swelled, whenever the point of this cone passed over them, apparently as if some magical spell were acting upon them by the effect of enchantment.   *Twice I noticed a gleam of lightning, or of electric fluid, to dart through the column of vapour, which served as a conductor for it to ascend from the water to the cloud.   After the flash the foam of the water seemed immediately to diminish for a moment, as if the discharge of the electric fluid had served to calm the excitement on its agitated surface.*

" The progress of the tornado was nearly in a straight line, following the direction of the wind, with a velocity of perhaps eight or ten miles per hour.

" Near as I was to the exterior edge of the circle of the tornado, I felt no extraordinary gust of wind; but noticed that the breeze continued to blow uninterruptedly from the same quarter from which it prevailed before the tornado occurred.

" I also particularly observed that there was no perceptible increase of temperature of the air adjacent to the edge of the whirlwind, which might have caused an ascending current by a rarefaction of a portion of the atmosphere. After passing over the sheet of water, and gaining the shore, I observed the shingles and fragments of a barn to be elevated and dispersed high in the air ; and the dark cloud continued to maintain the same appearance which it at first presented, until it passed away beyond the scope of a distinct vision of its misty outlines.

" The above imperfect sketch can convey to your mind only a feeble impression of this exciting scene, which in passing before me excited just enough of terror to impart to the spectacle the most awful sense of the power, sublimity, and grandeur of the Almighty, as described in the glowing words of the Psalmist. ' He bowed the heavens also, and came down : and darkness was under his feet ; and he did fly upon the wings of the wind. He made darkness his secret place ; his pavilion round about him were dark waters and thick clouds of the skies.' "

XXVI. *On various Electrical Apparatus, &c. By* CHARLES BARKER, ESQ. *In a letter to the Editor.*

Sir,

Should the following remarks be worth a place in your next number, you will perhaps oblige me so far as to insert them, first :—

With regard to the Leyden Jar,

I have for some time past adopted a plan which answers well. I pass the tube, communicating with the inside coating, through a *glass* tube, extending above and below the cover about six inches. The cover is thus insulated from the inside coating ; whilst, at the same time, I have the advantage of the stability which it gives the wire, and also the exclusion of all dust. My battery will thus retain its charge for days : thanks, however, in some measure, to Dr. Arnott. Fig 7, Plate V, will show the plan.

The Universal Discharger.

I have made a little improvement in this instrument. As at present made you cannot introduce anything between its arms wider than the distance will admit of ; but I cause that part

of the stand, which supports the insulated discharging arms, to slide out as shown in fig 8.   It is very convenient in many respects.

The revolving Spotted Fly.

In Number 2 of the Annals you describe "a brilliant experiment for the lecture table," and which I find the opticians make for the moderate sum of five pounds. With the permission of my friend, Mr. Nicholson, of Fareham, I can describe a better one, and which may be made for less than as many pence, and which will work, I can confidently say, much better.   Mr. Nicholson invented it six or seven years ago : and I then mentioned it to the leading opticians in town : amongst whom I may mention Messrs Jones, of Holborn, who made it to my order for seven shillings.   I am thus particular because I had just seen a book called the " *Experimental Philosopher.*" in which the experiment is mentioned and claimed by the author, Mr. Mullinger Higgins, as his.   It is true that at page 319 of his work, he says, that it has been described to him as my invention. but this comes *after* the paragraph suggesting it as his own.   If the experiment is really original with him, it is certainly curious to remark the singular similarity between them.   When my friend. Mr. Nicholson, first made the experiment nearly, or quite, seven years ago, he placed it horizontally ; but when made vertically, it is seen much better on the lecture table, as shown in fig. 9 ; which will explain the way of showing it.   I may just observe that it works much better if placed on an *insulated stand* and thus driven by sparks instead of being directly connected with the conductor.   The same plan of the insulated stand, answers also much better for exhibiting the common electrical fly ; which if thus placed, and the *points greased,* as mentioned, I think, by Singer, forms a very pretty experiment, as you thus get a *dotted circle of light ;* and by varying the length of the sparks, you may diversify the experiment very well.   I can assure you it is well worth trying.   The instrument is represented by fig. 9.

Revolving Spiral Tube.

Fig. 10 represents my improved plan of the revolving spiral tube.   It is made of a glass tube blown nice and round at one end, and open at the other ; ten inches long, and three-quarters diameter.   A ball or a piece of smooth tinfoil is fixed at the upper closed end, and the usual spots of tinfoil carried in a spiral form to the lower open end.   A wooden ring is cemented on the outside of the lower end of the tube

and a strip of foil pasted round it. From this ring, and touching the tinfoil, four wires project outwards, having their points bent at right angles in the usual way. The tube is then set on an upright wire which passes upwards into the tube to its top, and this wire is then set on an insulated stand. It can thus revolve with great ease ; the expense is a mere trifle ; and it answers well.

The Spotted Jar.

In the Annals, for June, 1838, a correspondent asks " how is it when a jar is coated in the usual way on the inside, and spotted on the outside, you do not see the sparks on its outside when charging?" I have tried the experiment and can only say that, as I had anticipated, I did see the sparks passing as usual.

The Falling Star.

In the Annals, for April, 1837, a correspondent requests information as to this experiment. The principal requisites are to have the jar *intensely* charged, and the tube exhausted *to a certain extent.* My tube is six feet long, and four inches diameter, and I use a jar having five square feet of coating. A friend works the air pump, whilst I occasionally try to pass the charge down, when *at a certain degree of exhaustion* it does so, in a most brilliant line of white light. In order to avoid the injury of the sharpe edged cylindrical caps, which throw off the electric fluid, in this and similar electric experiments, I have all my apparatus fitted with wooden rings, which cover such edges.

On the identity between Electricity and the Aurora.

When electricity is passed down a perfectly exhausted tube it presents the appearance, and is a close imitation, of the aurora ; and is, I believe, supposed to be one and the same thing. As the aurora is only electricity on a larger scale, passing through an attenuated atmosphere, they should produce similar effects. Now, the aurora deflects the magnetic needle ; and the passage of electricity down a vacuum should do the same. I take a glass tube three feet long, and two inches diameter, mounted with caps and a valve, as usual, so that it may be exhausted. Round the centre of this tube, and ten inches from either end, I wind one hundred and eighty feet of covered copper wire ; the two final extremities of which are connected with a galvanometer : see fig. 11. The tube is insulated, and has one end connected with the outer coating of a battery, of fifty square feet ; and the other end, with an insulated discharger, by means of a long wet thread. The

battery being charged and the tube exhausted, I connect one end, by means of the wet thread, with the battery, for the space of five seconds, then remove for the same time, then again apply it, and so on in the manner mentioned by Dr. Faraday, in his Researches. The needle is soon deflected nearly 30°. I believe this is new.

Now, allow me to make a few remarks on the common electric machine. It is generally admitted that the plate is more powerful than the cylinder; but as the plate is generally inverted you cannot obtain the negative electricity, so that in this respect the cylinder is preferable. When, however, the plate is inverted on the plan of Mr. Harris, of Plymouth, you can readily obtain both positive and negative electricity. It is both powerful in action and elegant in appearance; and yet we see works on the subject constantly appearing, in which the writers say the best machine, &c. for the experimentalist, is the cylinder. My electrical machine, constructed by Watkins and Hill, is a double three feet plate on Harris's plan, and I can safely say that its effects are most powerful, far more than any I ever yet saw. One turn will give from fifteen to eighteen sparks one inch long, passing between balls one inch and a quarter diameter. It has four pair of rubbers; and I will venture to say, that no one will readily take two sparks from the conductor.

Can you inform me how to coat the inside of large carboys, so as to make them Leyden jars?*

<div style="text-align:right">Your obliged,
CHARLES BARKER.</div>

Gosport, July, 8, 18 39.

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* The method which I have employed to line narrow necked bottles with metal is similar to that of silvering the inside of glass globes for mirrors. Melt equal parts of lead and tin, and whilst fused add quicksilver enough to keep the whole fluid whilst warm; and in this condition pour it into the bottle or carboy, turning the latter round and round in various ways till the whole of the inside be covered with the amalgam.

A little bismuth added to the other metals keeps the whole fluid at a low temperature, which is an advantage. This is the best method with which I am acquainted. The bottles may be lined up to the orifice of the neck. Metallic filings never do well for lining an electrical jar. I think that I first saw this plan practised by Mr. Marsh. EDIT.

XXVII.   *Fine Arts.—The Daguerre Secret.*

At the weekly sitting of the Academy of Sciences, on Monday, Aug. 19, the process of M. Daguerre, for the formation of photogenic drawings, was, as had been previously announced, communicated to the public. From an early hour all the seats allotted to the public were occupied; and upwards of 200 persons, disappointed of gaining admission, were stationed in a crowd in a court of the Institute, and formed a kind of scientific *émeute.* Every body was anxious to hear *the* secret, every body to catch the *mot*; all were desirous of learning whether their own scientific conjectures would be confirmed or not.

At three o'clock, M. Arago commenced his explanation. We shall not attempt to follow the great *savant* through all the details of his long statement, a large proportion of which related to the history of the discovery,—a subject already pretty well known to the public: and we shall only mention as much of that part of it as belongs to Messrs. Niepce and Daguerre. In a similar manner, our account of the process itself will be brief: both because M. Arago was obliged to give only a *résumé* of it, and because it is announced that, in a few days' time the documents communicated by M. Daguerre, under seal to the Committee of the Deputies, on his grant, will be published, and the inventor himself will give a series of public representations of the actual performance of his method.

M. Arago, after alluding to the history of the camera obscura, originally discovered by Porta, a Neapolitan chemist, reminded the Academy that, as early as 1566, the influence of light on what the alchemists termed *lune* or *argent corné* (chlorure of silver), had been observed, and was mentioned in the work of Frabicius.   A Frenchman, named Charles, at the commencement of the present century, had made use of a sensitive paper for the tracing of outlines, by the action of light, but had died without leaving any account of his method. After this came the memoir of Wedgewood, of 1802, &c.; a part of the history of the discovery too well known to need repetition.   The late M. Niepce, M. Arago proceeded to state, was living near Châlons-sur-Saône, occupied in scientific pursuits, and appeared to have commenced his photographic experiments in 1814.   His first connexion with M. Daguerre commenced in 1826, when, through information received from an optician at Paris, he learned that this gentleman was engaged in an independent series of photogenic researches, and especially in trying to fix the images of the

camera obscura. They associated their labours in 1829, after
M. Niepce had visited London in 1828, and had presented
his memoir on his photogenic discoveries to the Royal Society.
It is proved that for the *photogràphic copying* of engravings,
and for the formation of plates for engravers, in an advanced
state of preparatory sketching of the subject, this gentleman
was, in 1826, possessed of the secret of making shades corres-
pond to shades, light parts to lights, demi-tints to demi-tints,
&c.; and that he also knew how to make his drawings so pro-
duced insensible to the ulterior action of light. The act of
partnership, drawn up between M. Diepce and M. Daguerre,
and which afterwards stood good between his son and the
latter gentleman, states that some entirely new methods had
been discovered by M. Daguerre, and that they had the ad-
vantage of being able to reproduce images from sixty to eighty
times more rapidly than by the processes previously adopted.

It appears that M. Niepce, in his own photographic
researches, had first made use of a sheet of silver covered with
the purest bitumen (called, in France, *Bitume de Judée*),
which had been previously dissolved in oil of lavender. He
used to heat this sheet of silver till the oil was completely
evaporated, and only a kind of whitish powder remainded ad-
hering to the surface. He then placed the sheets so prepared
in the focus of the camera obscura, and obtained the image of
the object ; but the trace was hardly visible. To obviate this
imperfection, M. Niepce next thought of plunging the sheet,
when removed from the camera obscura, into a mixture of oil
of lavender and oil of petroleum,—a method that succeeded
and augured still further improvement ; for the image became
visible like an ordinary engraving, and on washing the sheet
with distilled water, left the representation permanent. As
a further improvement of this, M. Niepce used a new mixture
of sulphuret of potassium and iodine; but the light acted very
slowly upon it, and the iodine, spreading itself over the surface,
rendered the image confused and obscure. The oil of petro-
leum, first used by this gentleman, was found to have the
property of attaching those points of the metallic surface,
which had been preserved from the action of light by the
shades, while it was of no effect on those parts touched by the
solar rays.

It was at this stage of the invention that M. Daguerre's
labours were joined to those of Niepce ; and it is only after a
long series of experiments, carried on with unwearied perse-
verance for many years, that, after M. Niepce's death, M.
Daguerre has at length resolved the problem to its present
extent. We omit the history of these experiments, and pass

on at once to the actual process as is now used by its author.

A sheet of copper, plated with silver, is washed carefully in a solution of nitric acid, which removes from it all the extraneous matters on its surface, and especially any traces of copper from the silver surface. A slight degree of friction is requisite in this process, but it must not be applied always in the same direction. M. Daguerre has observed, that with the friction used in a particular manner, a sheet of copper thus plated with silver answered better than a sheet of silver alone; and he infers from this, that voltaic agency is not unconnected with the effect. When the sheet is thus prepared, it is placed in a closed box and exposed to the vapour of iodine. This vapour is made to pass through a very fine sheet of gauze, to render its distribution more equable over the surface of the silver, and in order to effect this object (which is quite indispensable) more certainly, the sheet has a small metallic rim raised round all its edges. A thin coating, of a yellow colour, is thus formed on the surface of the sheet, which is estimated by M. Dumas at not more than the *millionth part of a millimetre* in thickness. The sheet, when covered with this substance, is of the most excessive sensibility to light; and is thus ready for the camera obscura. M. Daguerre, in the instrument which he uses, employs a piece of unpolished glass, which he brings first of all into the focus of the lens, in order to determine the exact point at which the sheet should be placed; and, as soon as this is determined, the sheet is placed accordingly. A few seconds, or minutes, according to the time of day, the state of the atmosphere, &c., suffice for forming the photogenic image; but it is hardly, if it all visible on the surface of the sheet. To make it so, the sheet is placed in another box, and exposed to the vapour of mercury, heated at 60° Reaumur, or 167° of Fahrenheit. One of the most curious circumstances attending this part of the process is, that the mercury must act at a certain angle. If the drawing is intended to be seen vertically, it must be suspended over the mercury at an angle of 45°; if it is to be seen at an angle of 45°, it must be suspended horizontally. On being taken out from the mercury-bath, the sheet is plunged into another bath of the hypo-sulphite of soda; this solution attacking the parts upon which the light has not been able to act, and respecting the light parts,—being the very inverse of the action of the mercury. It may be supposed, therefore, observed M. Arago, that the light parts of the image are formed by an amalgamation of mercury and silver, and the dark parts by a sulphuret of silver, at the expense of the hypo-sulphite of soda. M. Arago observed, that no satis-

factory reason had yet been given for this latter part of the
process. The sheet is finally washed into distilled water, and
the operation is terminated.

The drawing, thus obtained, is perfectly insensible to the
action of light, but it is liable to injury, just like a crayon or
pencil-drawing, and requires to be preserved under a glass.
The effect of the whole is miraculous: and as an instance, we
may mention that one of the drawings exhibited by M.
Daguerre, on Monday, was the view of a room with some rich
pieces of carpet in it; the *threads* of the carpet were given
with mathematical accuracy, and with a richness of effect that
was quite marvelous.

Such is the process of M. Daguerre: we must add, that it
is found that the sun-light does not act equally well at all
hours of the day, nor even when the sun is at equal heights
above the horizon. Thus the effect is produced better at ten
in the morning than at two in the afternoon: and hence the
Daguerrotype may be of immense value in measuring the
intensity of light.

The camera obscura, employed by M. Daguerre, may be
put in a box two feet long, two feet wide, and two and a half
feet high: the price of the whole apparatus may be from 400
francs to 420 francs: the price of each metallic sheet is about
three or four francs.

M. Daguerre has intrusted the manufacture and selling of
the apparatus, to the house of Alphonsa Giroux, in the Rue
de Coq, St. Honoré.

We need hardly say that the most enthusiastic cheers re-
sponded from the grave benches even of the academy, on the
termination of M. Arago's description; and the President,
M. Chevreul, complimented M. Daguerre in the warmest
terms.*—*Literary Gazette.*

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* It seems to us that, beautiful as this process is upon metallic
substances, much of the utility of photogenic copying will be lost,
unless the artist and traveller can use paper instead of copper,
silvered, iodined, and mercurialised. The images produced by M.
Daguerre are exquisitely correct, but gloomy-looking. They re-
semble moonlight pictures done in ink. We also hear from Paris,
that M. Collat has succeeded in his method of copying busts, statues,
or other solid objects, with mathematical precision. This is, per-
haps, as remarkable a discovery as the photogenic; and one that
may be applied to many valuable purposes.—*Ed. L. G.*

XXVIII. *Supplementary Note to* Dr. Faraday's *Eleventh Series of Experimental Researches.*

1307. I have recently put into an experimental form that general statement of the question of *specific inductive capacity* which is given at No. 1252 of Series XI, and the result is such as to lead me to hope that the Council will authorize its addition to the paper in the form of a supplementary note. Three circular brass plates, about five inches in diameter, were mounted side by side upon insulated pillars: the middle one, A, was a fixture, but the outer plates B and C were moveable on slides, so that all three could be brought with their sides almost into contact, or separated to any required distance. Two gold leaves were suspended in a glass jar from insulated wires: one of the outer plates B. was connected with one of the gold leaves, and the other outer plate with the other leaf. The outer plates B and C were adjusted at the distance of an inch and a quarter from the middle plate A, and the gold leaves were fixed at two inches apart*; A was then slightly charged with electricity, and the plates B and C, with their gold leaves, thrown out of insulation *at the same time*, and then left insulated. In this state of things A was charged positive inductrically, and B with C negative inducteously; the same dielectric, air, being in the two intervals, and the gold leaves hanging, of course, parallel to each other in a relatively unelectrified state. •

1308. A plate of shell lac, three quarters of an inch in thickness, and four inches square, suspended by clean white silk thread, was very carefully deprived of all charge (1203.), so that it produced no effect on the gold leaves if A were uncharged, and then introduced between plates A and B; the electric relation of the three plates was immediately altered, and the gold leaves attracted each other. On removing the shell lac this attraction ceased; on introducing it between A and C it was renewed; on removing it the attraction again ceased; and the shell lac when examined by a delicate Coulomb electrometer was still without charge.

1309. As A was positive, B and C were of course negative; but as the specific inductive capacity of shell lac is about thrice that of air (1270.), it was expected that when the lac was introduced between A and B, A would induce more towards B than towards C; and therefore B would become more negative than before towards A, and, consequently, because of its insulated condition, be positive externally, as at its back or at the gold leaves; whilst C would be less negative towards

A, and therefore negative outwards or at the gold leaves. This was found to be the case ; for on whichever side of A the shell lac was introduced the external plate at that side was positive, and the external plate on the other side negative towards each other, and also to uninsulated external bodies.

1310. On employing a plate of sulphur instead of shell lac, the same results were obtained ; consistent with the conclusions drawn regarding the high specific inductive capacity of that body already given (1276.).

1311. These effects of specific inductive capacity can be exalted in various ways, and it is this capability which makes the great value of the apparatus. Thus I introduced the shell lac between A and B, and then for a moment connected B and C, uninsulated them, and finally left them in the insulated state; the gold leaves were, of course, hanging parallel to each other. On removing the shell lac the gold leaves attracted each other ; on introducing the shell lac between A and C this attraction was increased (as had been anticipated from theory), and the leaves came together, though not more than four inches long, and hanging three inches apart.

1312. By simply bringing the gold leaves nearer to each other I was able to show the difference of specific inductive capacity when only thin plates of shell lac were used, the rest of the dielectric space being filled with air. By bringing B and C nearer to A, another great increase of sensibility was made. By enlarging the size of the plates still further power was gained. By diminishing the extent of the wires, &c., connected with the gold leaves, another improvement resulted. So that in fact the gold leaves became, in this manner, as delicate a test of *specific inductive action* as they are, in Bennet's and Singer's electrometers, of ordinary electrical charge.

1313. It is evident that by making the three plates the sides of cells, with proper precautions as regards insulation, &c., this apparatus may be used in the examination of gases, with far more effect than the former apparatus (1187. 1290.), and may, perhaps, bring out differences which have as yet escaped me (1292. 1293.).

1314. It is also evident that two metal plates are quite sufficient to form the instrument; the state of the single inducteous plate when the dielectric is changed, being examined either by bringing a body excited in a known manner towards its gold leaves, or, what I think will be better, employing a carrier ball in place of the leaf and examining that ball by the Coulomb electrometer (1180.). The inductive and inducteous surfaces may even be balls ; the latter

being itself the carrier ball of the Coulomb electrometer (1181. 1229.).

1315. To increase the effect, a small condenser may be used with great advantage. Thus if, when two inducteous plates are used, a little condenser were put in the place of the gold leaves, I have no doubt the three principal plates might be reduced to an inch or even half an inch in diameter. Even the gold leaves act on each other for the time as the plates of a condenser. If only two plates were used, by the proper application of the condenser, the same reduction might take place. This expectation is fully justified by an effect already observed and described (1229.).

Observations on Dr. Faraday's Eleventh Series.

Our readers are already aware that in former parts of these " Annals" we have had occasion to notice considerable errors which are observable in various series of Dr. Faraday's *Experimental Researches in Electricity :* and we are happy to learn that our remarks have been productive of considerable experimental enquiry (even amongst some of our author's particular friends), which is one of the great objects for which this periodical was first established : and certainly the only method of arriving at those important truths on which this branch of scientific knowledge must ultimately rest.

The grand theme of the Eleventh Series is *Induction,* which Dr. Faraday has attempted to investigate in a variety of capacities : but with what success these investigations have been attended our readers will necessarily form their own opinions ; our business being merely that of pointing out a few particulars essentially connected with the subject : and showing their bearing on one another, and on the theories they are intended to support.

Respecting electro-decompositions, Dr. Faraday supposes that the "*first* step is an electro-polarization of the compound particles operated on, and *decomposition* the second" step in the process (1164). To this doctrine we can have no objections whatever, because we have entertained the same idea ourselves several years ago, as may be understood by consulting page 19, Vol. I, of these Annals : nor could we have any objections to the idea of polarizing contiguous particles of atmospheric air or of specific induction, provided we were favoured with any experiments in support of it. Dr. Faraday's cage experiments (1173) have thrown no light on either of these points, nor have they proved any thing not previously known.

The experiment which Dr. Faraday made with the electrometer in the cage (1174), was obviously a repetition of one of those very interesting experiments which Beccaria made with his "*electrical well*"* more than seventy years ago: the "well" in one case being, *virtually*, the cage in the other: and the "delicate gold leaf electrometer" of Dr. Faraday, the *scrutator* of the illustrious Italian. As far as Dr. Faraday pursued Beccaria's experiments, the results were the same; but when, "by working the machine, the air within this chamber could be brought into what is considered a highly electrified state;" and that, "every attempt to charge air bodily....failed (1173)," Dr. Faraday's experiments became truly original, and the results of a very different character to any thing ever obtained by Beccaria. Nor do we remember that our Italian philosopher has any where inferred that, "if any portion of the air was electrified, as glass or other insulators may be charged, it was accompanied by a corresponding *opposite* action *within* the cube (1173):" from experiments in which, "neither during the charge or after the discharge did the electrometer or air within (the cube) show the least sign of electricity (1174)."

Dr. Faraday *living in the cage* was the very worst situation he could possibly have chosen to *live in* whilst attempting to ascertain the electrical condition of its contents: for being surrounded by equal electrical forces he had no means of discovering any of them. An electrometer placed in the centre of force of an highly electrized cloud, would not indicate electric action, because of the surrounding forces balancing one another. If Dr. Faraday, whilst in the cage, had imitated the dwarf in the lantern, by holding his electrometer out at the door, in the manner which the little man holds out his bell, he would have found very different results to those he obtained *within*.

We do not mean to follow Dr. Faraday through all his evolutions of *induction*, as our principal object is to place those *experiments* on which the theory depends in a proper light before our readers. The globe experiments commencing at paragraph 1208, are exceedingly ingenious; and if they could be repeated, in other hands, with the same exactitude of results as represented by Dr. Faraday's figures, they would certainly be of great interest. In the first experiment we find the original charge of app. 1 to be 254°: but on a second trial it was reduced to 250°: and that the sum total of the divisions of the charge was $122+124+2+1=249°$: or just

* Beccaria's Artificial Electricity. London, Quarto Edition, p. 188.; also Cavallo's Electricity, Second Editon, p. 196.

one degree less than the whole charge before division. Now
we are told in paragraph 1250, that six minutes are necessary
to pass through the manipulation, after the first charge is
taken : " three minutes pass between the first charge of app.
1 and the division ; and three minutes between the division and
the discharge, when the force of the non-transferable electri-
city is measured." Now it cannot be doubted that the same
space of time would be required to ascertain the *latter* state
of the whole charge as would be required for the *first*. But
during the time the *first* state was measured, there occurred
a loss of 4°, and, consequently, a considerable loss would occur
during the time occupied in measuring the *latter* state of the
whole charge in app. 1 before the division was made. The
words " divided and instantly taken" do not imply that the
division was made at the instant the carrier left app. 1, for
to ascertain the *latter* state of the whole charge at the elec-
trometer. The division took place after the carrier had
returned a second time from the electrometer ; which must
necessarily have been the case ; for we find (1207), that
the carrier was back again " touching one of the balls" of the
apparatus at the time the division was made. Under these
circumstances then, if 4° were lost during the *first* measure-
ment, of the whole charge of app. 1, it is fair to infer that 3°,
at least, would be lost during the second measurement.
Therefore the sum total of the *divisions* of the charge as
stated (1208), would be 2° greater than the whole charge
itself. Moreover, as three minutes elapsed in measuring the
divisions, there would still be another loss to be brought into
account : and as there would also remain in the two globes
residual charges which the carrier could not take away, the
sum of the *parts* of the charge would obviously appear con-
siderably greater than the original charge itself; showing
pretty clearly that either the apparatus were very unfit for the
enquiry, or that the experiments were very inaccurately per-
formed. Dr. Faraday, however, has squared his figures
pretty well to make " both ends meet," without noticing these
losses and residuals of charge which ought to have been
accounted for.

Having given one specimen of the globe experiments we must
leave our readers to judge for themselves of the rest of them.
We shall not proceed with our observations on " induction in
curved lines (1215)" at present ; but shall place before our
readers the *origin* of the experiments and the explanations of
the phenomena given by their authors, in our next number.

EDIT.

XXIX. *On a phenomenon observed while experimenting with a Wollaston's battery of a large size. By* E. Lenz.

It is well known that, in accordance with Ampère's theory, two succeeding particles of one and the same galvanic current mutually repel each other, and that Ampère bore out this deduction from his theoretical views by an experiment we find cited in all our manuals of electro-magnetism, namely, that a duly bent conducting wire resting on two disconnected surfaces of quicksilver acquires a sailing motion the instant these surfaces are brought into connexion with the polar wires of a galvanic battery.

Some time ago when at Professsor Jacobi's I witnessed a phenomenon evidently identical with that I have just mentioned, but of a remarkable degree of intensity, The battery consisted of twelve pairs arranged as has been stated, and each presenting (taking only one side into account) a surface of three square feet of zinc. The plates were fastened to a frame; and the troughs, which were all filled with a very active mixture of diluted sulphuric and nitric acids, were so placed on a board that by means of a winch and wheel-work they could be raised up to them. The separate plates were so connected as to form a continuous whole by means of thick copper wires of this shape ⌐‾‾‾⌐ , their legs dipping down into mercury cups that were screwed on to the zinc and copper plates.

The action of this battery was so energetic that it at first raised to a white heat, and then melted through in the middle, a platina wire 0,125 inch thick, and $3\frac{1}{2}$ inches long; that is to say, the length of the connecting copper wires.

The most remarkable phenomena that we observed with this battery was however the following, namely, that on completing the circuit in itself by means of the copper connecting wires only and then raising up the troughs, as soon as the greater part of the surface of the plates became immersed THE WHOLE OF THE CONNECTING WIRES SPRUNG OUT OF THE MERCURY CUPS WITH A LOUD CRACKLING NOISE. We can, it is clear, only explain how the wires are thus thrown out by means of the repulsion we have alluded to, and which is here called forth at the places where the particles of the current in immediate succession pass out of the mercury into the connecting wire, and from this again into the mercury of the next pair of plates. It is however remarkable that this power of repulsion should be strong enough to heave up wires, each of which weighs 210 grains.

The like was also observed when the battery did not con-
sist of the whole of the twelve pairs in connexion, but only of
some of them ; this, however, is in strict accordance with
Ohm's law, according to which the strength of a current is
the same, be the number of the elements composing the bat-
tery what it may, provided it is not interrupted by the inser-
tion of any foreign conductor.
                JULIAN GUGGSWORTH.
Wormwood Scrubbs,
September 11, 1839.

XXX.   *Supplementary Note to Article* XVI. *p.* 190.

The lightning which fell on the *Royal Hotel of Invalides,*
on the 8th of June last (see p. 215), has produced effects from
lateral explosions much greater than any I ever heard of before,
and highly corroborative of the inferences I have drawn res-
pecting the "probable effects" which similar explosions would
have on Mr. Harris's conductors (206). The projecting of
the nails from the lead on the lantern, the lifting up of that
great mass of lead, and the entire removal of " the lead sur-
rounding the base of one of the columns of the lantern ", are
ample proofs of the astonishing effects of lateral explosions :
and certainly the best data which could be furnished for sup-
posing that Mr. Harris's conductors are liable to be peeled
from the masts by a similar kind of action.

I have already mentioned that lateral discharges were ex-
tensively studied by Viscount Mahon; but, perhaps, the experi-
ments of Professor Henry, of New Jersey College, in the
United States, would be more to my purpose. This philosopher
informed the British Association at the Liverpool meeting,
that he had taken sparks from various parts of a lightning rod
erected upon the best principle, and well connected with the
ground, when that rod was carrying a discharge of electricity;
and stated that he believed that no rod could be sufficiently
uninsulated as not to produce lateral discharges.

Since my fourth memoir was printed I have been favoured
with the opinions of several naval officers respecting my plan
of protecting shipping from lightning ; and having availed
myself of some valuable suggestions as to the probability of
those branch conductors which I had proposed to be placed
before the shrouds of each mast, being liable to injury by
pressure from the yards when the ship is close hauled, *I have*
dispensed with them altogether. The system will thus be
much simplified, and give an opportunity of placing the whole
of the metal in those conductors which are *aft* the shrouds,

and entirely away from the masts. This plan will also remove every apprehension which might have been entertained of the shrouds being ignited in consequence of lightning heating those conductors which were in partial contact with them.

Fig. 8, Plate IV. represents a vertical section of the *starboard* and *larboard* conductors of each mast from the topgallant mast head to the copper sheathing of the vessel; and the darts show the manner in which a flash of lightning would be conducted down both sides of the rigging to the sea. Should a flash of lightning happen to strike either of the topgallant mast branches, then, because of their metallic union above, at the mast head, the lightning would be distributed through both branches, as decidedly as if it had struck the spindle above the truck. The topmast conductors being also metallically connected at the crosstrees would each conduct its own share of any flash of lightning which should happen to strike on either the starboard or the larboard branch. The lower mast conductors would also reciprocally and mutually assist in conducting lightning which should strike either of them. Hence, therefore, on whatever part of a conductor lightning should strike, it would invariably find two conducting channels to the sea, one on each side of the ship.

Fig. 9, gives a side view of the starboard and forestay conductors of a three masted vessel; also a view of the main topmast and mizen topmast stay conductors, which might be applied if thought necessary. These latter would be the means of uniting all the conductors into one system from the topmasts' heads downwards to the sheathing. By this means every flash of lightning striking any branch conductor of the system would find its way to the sea through seven conducting channels, three on each side of the ship and one from the fore stay conductor which terminates under the bows. The small darts in fig. 9 show the distribution of the electric fluid from a flash of lightning supposed to strike the main-topgallant mast head, or the vane spindle above it. Lightning thus extensively distributed amongst distant branches of the conducting system would be rendered perfectly harmless.

The fastenings once completed, the conductors could at any time be put up in a few hours, and remain stationary during a cruise or a voyage, as decidedly as the standing rigging.

Mr. Harris's answer to my letter of September 12, is dated " Plymouth, September 15th," in which he informs me that he was Secretary of the Physical Section of the British Association, at Birmingham, but did not see or hear of my papers.

W. S.

XXXI. *Reviews and Notices of New Books.*

The Universal Ready Reckoner, or Royal Road to Arith-metic ; being instructions in the use of the Sliding-Rule : compiled for the use of Ladies and idle Gentlemen. By an IDLE GENTLEMAN. JOHN LEE, 440, *West Strand, near the Lowther Arcade.*

The author of this little work is, in our estimation, so far from deserving the title which he has given himself, that we consider him a very industrious and useful man : and we heartily recommend the *Royal Road to Arithmetic* to that class of gentlemen to whom it is addressed ; not for the purpose of indulging in their idleness, but as an example to induce them to bring their talent into active and useful play, and devote their time to those mental pursuits which are the strong bulwarks against the invasions of lethargy and vice, and the sure means of becoming benefactors to mankind, and of attaining true honour and lasting reputation.

"The Rule is justly described as most useful to those engaged in long calculations, however skilful they may be in arithmetic, both as saving much time in many cases, and more especially in acting as a check to detect and remedy errors which are always liable to creep into the calculations of the most skilful, and which if undetected, will vitiate a whole process.

" Its great use, however, is to the unskilful, and especially in the facility it gives in multiplying and dividing fractional and mixed numbers, in measuring the surfaces and solidity of various things : in short, its acting as a complete universal Ready-reckoner, with little more trouble than is necessary to understand a printed ready reckoner for one thing only. Moreover, nobody can make themselves tolerably *au fait* at the Rule without at the same time improving themselves, if ignorant or deficient, very much in the science of arithmetic. The time and trouble it saves and the accuracy it gives, and the power of doing things which would have been deemed by most people utterly hopeless, are beyond all praise.

"I trust, therefore, that old as the rule may be, considering how little it is known in the fashionable world, I am not over presumptuous in hoping, I may have done some service in calling afresh the attention of individuals to it. The first part *of the book* is addressed to the most ignorant and most idle, whom, by restricting myself to the very simplest directions, I have endeavoured not to disgust or confuse. In the second I have entered more into detail, and given such reasons and explanations as may satisfy laudable curiosity and give some useful instruction apart from the actual use of the instrument."

We have perused this little volume with great pleasure, and consider it our duty to recommend it to every class of professional men, and artificers, who are desirous of availing themselves of the assistance of the sliding Rule.

XXXII. MISCELLANEOUS ARTICLES.

Mr. Leeson, who has succeeded Mr. Phillips to the lecture-ship at St. Thomas's Hospital, has lately been engaged in a series of experiments for the purpose of ascertaining the amount of pressure which would stop the electro-chemical decomposition of water by voltaic electricity of a certain tension; and has discovered that about 50 atmospheres is capable of preventing decomposition from an electric force of a voltaic series of about 20 pairs. Mr. Leeson has employed cylinders of glass about 3 inches long and nearly 4 inches diameter, with a hollow axis the diameter of which is about $\frac{3}{4}$ of an inch, leaving the solid glass of more than an inch and a-half thick. The ends of the cylinder are ground perfectly flat and smooth, and are covered with flat iron plates of considerable thickness, also ground flat and smooth, in order to form air-tight joints with the two ends of the glass cylinder. The cylinder, being placed on one of the iron plates, is filled with water, and the other plate is then placed on the other end; and both plates kept firmly in their places by bolts and screw nuts which hold all together. It is to be understood that there is a contrivance for fixing two strips of platinum within the cavity of the cylinder, and for connecting them with the poles of the battery employed. Having thus described the mode in which Mr. Leeson proceeds with his interesting experiments, it remains that we inform our readers, that we have seen the fragments of one of those glass cylinders which was split by the internal pressure of the liberated gases from the decomposition of water. The broken cylinder which we saw in Mr. Leeson's laboratory, was at least an inch and a-half thick of solid glass; which seems to have broken by a pressure of a little more than 50 atmospheres. EDIT.

On a remarkable property of Electric Tension.

M. C. Doppler, Professor of Mathematics, at Prague, has said that he has discovered that a feeble electric charge given to a bar of insulated metal causes it to contract; and, of course, if negatively electrized, the metal expands. The following is the method employed in the experiment :—

A thin copper tube, about three feet long, is properly placed in a delicate compound lever-apparatus, something similar to our lever-pyrometers, and properly insulated by glass stems. If now, a feeble electric tension be given to the tube, the index of the lever-apparatus immediately begins to move, indi-

cating a *contraction* of the tube. By increasing the *tension* the motion of the index is so swift that the eye cannot trace it ; so that for high electric tensions a simple lever-apparatus answers better than the delicate compound one. When sparks are either intentionally taken from the tube or that they are discharged spontaneously, the index rapidly recoils, indicating an *expansion* (prolongation) of the tube ; which *contracts* again by an increase of electric tension. This property of electricity is said to have been discovered by a single pair of plates ; and, as Poggendorff has justly said, " probably by an electric current," which would render the circumstance still more curious, unless the current produced a depression of temperature in the tube. *From Poggendorff's Annalen der Physik und Chemie.* EDIT.

———

Mr. Mason, of High Holborn, who is an exceedingly dexterous experimenter, has lately discovered a method of enhancing the beauty and splendour of charcoal deflagrations by voltaic electricity. He powders bichromate of potash and places the powder on the points of the charcoal, and when the latter has become red hot, the bichromate also enters into deflagration. The light produced is two-fold that given by the charcoal alone. The potash becomes decomposed and the liberated potassium scintilates in a very beautiful manner. We have seen Mr. Mason produce a beautiful effect with a battery of only thirty small jars. EDIT.

———

Speaking of deflagrations, perhaps it may be interesting to some of our readers to know that the laminated metals, such as gold, silver, or copper leaf, &c., give a good effect when deflagrated on a strip of bright tin plate, which may be connected with one pole of the battery, whilst the metallic leaf, placed on a long copper wire in connexion with the other pole, is passed gently over its surface. Gold gives the feeblest light, copper next. The light from white Dutch metal is much more brilliant, and of a pale purple tinge. That from silver is exceedingly handsome, being a pale greenish yellow. We have, for many years, used zinc turnings for deflagrating at the lecture table. They burst into a complete blaze, and become converted into *philosophical wool*, which floats in the air for a long time afterwards. EDIT.

My Dear Sir,

I have received your very interesting paper on successions, or series of electric currents, with which I am quite delighted. You will perceive, by looking at page 112 of vol. 2 of the Annals, that I had proceeded as far as tertiary currents, by means of the Magnetic Electrical Machine; but you have gone much farther, and the results of your experiments are really beautiful. I shall place them before our English experimenters in the January number of the Annals.

My principal object for making this letter public is, that of giving philosophers an opportunity of testing my theory of magnetic electricity, as given in vol. 1. of the "Annals of Electricity, &c." with your extensive series of successive currents You have given a series of symbols expressive of the direction of the currents to the fifth degree, by *opening the circuit.* Now, if my theory be correct, I can predict the direction of the currents which would be brought into play by *closing the circuit*, which series will be very different to that given by *opening the circuit.* The two will stand as follows:—

	On opening the Circuit.	On closing the Circuit.
Primary Current	...... +	+
Secondary Current	.... +	—
Current of the 3d order	—	+
Ditto, 4th order	+	— •
Ditto 5th order	—	+
	&c.	&c.

I have not tried the experiment, but I have no doubt of your finding the currents as I have predicted from an application of the principles of the theory.

<div align="right">

I am, my dear Sir,
Yours very truly,
W. STURGEON.

</div>

Joseph Henry, Esq. L.L.D.
    Prof. of Nat. Philosophy,
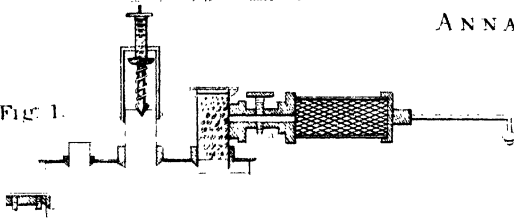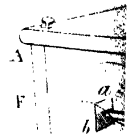In the College of New Jersey,
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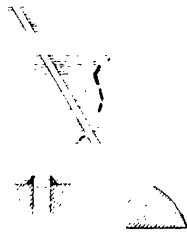
Fig. 1.

Fig. 4.
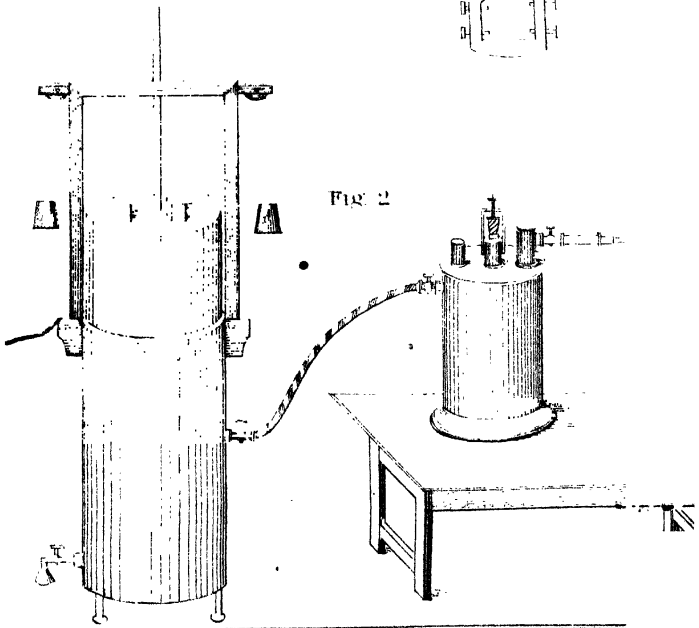
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Fig. 5.   Fig. 7.

Fig. 2

Further researches on the Magnetic Phenomena produced by Electricity : with some new experiments on the Properties of Electrified Bodies in their relation to Conducting Powers and Temperature. By Sir HUMPHRY DAVY, BART., P.R.S.

From PHIL. TRANS. for 1821.

Read July 15th, 1821.

I. In my letter to Dr. Wollaston on the new facts discovered by M. Ærsted, which the society has done me the honour to publish, I mentioned that I was not able to render a bar of steel magnetic by transmitting the electrical discharge across it through a tube filled with sulphuric acid ; and I have likewise mentioned, that the electrical discharge passed across a piece of steel through air, rendered it less magnetic than when passed through a metallic wire ; and I attributed the first circumstance to the sulphuric acid being too bad a conductor to transmit a sufficient quantity of electricity for the effect, and the second, to electricity passing through air in a more diffused state than through metals.

To gain some distinct knowledge on the relation of the different conductors to the magnetism produced by electricity, I instituted a series of experiments, which led to very decisive results, and confirmed my first views.

II. I found that the magnetic phenomena were precisely the same, whether the electricity was small in quantity, and passing

through good conductors of considerable magnitude; or, whether the conductors were so imperfect as to convey only a small quantity of electricity; and in both cases they were neither attractive of each other, nor of iron filings, and not affected by the magnet; and the only proof of their being magnetic, was their causing a certain small deviation of the magnetized needle.

Thus, a large piece of charcoal placed in the circuit of a very powerful battery, being a very bad conductor compared with the metals, would not affect the compass needle at all, unless it had a very large contact with the metallic part of the circuit; and if a small wire was made to touch it in the circuit only in a few points, that wire did not gain the power of attracting iron filings; though, when it was made to touch a surface of platinum foil coiled round the end of the charcoal, a slight effect of this kind was produced. And in a similar manner fused hydrate of potassa, one of the best of imperfect conductors, could never be made to exert any attractive force on iron filings, nor could the smallest filaments of cotton moistened by solution of hydrate of potassa, placed in the circuit, be made to move by · the magnet; nor did steel needles floating on cork on an electrized solution of this kind, placed in the voltaic circuit, gain any polarity: and the only proof of the magnetic powers of electricity passing through such a fluid, was afforded by its effect upon the magnetized needle, when the metallic surfaces, plunged in the fluid, were of considerable extent. That the mobility of the parts of fluids did not interfere with their magnetic powers as developed by electricity, I proved, by electrifying mercury, and Newton's metal fused, in small tubes. These tubes, placed in a proper voltaic circuit, attracted iron filings, and gave magnetic powers to needles: nor did any agitation of the mercury or metal within, either in consequence of mechanical motion or heat, alter or suspend their polarity.

III. Imperfect conducting fluids do not give polarity to steel when electricity is passed through them; but electricity passed through air produces this effect. Reasoning on this phenomenon, and on the extreme mobility of the particles of air, I conclude, as M. Arago had likewise done from other considerations, that the voltaic current in air would be affected by the magnet. I failed in my first trial, which I have referred to in a note to my former paper, and in other trials made since by using too weak a magnet; but I have lately had complete success: and the experiment exhibits a very striking phenomenon.

Mr. Pepys having had the goodness to charge the great battery of the London Institution, consisting of two thousand double plates of zinc and copper, with a mixture of 1168 parts of water, 108 parts of nitrous acid, and 25 parts of sulphuric acid, the poles were connected by charcoal, so as as to make an

arc, or column of electrical light, varying in length from one to four inches, according to the state of rarefaction of the atmosphere in which it was produced ; and a powerful magnet being presented to this arc, or column, was attracted or repelled with a rotatory motion, or made to revolve, by placing the poles in different positions, according to the same law as the electrified cylinders of platinum described in my last paper, being repelled when the negative pole was on the right hand by the north pole of the magnet, and attracted by the south pole, and *vice versa.*

It was proved by several experiments that the motion depended entirely upon the magnetism, and not upon the electrical inductive power of the magnet, for masses of soft iron, or of other metals, produced no effect.

The electrical arc or column of flame, was more easily affected by the magnet, and its motion was more rapid when it passed through dense than through rarefied air : and in this case, the conducting medium or chain of æriform particles was much shorter.

I tried to get similar results with currents of common electricity sent through flame, and in vacuo. * They were always affected by the magnet : but it was not possible to obtain so decided a result as with voltaic electrity, because the magnet itself became electrical by induction, and that whether it was insulated or connected with the ground. *

IV. Metals, it is well known, readily transmit large quantities of electricity : and the obvious limit to the quantity which they are capable of transmitting seems to be their fusibility, or volatilization by the heat which electricity produces in its passage through bodies.

Now I had found in several experiments, that the intensity of this heat was connected with the nature of the medium by which the body was surrounded : thus a wire of platinum which was readily fused by transmitting the charge from a voltaic battery in the exhausted receiver of an air pump, acquired in air a much lower degree of temperature. Reasoning on this circumstance, it occurred to me, that by placing wires in a medium much denser than air, such as ether, alcohol, oils, or water, I might enable them to transmit a much higher charge of electricity than they could convey without being destroyed in air : and thus not only gain some new result as to the magnetic states of such wires, but, likewise, perhaps, determine the actual limits

* I made several experiments on the effects of currents of electricity simultaneously passing through air in different states of rarefaction in the same and different directions, both from the voltaic and common electrical batteries, but I could not establish the fact of their magnetic attraction or repulsion with regard to each other, which probably was owing to the impossibility of bringing them sufficiently near.

to the powers of different bodies to conduct electricity, and the relations of these powers.

A wire of platinum of $\frac{1}{220}$, of three inches in length, was fused in air, by being made to transmit the electricity of two batteries of ten zinc plates of four inches with double coppers, strongly charged: a similar wire was placed in sulphuric ether, and the charge transmitted through it. It became surrounded by globules of gas: but no other change took place: and in this situation it bore the discharge from twelve batteries of the same kind, exhibiting the same phenomena. When only about an inch of it was heated by this high power in ether, it made the ether boil, and became white hot under the globules of the vapour, and then rapidly decomposed the ether, but it did not fuse. When oil or water was substituted for the ether, the length of the wire remaining the same, it was partially covered with small globules of gas, but did not become red hot.

On trying the magnetic powers of this wire in water, they were found to be very great, and the quantity of iron filings that it attracted, was such as to form a cylinder round it of nearly the tenth of an inch diameter.

To ascertain whether short lengths of fine wire, prevented from fusing by being kept cool, transmitted the whole electricity of powerful voltaic batteries, I made a second independent circuit from the ends of the battery with silver wires in water, so that the chemical decomposition of the water indicated a residuum of electricity in the battery. Operating in this way, I found that an inch of wire of platinum of $\frac{1}{220}$, kept cool by water left a great residual charge of electricity in a combination of twelve batteries of the same kind as those above mentioned: and after making several trials, I found that it was barely adequate to discharge six batteries.

V. Having determined that there was a *limit* to the quantity of electricity which wires are capable of transmitting, it became easy to institute experiments on the different conducting powers of different metallic substances, and on the relation of this power to the temperature, mass, surface, or length of the conducting body, and to the conditions of electro-magnetic action.

These experiments were made as nearly as possible under the same circumstances, the same connecting copper wires being used in all cases, their diameter being more than one-tenth of an inch, and the contact being always preserved perfect; and the part of the same solutions of acid and water were employed in the different batteries, and the same silver wires and broken circuit with water were employed in the different trials: and when no globules of gas were observed upon the negative silver wire of the second circuit, it was concluded that the metallic con-

ducting chain, or the primary circuit, was adequate to the discharge of the combination. To describe more minutely all the precautions observed, would be tedious to those persons who are accustomed to experiments with the voltaic apparatus, and unintelligible to others; and after all, in researches of this nature, it is imposible to gain more than approximations to true results; for the gas disengaged upon the plates, the different distances of the connecting plates, and the slight difference of time in making the connections, all interfere with their perfect accuracy.

The most remarkable result that I obtained by these researches, and which I shall mention first, as it influences all the others, was, that *the conducting power of metallic bodies varied with the temperature, and was lower in some inverse ratio as the temperature was higher.*

Thus a wire of platinum of 1-220, and three inches in length, when kept cool by oil, discharged the electricity of two batteries, or of twenty double plates; but when suffered to be heated by exposure to air, it barely discharged one battery.

Whether the heat was occasioned by the electricity, or applied to it from some other source, the effect was the same. Thus a wire of platinum, of such length and diameter as to discharge a combination without being considerably heated; when the flame of a spirit lamp was applied to it so as to make a part of it red hot, lost its power of discharging the whole electricity of the battery, as was shown by the disengagement of abundance of gas in the secondary circuit: which disengagement ceased as soon as the source of heat was withdrawn.

There are several modes of exhibiting this fact, so as to produce effects which, till they are witnessed, must almost appear impossible. Thus, let a fine wire of platinum of four or five inches in length be placed in a voltaic circuit, so that the electricity passing through it may heat the whole of it to redness, and let the flame of a spirit lamp be applied to any part of it, so as to heat that part to whiteness, the rest of the wire will become cooled below the point of visible ignition. For the converse of the experiment, let a piece of ice or a stream of cold air be applied to a part of the wire; the other parts will immediately become much hotter, and from a red will rise to a white heat. The quantity of electricity that can pass through that part of the wire submitted to the changes of temperature, is so much smaller when it is hot than when it is cold, that the absolute temperature of the whole wire is diminished by heating a part of it, and, *vice versa,* increased by cooling a part of it.

In comparing the conducting powers of different metals, I found much greater difference than I had expected. Thus six inches of silver wire 1-220 discharged the whole of the electricity of sixty-five pair of plates of zinc and double copper made

active by a mixture of about one part of nitric acid of commerce, and fifteen parts of water. Six inches of copper wire of the same diameter discharged the electricity of fifty-six pairs of the same combination, six inches of tin of the same diameter carried off that of twelve only, the same quantity of wire of platinum that of eleven, and of iron that of nine,   Six inches of wire of lead 1-220 seemed equal in their conducting power to the same length of copper wire of 1-220.   All the wires were kept as cool as possible by immersion in a basin of water.

I made a number of experiments of the same kind, but the results were never precisely alike, though they sometime approached very near each other.   When the batteries were highly charged, so that the intensity of the electricity was higher, the differences were less between the best and worst conductors, and they were greater when the charge was extremely feeble. Thus, with a fresh charge of about one part of nitric acid, and nine parts of water, wires of 1-220 of silver and platinum five inches long, discharged respectively the electricity of thirty, and seven double plates.

Finding that when different portions of the same wire plunged in a non-conducting fluid were connected with different parts of the same battery equally charged, their conducting powers appeared in the inverse ratio of their lengths: so, when six inches of wire of platinum 1-220 discharged the electricity of ten double plates, three inches discharged that of twenty, $1\frac{1}{2}$ inch that of forty, and one inch that of sixty : it occurred to me that the conducting powers of the different metals might become easily compared in this way, as it would be possible to make the contacts in less time than when the batteries were charged, and consequently with less variation in the charge.

Operating in this way, I ascertained that in discharging the electricity of sixty pairs of plates, one inch of platinum was equal to about six inches of silver, to $5\frac{1}{2}$ inches of copper, to 4 of gold, to 3-8 of lead, to about 9-10 of palladium, and 8-10 of iron, all the metals being in a cooling fluid medium.

I found, as might have been expected, that the conducting power of a wire for electricity, in batteries of the size and number of plates just described, was nearly directly as their mass : thus, when a certain length of wire of platinum discharged one battery,[*] the same length of wire of six times the weight, discharged six batteries ; and the effect was exactly the same, provided the wires were kept cool, whether the mass was a single wire, or composed of six of the smaller wires in contact with each other.   This result alone showed, that the surface had no relation to conducting power, at least, for electricity of this kind, and it was more distinctly proved by a direct experiment : equal

* A foot of this wire weighed 1·13 grains, a foot of the other 6·7 grains.

lengths and equal weights of wire of platinum, one round and one flattened by being passed transversely through rollers so as to have six or seven times the surface, were compared as to conducting powers; the flattened wire was the best conductor in air from its greater cooling powers, but in water no difference could be perceived between them.

VI. I tried to make a comparison between the conducting powers of fluid menstrua and charcoal and those of metals. Six inches of platinum foil, an inch and one-fifth broad, were placed in a vessel which could be filled with any saline solution: and a similar piece of platinum placed opposite at an inch distance; the whole was then made part of a voltaic circuit, which had likewise another termination by silver wires in water: and solution of salts added, till gas ceased to be liberated from the negative silver wire. In several trials of this kind it was found that the whole of the surface of six inches, even with the strongest solutions of common salt, was insufficient to carry off the electricity of even two pairs of plates; and a strong solution of potassa carried off the electricity of three pair of plates only; whereas an inch of wire of platinum of 1-220 (as has been stated) carried off all the electricity of sixty pair of plates. The gas liberated upon the surface of metals when they are placed in fluids, renders it impossible to gain accurate results: but the conducting power of the best fluid conductor, it seems probable from these experiments, must be some hundreds of thousand times less than those of the worst metallic conductors.

A piece of well-burnt compact boxwood charcoal was placed in the circuit, being 3-10th of an inch wide by 1-10th thick, and connected with large surfaces of platinum. It was found that one inch and 9-10th carried off the same quantity of electricity as six inches of wire of platinum of 1-220.

VII. I made some experiments with the hope of ascertaining the exact change of ratio of the conducting powers dependent upon the change of the intensity and quantity of electricity; but I did not succeed in gaining any other than the general result, that the higher the intensity of the electricity, the less difficulty it had in passing through bad conductors: and several remarkable phenomena depended upon this circumstance.

Thus, in a battery when the quantity of electricity is very great and the intensity very low, such as are composed of plates of zinc and copper, so arranged as to act only as single plates of from twenty to thirty-feet of surface each, and charged by a weak mixture of acid and water. Charcoal made to touch only in a few points, is almost as much an insulating body as water, and cannot be ignited, nor can wires of platinum be heated when their diameter is less than 1-80th of an inch, and their length three or four feet; aud a foot of platinum wire of 1-30 is scarcely heated by such a battery, whilst the same length of

silver wire of the same diameter is made red hot : and the same lengths of thicker wires of platinum or iron are intensely heated.

The heat produced where electricity of considerable intensity is passed through conductors, must always interfere with the exact knowledge of the changes of their conducting powers, as is proved by the following experiment :—A battery of twenty pair of plates of zinc, and copper plates ten inches by six, was very highly charged with a mixture of vitriolic acid and water, so as to exhibit a considerable intensity of electrical action, and the relative conducting process of silver and platinum in air and water ascertained by means of it. In air, six inches of wire of platinum of one-eightieth, discharged only four double plates, whilst six inches of silver wire of the same diameter, discharged the whole combination, the platinum was strongly ignited in this experiment, whilst the silver was scarcely warm to the touch. On cooling the platinum wire by placing it in water, it was found to discharge ten double plates. When the intensity of the electricity is very high, however, even the cooling powers of fluid media are of very little avail : thus, I find, that fine wire of platinum was fused by the discharge of a common electrical battery under water : so that the conducting power must always be diminished by the heat generated, in a greater proportion as the intensity of the electricity is higher.

It might at first view be supposed, that when a conductor placed in the circuit, left a residuum of electricity in any battery, increase of the power of the battery, or of its surface, would not enable it to carry through any additional quantity. This, however, is far from being the case.

When saline solutions were placed in the circuit of a battery of twenty plates, though they discharged a very small quantity only of the electricity, when the troughs were only one quarter full, yet their chemical decomposition exhibited the part of a much larger quantity passing through them, when the cells were filled with fluid.

And a similar circumstance occurred with respect to a wire of platinum, of such a length as to leave a considerable residuum in a battery, when only half its surface was used : yet when the whole surface was employed, it became much better, and nevertheless left a still more considerable residuum.

VIII. I found long ago, that in increasing the number of interruptions of similar plates, the quantity of electricity seemed to increase as the number, at least as far as it could be judged of by the effects of heat upon wires; but only within certain limits, beyond which the number appeared to diminish, rather than increase the quantity. Thus, the two thousand double plates of the London Institution, when arranged as one battery, will not ignite so much wire as a single battery of two plates with double copper.

It it not easy to explain this result. Does the intensity mark the rapidity of the motion of the electricity? or, merely its diminished attraction for the matter on which it acts? And does this attraction become less in proportion as the circuit through which it passes, or in which it is generated, contains a greater number of alternations of bad conductors?

Mr. Children, in his account of the experiments made with his battery of large plates, has ingeniously referred the heat produced by the passage of electricity through conductors, to be resistance it meets with, and has supposed, what proves to be the fact, that the heat is in some inverse ratio to the conducting power. The greatest heat, however, is produced in air, where there is reason to suppose the least resistance; and as the presence of heat renders bodies worse conductors, another view may be taken, namely, that the excitation of heat occasions the imperfections of the conducting power. But till the causes of heat and electricity are known, and of that peculiar constitution of matter which excites the one, and transmits or propagates the other, our reasoning on this subject must be inconclusive.

I found that when equal portions of wire of the same diameter, but of different metals, were connected together in the circuit of a powerful voltaic battery, acting as two surfaces, the metals were heated in the following order:—iron most, then palladium, then platinum, then tin, then zinc, then gold, then lead, then copper, and silver least of all. And from one experiment, in which similar wires of platinum and silver joined in the same circuit, were placed in equal portions of oil, it appeared that the generation of heat was nearly as their conducting power. Thus the silver raised the temperature of the oil, only four degrees, whilst the platinum raised it twenty-two. The same relations to heat seem to exist, whatever is the intensity of the electricity: thus circuits of wires placed under water, and acted on by the common electrical discharge, were heated in the same order as the voltaic battery, as was shown by their relative fusion; thus iron fusing before platinum, platinum before gold, and so on.

If a chain be made of wire of platinum and silver, in alternate links soldered together, the silver wire being four or five times the diameter of the platinum, and placed in a powerful voltaic circuit, the silver links are not sensibly heated, whilst all those of the platinum become intensely and equally ignited. This is an important experiment for investigating the nature of heat. If heat be supposed a substance, it cannot be imagined to be expelled from the platinum, because an unlimited quantity may be generated from the same platinum, i. e. as long as the electricity is excited, or as often as it is renewed. Or if it is supposed to be identical with, or an element of, electricity, it ought

to bear some relation to its quantity, and might be expected to
be the same in every part of the chain, or greatest in those
parts nearest the battery.   •

IX. The magnetism produced by electricity, though with the
same conductor, it increases the heat, as I mentioned in my last
paper, yet with different conductors I find it follows a very
different law.   Thus, when a chain is made of different con-
ducting wires, and they are placed in the same circuit, they all
exhibit equal magnetic powers, and take up equal quantities of
iron filings.   So that the magnetism seems directly as the quan-
tity of electricity which they transmit.   And when in a highly
powerful voltaic battery, wires of the same diameter and length,
but of which the best conducting is incapable of wholly dis-
charging the battery, are made, separately and successively, to
form the circuit, they take up different quantities of iron filings,
in some direct proportion to their conducting powers.

Thus, in one experiment, two inches of wire of one-thirtieth
of an inch being used, silver took up thirty-two grains, copper
twenty-four, platinum eleven, and iron eight and two-tenths.

On the Electrical Phenomena exhibited in Vacuo.   By Sir Hum-
PHRY, BART.; P.R.S.

The production of heat and light by electrical discharges :
the manner in which chemical attractions are produced, des-
troyed, or modified by changes in the electrical states of bodies ;
and the late important discovery of the connexion of magne-
tism with electricity, have opened an extensive field of enquiry
in physical science, and have rendered investigations concern-
ing the nature of electricity and the laws by which it is go-
verned, and the properties which it communicates to bodies,
much more interesting than at any former period of the history
of philosophy.

Is electricity a subtile elastic fluid ? or are electrical effects
merely the exhibition of the attractive powers of the particles
of bodies ? Are heat and light elements of electricity, or merely
the effects of its action ?   Is magnetism identical with electri-
city, or an independent agent, put into motion or activity by
electricity ?   Queries of this kind might be considerably mul-
tiplied, and stated in more precise and various forms : the solu-
tion of them, it must be allowed, is of the highest importance :
and though some persons have undertaken to answer them in
the most positive manner, yet there are, I believe, few sagacious

reasoners, who think that our present data are sufficient to ena-
ble us to decide on such very abstruse and difficult parts of
corpuscular philosophy.

It appeared to me an object of considerable moment, and one
intimately connected with all these enquiries, *the relations of
electricity to space, as nearly void of matter as it can be made
on the surface of the earth ;* and, in consequence, I undertook
some experiments on the subject.

It is well known to the Fellows of this Society who have
considered the subject of electricity, that Mr. WALSH believed
that the electrical light was not producible in a perfect Torri-
cellian vacuum : and that Mr. MORGAN drew the same inference
from his researches : and concluded that such a vacuum pre-
vented the discharge of coated glass. Now it is well known,
that in the most perfect vacuum that can be made in the torri-
cellian tube, vapour of mercury, though of extremely small
density, exists ; I could not help, therefore, entertaining a doubt
as to the perfect accuracy of these results, and I resolved not
only to examine them experimentally, but likewise, by using a
comparatively fixed metal in fusion for making the vacuum, to
exclude, as far as possible, the presence of any volatile matter.

The apparatus that I employed was extremely simple,
and consisted of a curved glass tube with one leg closed and
longer than the other. In this closed leg a wire of platinum
was hermetically cemented, for the purpose of transmitting the
electricity : or to ascertain the power of the vacuum to receive
a charge, it was coated with foil of tin or platinum. The open
end, when the closed leg had been filled with mercury or any
other metal, was exhausted either by being placed under the
receiver, or connected with the stop cock of an excellent air
pump : and in some cases, to ensure greater accuracy, the ex-
haustion was made after the tube and apparatus had been filled
with hydrogen.*

Operating in this way, it was easy to procure a vacuum either
of a large or a small size, for the rarified air or gas could be
made to balance a column of fluid metal of any length, from 20
inches to the 20th of an inch, and by using only a small quan-
tity of metal, it could be more easily purged of air.

I shall first mention the results I obtained with quicksilver·
I found that by using recently distilled quicksilver in the tubes,
and boiling it in vacuo six or seven times from the top to the
bottom, and from the bottom to the top, making it vibrate re-
peatedly by striking it with a small piece of wood, a column
was obtained in the tube free from the smallest particle of air:
but a phenomenon occurred, in discovering the causes of which

* The figure near the end of this article will best explain the form of the
apparatus.

I had a great deal of trouble. When 1 used a short tube of four or five inches long only, I found, that after continued boiling and much agitation of the mercury, though there was no appearance of elastic matter, when the mercury adhered strongly to the upper part of the tube, yet that, after electrization, or even on suffering the mercury to pass slowly back into the closed part, a minute globular space sometimes appeared : I thought at first that this was air, which, though so highly rarefied, as it must have been by the exhaustion, adhered to the mercury : and I endeavoured by long boiling the mercury in an exhausted *double* syphon, and making the vacuum in one of the curves, to prevent entirely the presence of air : but the phenomenon always occurred when there was no strong adhesion of mercury to the glass. This, and another circumstance, namely, that when the leg in which the torricellian vacuum was made was 15 or 16 inches long, the phenomenon was very rarely perceptible, and always disappeared when the tube was inverted, and the mercury made to strike the top with some force, led me to conclude that the minute space was really filled with the vapour of mercury : the attraction of the particles of the fluid mercury for each other preventing their actual contact with the glass, except when this contact was forcibly made by mechanical means : and I soon proved that this was the case ; for by causing the mercury, when its column was short, to descend into the more perfect from the less perfect vacuum, with more or less velocity, I could make the space more or less, or cause its disappearance altogether, in which last case the cohesion between the mercury and the glass was always extremely strong.

I found that in all cases when the mercurial vacuum was perfect, it was permeable to electricity, and was rendered luminous by either the common spark, or the shock from a Leyden jar, and the coated glass surrounding it became charged : but the degree of intensity of these phenomena depended upon the temperature : when the tube was very hot, the electrical light appeared in the vapour of a bright green colour, and of great density ; as the temperature diminished, it lost its vividness ; and when it was artificially cooled to 20° below zero of FAHRENHEIT, it was so faint as to require considerable darkness to be perceptible.

The charge likewise communicated to the tin or platinum foil was higher the higher the temperature ; which, like the other phenomenon, must depend upon the different density of the vapour of mercury ; and at 0° FAHRENHEIT it was very feeble indeed.

A very beautiful phenomenon occurred in boiling the mercury in the exhausted tube, which shewed the great brilliancy of the electrical light in pure dense vapour of mercury. In the formation and condensation of the globules of mercurial

vapour, the electricity produced by the friction of the mercury against the glass, was discharged through the vapour with sparks so bright as to be visible in daylight.

In all cases when the minutest quantity of rare air was introduced into the mercurial vacuum, the colour of the light produced by the passage of the electricity changed from green to sea-green; and, by increasing the quantity, to blue and purple; and when the temperature was low, the vacuum became a much better conductor.

-I tried to get rid of a portion of the mercurial vapour, by using a difficultly fusible amalgam of mercury and tin, which was made to crystalize by cooling in the tube; but the results were precisely the same as when pure mercury was used.

I tried to make a vacuum above the fusible alloy of bismuth, but I found it so liable to oxidate and dirt the tube, that I soon renounced farther attempts of this kind.

On a vacuum above fused tin I made a number of experiments; and by using freshly cut pieces of grain tin, and fusing them in a tube made void after being filled with hydrogen, and by long-continued heat and agitation, I had a column of fused tin which appeared entirely free from gas; yet the vacuum made above this, exhibited the same phenomena as the mercurial vacuum. At temperatures below 0^c, the light was yellow, and of the palest phosphorescent kind, requiring almost absolute darkness to be perceived; and it was not perceptibly increased by heat.

I made two experiments on electrical and magnetic repulsions and attractions in the mercurial vacuum, by attaching to the platinum wire two fine wires in one case of platinum, in the other of steel, terminated by minute sphericles of the same metals: I found that they repelled each other when the wire was electrified in the most perfect mercurial vacuum, as they would have done in usual cases: and the steel globules were as obedient to the magnet as in air: which last result it was easy to anticipate.

In some of the first of these experiments, I used a wire for connecting the mercury, or the tin in the tube, with the stop-cock; but latterly the rarefied air or gas was the only chain of communication: and this circumstance enabled me to ascertain that the feebleness of the light in the most perfect vacuum was not owing merely to a smaller quantity of electricity passing through it, for the same discharge which produced a faint green light in the upper part of the tube, produced a bright purple light in the lower part, and a strong spark in the atmosphere.

The boiling point of pure olive oil is not much below that of mercury: and the butter or chloride of antimony (antimonane) boils at about 380° FAHRENHEIT. I tried both these substances in the vacuum, and found, as might be expected, that the light produced by electricity passing through the vapour of the

chloride, was much more brilliant than that produced by it in
passing through the vapour of the oil; and in the last it was
more brilliant than in the vapour of mercury at common tem-
peratures : the lights were of different colours, being of a pure
white in the vapour of chloride, and of a red, inclined to purple,
in that of oil; and in both cases permanent elastic fluid was
produced by its transmission.

The law of the diminution of the density of vapours, by
diminution of temperature, has not been accurately a certained:
but I have no doubt from the experiments of Mr. Dalton, and
some I have made myself, that it is represented by a geometri-
cal progression; the decrements of temperature being in arith-
metical progression,* the ratio seemed nearly uniform for the
same number of degrees below the boiling point; and (taking
intervals of 20° of temperature) ·369416.   Upon this datum,
and considering the boiling points of mercury to be 600°, that
of oil 540°, that of the chloride of antimony 340°, and that of
tin 5000° all above 52°, and the elastic force of vapour of
water at this temperature to be equal to raise by its pressure
about .45 parts of an inch of mercury ; the relative strengths of
vapour will be, for mercury 000015615, for oil 0016819, for
chloride of antimony 01692, and for tin 37015, preceded by 48
zeros.†

It is not known whether the vapour from solids follows a
similar law of progression as that from fluids, and these num-
bers are only given to show how minute the quantity of matter
must be in vapour where its effects are distinct upon electrical
phenomena: and how much more minute it must be in the case
of mercury artificially cooled: and almost beyond imagination
so in vapours from substances requiring very elevated tempera-
tures for their ebulition.

I made some comparative experiments to ascertain whether
below the freezing point of water, the temperature of the torri-
cellian vacuum diminished the power of transmitting electri-
city, or of being rendered luminous by it.   To about 20° this
appeared to be the case; but between 20° above and 20°
degrees below zero, the lowest temperature I could produce
by pounded ice and muriate of lime, it seemed stationary :
and as well as I could determine, the electrical phenomena
were nearly of the same intensity as those produced in the
vacuum above tin.

Unless the electrical machine was very active, no light was
visible during the transmission of the electricity : but that this
transmission took place, was evident from the luminous ap-

* Water, chloride of phosphorus, and alcohol or carburet of sulphur.‡

† I am obliged to Charles Babbage, Esq., F.R.S., for these calculations.

pearance of the rarified air in the other parts of the syphon, and from the diminution of the repulsion of the ball of the quadrant electrometer attached to the prime conductor. When the machine was in great activity, there was a pale phosphorescent light above, and a spark on the mercury below, and brilliant light in the common vacuó. A Leyden jar *weakly* charged could not be made to transmit its electricity by explosion through the cooled torricellian vacuum, but this was slowly dissipated through it ; and when *strongly* charged, the spark passed through nearly as much space as in common air, and with a light visible in the shade. At all temperatures below 200°, the mercurial vacuum was a much worse conductor than highly rarefied air ; and when the tube containing it was included in the exhausted receiver, its temperature being about 50°, the spark passed through a distance of six times greater in the Boylean than in the mercurial vacuum.

It is evident from these general results that the light (and probably the heat) generated in electrical discharges depends *principally* on some properties or substances belonging to the ponderable matter through which it passes: but they prove likewise that space, where there is no appreciable quantity of this matter, is capable of exhibiting electrical phenomena : and, under this point of view, they are favourable to the idea of the phenomena of electricity being produced by a highly subtile fluid or fluids, of which the particles are repulsive, with respect to each other, and attractive of the particles of other matter. On such an abstruse question, however, there can be no demonstrable evidence. It may be assumed, as in the hypothesis of Hooke, Huygens, and Euler, that an etherial matter, susceptible of electrical affections, fills all space; or that the positive and negative electrical states, may increase the force of vapour from the substances in which they exist : and there is a fact in favor of this last idea which I have often witnessed—when the voltaic discharge is made in the Boylean vacuum, either from platinum or charcoal, in contact with mercury, the discharging surfaces require to be brought very near in the first instance ; but the electricity afterwards may be made to pass to considerable distances through the vapour generated by the mercury or charcoal by its agency ; and when two surfaces of highly fixed metal, such as platinum or iron are used, the discharge will pass only through a very small distance, and cannot be permanently kept up.

The circumstance, that the intensity of the electrical light in the mercurial vacuum diminishes as it is cooled to a certain point, when the vapour must be of infinitely small density, and is then stationary, seems strongly opposed to the idea, that it is owing to any *permanent* vapour, emitted constantly by the mercury. The results with tin must be regarded as more equivocal :

because as this substance cannot be boiled in vacuo, it may always be suspected to have emitted a small quantity of the rare air or gas to which it has been exposed ; yet supposing this circumstance, such gas must at least be as highly expanded as the vapour from cooled mercury, and can hardly be supposed capable of affording the dense light, which the passage of the electricity of the Leyden phial through the vacuum produces.

When the intense heat produced by electricity is considered, and the strong attractive powers of differently electrified surfaces, and the rapidity of the changes of state, it does not seem at all improbable, that the superficial particles of bodies, which, when detached by the repulsive power of heat, form vapour, may be likewise detached by electrical powers, and that they may produce luminous appearances in a vacuum, free from all other matter, by the annihilation of their opposite electrical states.

In common cases of electrical action, the quantity of heat generated by the annihilation of the different electrical states, depends, as I stated in my last communication to the society, upon the nature of the matter on which it acts ; and in cases when electrical sparks are taken in fluids, vapour or gas is always generated ; and in elastic fluids, the intensity of the light is always greater, the denser the medium. The luminous appearances therefore, it is evident from all the statements, must be considered as secondary : whilst the uniform exertions of electrical attractions and repulsions, under all circumstances, in rare and dense media and vacuo, and with respect to solids, fluids, and gases, point them out (whether they be specific affections of a subtile imponderable fluid, or peculiar properties of matter) as primary and invariable electrical phenomena.

I have mentioned in the last page the suspicion, that melted tin may contain air. I shall conclude this paper by stating the grounds of this suspicion, and noticing a circumstance which appears to be of considerable importance, both in relation to the construction of barometers and thermometers, and to the analysis of gaseous bodies. Recently distilled mercury that has been afterwards boiled and cooled in the atmosphere, and which presents a perfectly smooth surface in a barometer tube, emits air when strongly heated in vacuo, and that in quantities sufficient to cover the whole interior of the tube with globules ; and on keeping the stop cock of one of the tubes used in the experiments on the mercurial vacuum open for some hours, it was found that the lower stratum of mercury had imbibed air, for when heated in vacuo, it emitted it distinctly from a space of a quarter of an inch of the column ; and its production ceased at about an inch high in the tube. There is great reason to believe, that this air exists in mercury in the same invisible state as in water, and is distributed through its pores ; and the fact

shows the necessity of long boiling the mercury in the baro-meter and thermometer tubes, and the propriety of exposing as small a surface of the mercury as possible to the air. It may explain, likewise, the difference of the height of the mercury in different barometers; and seems to indicate the propriety of re-boiling the mercury in these instruments after a certain lapse of time.
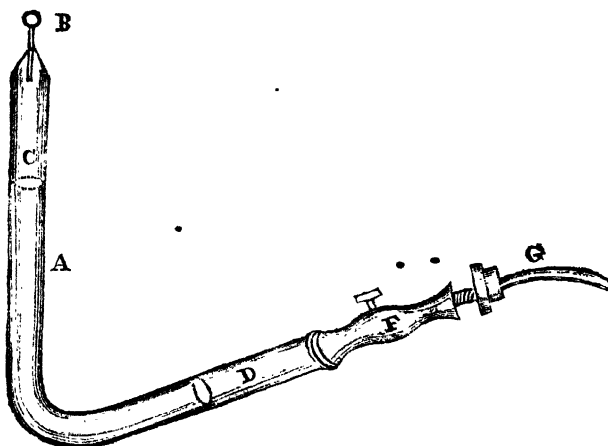
A The tube of the usual diameter.

B The wire for communicating electricity.

E A small cylinder of metalic foil, to place as a cap on tubes not having the wire B, to make a coated surface.*

C The surface of the quicksilver, or fused tin.

D The part of the tube to be exhausted by the stop cock F, after being filled by means of the same stop cock, when neces-sary, with hydrogen.

G The moveable tube connected with the air pump.

It is evident that by introducing more mercury, the leg D may be filled with mercury, and the stop cock closed upon it, so as to leave only a torricellian vacuum in the tube, in which the mercury may be boiled. I have found that the experiment tried in this way offers no difference of result.

* Not in the figure.

An Account of some Experiments on the Blood in connexion with the Theory of Respiration. By JOHN DAVY, M.D. F.R.S., *Assistant Inspector of Army Hospitals.*

Connected with the theory of respiration and of animal heat there are many questions of interest respecting the blood, about which physiologists differ in opinion, and which consequently are fit subjects for further inquiry.

Some of the more important and fundamental of these questions I have endeavoured to investigate experimentally, and in the present communication I propose to submit to the Royal Society the results which I have obtained, with the hope that they may be considered not unworthy of a place in the Philosophical transactions.

I. *Is blood capable of absorbing oxygen independent of putrefaction ?*

To endeavour to satisfy myself on this point, on which in a former enquiry I had arrived at a negative conclusion in opposition to the commonly received opinion, I have employed two methods of experimenting ; one, of agitating blood, recently drawn and rapidly cooled in common air and in oxygen, in a tube of the capacity of two cubic inches, divided into a hundred parts ; the other, of agitating it in larger quantities, with the same airs, in the very convenient apparatus employed by Dr. CHRISTISON, when engaged in a similar inquiry, consisting of a double tubulated bottle of the capacity of thirty-two cubic inches, provided with stop-cocks adapted by grinding, to one of which a moveable bent glass tube was fitted to connect it with a pneumatic trough, and to the other a perpendicular tube surmounted by a funnel.

The blood subjected to experiment in every instance was prepared by the displacement of its fibrin. This was done by agitating it with small pieces of sheet-lead in a bottle filled to overflowing and closed with a cork, enveloped in moist bladder and covered over with the same, tied round the neck of the bottle, so as to exclude atmospheric air, whilst in the act of coagulating and of cooling, and to allow, when cooled, of the withdrawal of the cork.

Prepared thus, and rapidly cooled, I have tried different specimens of blood, venous and arterial, of Man, of the Sheep, Ox, Dog and Cat ; and the results, when the blood has been taken from a healthy animal, have been decisive and consistent. In every instance, whether atmospheric air or oxygen was used, after agitation, there was a marked diminution of the volume of air.

In the examples which it may be advisable to bring forward, I shall confine myself to a few of the experiments I have made on the blood of the Sheep.

Using the graduated tube over mercury, sixty-two measures of arterial blood from the carotid artery, agitated with thirty-three measures of common air, produced a diminution of two measures ; and sixty-three of the same arterial blood, agitated with thirteen of pure oxygen, a diminution of three ; whilst sixty-three of venous blood from the jugular vein of the same animal, agitated with thirty-three of common air, produced a diminution of six ; and seventy of this blood, with thirteen of oxygen, a diminution of eight.*

In experiments on a larger scale, using the double-mouthed bottle, in which about ten cubic inches of blood were agitated with about twenty-two cubic inches of air, the results were in accordance with the preceding. Thus when the arterial blood of the Sheep was agitated with common air and with oxygen, on turning the stop-cock of the bent tube there was an absorption in one instance of about ·3 cubic inch, and in the other of about ·4 cubic inch ; and, with venous blood, in the instance of common air, of about 1· cubic inch, and in that of oxygen, of about 1·25 cubic inch.

These experiments were all made on the blood of the same animal. In experiments on the blood of different individuals of the same species of animal, and on the blood of animals of different species, the results have varied in regard to the degree of absorption, and remarkably so in the instance of the blood of man.

In every instance the absorption or disappearance of a portion of the air has been attended with some change in the colour of the blood ; the venous has invariably acquired the florid vermillion hue of arterial blood, and the arterial has had its florid hue heightened.

The air that has been absorbed or which disappears, when atmospheric air has been used, in accordance with the commonly received opinion and the results of Dr. CHRISTISON's experiments†, has been found to be oxygen.

Relative to the residual air, when pure oxygen has been used, whether on the smaller or larger scale of experiment, no car-·bonic acid gas has been detected in it in the most carefully

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* Notwithstanding the frothing attending the agitation of blood in air, the absorption or diminution of volume was ascertained with tolerable accuracy by observing the rise of the mercury in the tube. The experiments were made as nearly as possible under ordinary atmospheric pressure, which was easily effected, as the mercurial pneumatic trough used exceeded in depth the length of the tube.

† Edinburgh Medical and Surgical Journal, vol. xxxv. p. 94.

conducted trials.   When common air has been employed, then a trace of this acid gas has been found in the residual air after agitation with the blood, but not exceeding one per cent. at a temperature between 40° and 50°.   I shall notice in detail an experiment with each, as the results are of consequence.

Ten cubic inches of venous blood (its fibrin displaced) from the jugular vein of a sheep, rapidly cooled, were agitated in twenty-two cubic inches of oxygen gas, which had been well washed with lime-water.   After an absorption equal to about one cubic inch, some of the gas was expelled and passed into lime-water, the transparency of which it did not in the slightest degree impair.

The same quantity of similar blood was similarly treated with common air, which also had been washed with lime-water. After an absorption equal to about one cubic inch, some of the air was passed into lime-water : it occasioned a just perceptible cloudiness ; and eighty measures of it agitated with lime-water were reduced hardly to 79·5.

Taking it for granted that this very minute quantity of carbonic acid derived from the blood existed in the state of gas, and not contained in the aqueous vapour, as is possible, it is matter for consideration from whence it was derived,— whether it was formed at the moment by the action of the air on the blood, or, previously existing in the blood, was now merely expelled.   Further on I propose to return to this important question.

When I reflect on the results stated above relative to the absorption or disappearance of oxygen, and compare them with those alluded to formerly obtained, I am not a little surprised at their discrepancy ; and I can only account for it by supposing that it may have been owing in part to the difference of season when the two sets of experiments were conducted.   The first were made in Malta in 1829, in the hot months of July and August, when the thermometer in the open air was generally above 80° and occasionally above 90°.   The last have been made in England, and principally in January of the present year, when the temperature of atmosphere was occasionally low, the greater part of the time below the freezing point, and often as low as 20°.   From what I have witnessed, I am induced to infer, that the higher the atmospheric temperature is, and the less necessity there is for the production of animal heat, the less difference there is between venous and arterial blood, and the less power the former has of combining with oxygen, and of forming or evolving carbonic acid.   In Malta I carefully compared the blood of the jugular vein and carotid artery of a sheep during the season mentioned ; when congulated and still hot, there was no perceptible difference in their colour ; in each

it was less florid than the arterial blood of the same animal in England in winter, and less dark than the venous: its hue was as it were a mixture of the two. And in this observation I could not be mistaken; the circumstances under which it was made precluded mistake; vessels of the same size were used; similar quantities of blood were introduced, and they were seen in the same light side by side. And in confirmation of this view I may remark, that during the last winter, when the cold was unusually severe, I found the temperature of deeply seated parts, and especially of the heart and its left ventricle, in the instance of sheep, unusually high: the mean of nine observations on the temperature of the left ventricle in different animals was 107·5; the lowest was 105·5: the highest 109·; whilst the temperature of the rectum (the mean of the same number of observations) was 104·4.

II. *Does the blood, especially venous blood, contain carbonic acid capable of being expelled by agitation with another gas, such as hydrogen or oxygen?*

In a paper published in the Philosophical Transactions for 1832, Dr. STEVENS has answered this question in the affirmative: he maintains that carbonic acid gas exists in venous blood; that it may be expelled by oxygen or by hydrogen, although not by the air-pump; and he supposes that the difference of effect is owing to a peculiar attraction for carbonic acid exercised by these gases.

To endeavour to resolve my doubts on this important point, I have had recourse to the apparatus already mentioned. viz. the graduated tube with the mercurial pneumatic trough, and the double-mouthed bottle furnished with stop-cocks, &c. as being well adapted for simple and decisive experiments.

By means of the graduated tube I have agitated venous blood in hydrogen over mercury, as about a cubic inch of each, and other proportions, and have left the blood exposed to the influence of the gas for several hours; and I have made similar trials with it, using larger quantities in the double-mouthed bottle, as sixteen cubic inches of each, and also other proportions; the results have been either of the same negative character, or, if different, indicating only the disengagement of carbonic acid was in an extremely minute quantity. In all the experiments with the graduated tube in which fresh blood was used, whether of man or of the sheep, the fibrin displaced out of the contact of the air, on agitation with hydrogen, there was no sensible increase of the volume of the gas, and no diminution of it when it was transferred to, and shaken with, lime-water. And in the best experiments, on a larger scale, with the double-mouthed bottle, when the most attention was paid to all the circumstances likely to insure accuracy, as in the first instance the exclusion

of air from the blood, and in the second the having it of the
temperature of the bottle and of the room, the results have been
similar, and of a negative character.   I shall describe a small
number of experiments, those, the results of which appeared
least ambiguous.

Seven cubic inches of sheep's blood from the jugular vein,
its fibrin broken up by agitation with lead whilst coagulating,
out of contact with air, and cooled under water, were agitated
with hydrogen (twenty-five cubic inches), previously well
washed with lime-water, and which, tested by lime-water, after
this precaution were found perfectly free from carbonic acid.
On turning the stop-cock of the bent tube connected with dis-
tilled water, no change of volume was indicated, and the blood
was agitated again with the same result.   By means of the per-
pendicular tube distilled water was admitted, and some of the
gas expelled ; first a cubic inch into a graduated tube filled with
lime-water, and next about four cubic inches into a vial filled
with distilled water, and in which afterwards a little lime-water
was added to the gas ; in neither could any traces of carbonic
acid be detected ; the lime-water remained transparent.

Ten cubic inches of venous blood, taken by a large orifice
from the arm of a young man threatened with hæmoptysis, the
fibrin broken up in the same manner as the last, and rapidly
cooled under water in a running stream to the temperature of
the room, 51°, were similarly treated with hydrogen, and with
precisely the same result ; after having been twice well shaken,
on turning the stop-cock, there was no change of volume. The
blood was kept in contact with the hydrogen over night, the
stop-cocks closed.   The night, that of the 2ord of January,
was severely cold ; at 11 o'clock the following morning the
temperature of the room was only 45° ; now on turning the
stop-cock of the bent tube the water rose in it to the extent of
about one-eighth of a cubic inch.

Twenty-four cubic inches of the mixed arterial and venous
blood of the sheep, collected and prepared with similar precau-
tions, were divided into two portions of about twelve cubic
inches ; one was agitated in the double-mouthed bottle with
hydrogen after the introduction of a little milk of lime, the
other without this addition.   The result in each instance was
the same ; on opening the stop-cock, after the agitation, the
water rose a very little in the bent tube, about one-twentieth of
a cubic inch.

The results of some of the trials already described on the
action of oxygen on venous blood, both pure and mixed with
azote, in the form of common air, are very consistent with those
just detailed on hydrogen.   Previously to stating some other
results in quest of fresh evidence on the same subject, it may be
advisable to notice particularly the power which blood possesses
of absorbing carbonic acid.

From experiments which I made on blood and serum in 1824 and 1828,* I inferred that each is capable of absorbing about an equal volume of this acid gas. I now find that when pure carbonic acid gas is brought in contact with blood or serum over mercury, and moderately agitated under ordinary atmospheric pressure, that the absorption of gas exceeds the volume of the fluid, both in the instance of blood and serum. The results of some experiments are exhibited in the following table. The majority of them were obtained between a temperature of 40° and 45°; the three last at about 51°. When the venous and arterial blood, and the serum tried, were from the same animal, the numbers expressing the results are entered in the same line.

Volume of carbonic acid gas absorbed in 100 parts of blood and

	Animal.	Venous blood.	Venous serum.	Arterial blood.	Arterial serum.
	Sheep..	160		•167	
	Sheep..	155			
3	Sheep..			133	
4	Sheep..	142			
5	Sheep..	.....		150	
6	Sheep..	166			
7	Sheep..				120
8	Ox	194			
9	Ox	181			
10	Man ..	118			
11	Heifer	120			117
12	Sheep..	118	125	141	125
13	Sheep..		118		118
14	Man ..	153			
15	Sheep..	160			
16	Sheep..			159	

The effect of the absorption of the gas to perfect saturation was on the arterial and venous blood the same; it rendered both very dark; the serum it rendered more liquid, which was well marked by diminished tendency to froth on agitation.

I shall now proceed to notice the trials which I have instituted of agitating blood and serum, to which a known quantity of carbonic acid had been added, with one or more of the gases considered by Dr. STEVENS as exerting an attraction on carbonic acid, and by that means expelling it.

From the experiments which I have made on serum, it appears in its healthy state incapable of absorbing oxygen, or of immediately furnishing carbon to form carbonic acid; and in

no instances in which I have agitated it with common air or with hydrogen, when obtained from the blood of a healthy animal, has there been any indication of the disengagement of gas ; it therefore is peculiarly well fitted for the trial in question.

To nine cubic inches of serum from the mixed blood- of the sheep one cubic inch more of serum was added, containing a cubic inch of carbonic acid, with which it had been impregnated over mercury. The mixture of the two was introduced into the double-mouthed bottle without delay, and well agitated with twenty-two cubic inches of common air. On turning the stop-cock there was no change of volume. The serum was transferred, and there was added to it, with as little motion as possible, another cubic inch of serum, containing the same quantity of carbonic acid. Now poured back into the bottle and agitated, on opening the stop-cock a little air was disengaged ; it was collected and found equal to $\frac{4}{100}$ths cubic inch. The serum was left exposed to the action of the air in the bottle over night, the stop-cocks closed ; the following morning on opening the stop-cock of the bent tube no air was expelled ; on the contrary, there was a just perceptible rise of water in it. The experiment was carried further: the serum was transferred to a vial and closed, and the double-mouthed bottle was filled with hydrogen. The serum was returned and well agitated with the hydrogen. On turning the stop-cock $\frac{14}{100}$ths of a cubic inch of air was expelled. It was agitated a second time without further expulsion of air, and left in contact with hydrogen for more than twelve hours without any further effect. Thus it appears that of the two cubic inches of carbonic acid gas introduced into the serum, only one-fifth of a cubic inch was expelled by successive agitation with atmospheric air and hydrogen.

One cubic inch of venous blood of a man which had absorbed 1·2 cubic inch of carbonic acid, was mixed with twelve cubic inches of similar blood, and agitated with hydrogen in the double-mouthed bottle. A very little air only was expelled, viz. $\frac{4}{100}$ths cubic inch.

To fifty-five measures (1·1 cubic inch) of venous blood of a sheep, twenty-measures of gas were added over mercury, composed of about equal parts of oxygen and carbonic acid. After agitation about seventeen measures were absorbed, and the blood had acquired a florid hue ;. ten measures more of oxygen were added ; there was no further absorption. The tube was transferred to lime-water and agitated ; the residual air was not diminished ; it amounted to thirteen or fourteen measures (the froth prevented precision in marking the quantity), which possessed the properties of oxygen, as tested by the taper.

To twenty-eight measures of venous blood of a sheep, which

had absorbed twenty-six of carbonic acid, forty-nine of oxygen were added in the graduated tube over mercury : after agitation the blood had acquired the florid arterial hue, and there appeared to be an expansion of one measure. Transferred to lime-water there was an absorption of one or two measures, and no more.

I shall notice one experiment more, in which blood nearly saturated with carbonic acid was exposed to oxygen, a membrane intervening. Forty-seven measures of venous blood, which had absorbed thirty-three of carbonic acid, were introduced into a glass tube half an inch in diameter, closed at one end with gold-beater's skin, and when filled with blood at the other end also, it was placed over mercury in a small receiver, and thirty-seven measures of oxygen were added to it, under a diminished pressure of about one inch. After twenty-four hours there was no change of volume ; the blood in the tube had acquired throughout the arterial hue ; the gas, thirty-eight measures, transferred to lime-water and agitated, diminished to thirty-two.

The tube was now placed under a receiver, and the air exhausted by the air-pump ; a good deal of air was disengaged in the form of froth, and the gold-beater's skin was so distended that it appeared ready to burst ; after three or four minutes air was re-admitted ; a notable portion of gas was found free between the membrane and blood ; thus showing that in oxygen gas carbonic acid gas is less freely exhaled through a membrane than in vacuo.

The results of this second set of experiments are in accordance with those of the first. The inference I am induced to draw from both is rather of a negative kind, and unfavourable to the conclusion of Dr. Stevens already referred to, at least in a strict and general sense. I think it right to express myself thus reservedly, reflecting on some of the experiments in which a very little carbonic acid gas appeared to be extricated on agitating blood with hydrogen ; and believing that Dr. Stevens, and other able inquirers, could not have been misled on a point so little exposed to fallacy.

Relative to the effects which Dr. Stevens refers to the attraction of one gas for another, they appear to me, from what I have witnessed in carrying on the inquiry, to admit of explanation on Dr. Dalton's theory of mixed gases, and that in no instance is the effect of disengagement of air from a fluid, agitated with another kind of air, greater than were it agitated in a vacuum.

III. *What is the condition of the alkali in the blood in relation to carbonic acid ?*

On this point much difference of opinion exists amongst in-

quirers ; some believing that the alkali, or at least a portion of
it, is uncombined, or combined merely with water, or with
water and albumen ; some that it is united to carbonic acid in
the state of carbonate ; others that it is saturated with this gas,
and in the state of bi-carbonate.

The subject, it must be confessed, is one of great difficulty,
and very perplexing ; partly from the nature of the blood,
liable to great variations during life, and to rapid change after
death ; and partly also from the nature of the alkaline carbo-
nates, hardly less disposed to change than the blood itself, from
variation of circumstances, and to pass from one degree of com-
bination into another.

The bicarbonate of soda, I believe, like the bicarbonate of
ammonia, can only exist in perfection in the solid state. In dis-
solving, I find, when exposed to the atmosphere, it gives off a
part of its acid, and still more when it is agitated with com-
mon air, and more still with hydrogen, and in a greater degree
the higher the temperature. This is not favourable to the idea
that it exists in the blood, especially when it is considered that
this fluid may be exposed to a temperature of 212° without dis-
engaging carbonic acid, of which I have had proof in several
trials.

Sulphuretted hydrogen does not expel carbonic acid from the
alkalies in solution in water. Bicarbonate of soda dissolved to
saturation in distilled water absorbs, I find, 143 per cent. of its
volume of this gas ; whilst the serum blood (it was sheep's that
was tried) dissolved 207 per cent., arterial blood 235, and venous
290. This, too, is unfavourable to the same idea ; as is also
the large proportion of carbonic acid which blood, it has been
shown, is capable of absorbing.

Supertartrate of potash occasions an effervescence, when mixed
in substance with a solution of the sesquicarbonate, but not of
the carbonate of soda ; and the effect is similar, whether the
mixture be made over mercury, air excluded, or in an open
vessel exposed to the atmosphere. The supertartrate of potash
also, I find, mixed with blood and agitated with common air,
acts as with the sesquicarbonate, and occasions a disengagement
of air, and both from arterial and venous blood, and from serum;
and the air I have ascertained is carbonic acid.

From these facts, may it not be inferred that the alkali in the
blood, in its normal or healthiest condition, is neither in the
state of carbonate nor of bicarbonate, but of sesquicarbonate ?
The power of the blood to absorb carbonic acid and sulphuret-
ted hydrogen accords best with this view, and some other im-
portant properties of the fluid are, I believe, in harmony with
it.

The sesquicarbonate, I may add, seems to be the state of rest

of the alkali in combination with carbonic acid, under ordinary circumstances of exposure to the atmosphere. Thus the native compound is the sesquicarbonate, as is also, I believe, the effloresced salt.* And I find that although a solution of the bicarbonate may be brought by the air pump to the state of sesquicarbonate, it cannot be reduced to that of the carbonate: after it has ceased to give off any air in vacuo, it effervesces with the supertartrate of potash; and if evaporated to dryness over sulphuric acid under an exhausted receiver, on being subjected to heat, it disengages carbonic acid gas.

IV. *Does the blood contain any gas capable of being extricated by the air pump?*

On this subject also there has been much difference of opinion. Our distinguished countryman MAYOW, more than a century ago, stated that blood, especially arterial blood, effervesces in vacuo, which he attributed to the disengagement of air, his Spiritus Nitro-aëreus.† Sir EVERARD HOME, on the authority of Mr. BRANDE, in 1818, asserted that blood, both venous and arterial, under the exhausted receiver, evolves a large quantity of carbonic acid gas, an ounce of blood as much as two cubic inches of gas.‡ I repeated the experiment shortly after, but without confirming the result; neither by the airpump, nor by heat applied even to coagulation of blood and serum in close vessels, did I succeed in demonstrating the extrication of this acid.§ Since that time the experiment of the airpump on the blood has been frequently made, and by observers of great accuracy, as by Drs. DUNCAN and CHRISTISON in this country, by MM. TIEDEMAN, GMELIN, MITSCHENLICH and MULLER on the continent, and recently by MM. BISCHOFF and MAGNUS. With the exception of the last-mentioned inquirers, the results have been negative. MM. BISCHOFF and MAGNUS, on the contrary, state that by careful exhaustion they have obtained gas from the blood; the former a small quantity of carbonic acid gas, the latter a notable quantity, and not only of carbonic acid gas, but also of oxygen and azote.||

M. MAGNUS attributes the failures of former experimenters to their having used pumps of imperfect exhausting power, or

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* On exposing carbonate of soda in excess to carbonic acid gas over mercury, the gas is rapidly absorbed with the expulsion of part of the water of crystallization, so as to produce an appearance of deliquescence, and the sesquicarbonate is formed.

† JOHANNIS MAYOW Opera omnia. Hagæ Com. 1681. p. 133.

‡ Philosophical Transactions for 1818, p. 181.

§ Ibid. 1823, p. 516.

|| Annales des Sciences Nat. tom. viii. p. 79. *et seq.* contain a translation of M. MAGNUS' paper, with a figure of the apparatus, &c. used; the original appeared in POGGENDORF's Journal, vol. xl. part 3.

to their not having carried the exhaustion sufficiently far. In my early and first trial I employed the air pump belonging to the laboratory of the Royal Institution, which was an excellent one, and which I then believed was in good order ; but I might have been mistaken.

To endeavour to satisfy myself on this point, I have had an air-pump constructed for the purpose, under the direction of an able artist, Mr. Ross, of 33 Regent-street, already distinguished for his excellent achromatic microscopes, and it has answered perfectly. When the exhaustion has been carried as far as possible, the difference of level of the mercury in the siphon-guage has not exceeded a quarter of an inch, and over water has not exceeded half an inch.

Experimenting with this machine on blood, collected with such precautions as I believe to have been adequate to insure accuracy of results, in a majority of instances the disengagement of gas has been rendered manifest, and both from arterial and venous blood.

I shall briefly mention the trials which I consider most conclusive, and which satisfied me in spite of an opposite pre-existing bias.

Vials provided with well ground glass stoppers were filled with distilled water, deprived as much as possible of air by the air-pump ; they were then placed under the receiver and kept in vacuo until all adhering air was removed : the stoppers were now introduced, all air being excluded, and they were instantly immersed in distilled water, which had been well boiled. Thus prepared they were taken to an adjoining slaughter-house, where they were filled with Sheep's blood in the following manner, without its coming into contact with the air. For venous blood the jugular vein was exposed ; two ligatures were applied to it : the vein was divided between the two, and the upper part, slightly detached, was introduced into a prepared vial under water the instant the stopper was withdrawn, and laid open. The heavier blood proceeding from the vessel of course stopped the water ; and when it was supposed to be all expelled the stopper was restored, and the vial was replaced in the water. In the instance of arterial blood it was collected in the same manner from the carotid artery. In some trials the blood was allowed to coagulate undisturbed ; in others the fibrin was detached, and the liquidity of the blood preserved by agitating it, the instant the stopper was replaced, with some mercury, introduced with the distilled water, and equally deprived of adhering air.

In about half an hour from the abstraction of the blood, in every instance, it was subjected to the air-pump. The instant the stopper was withdrawn the vial was placed in a small re-

ceiver on the plate of the pump, and covered with a little larger receiver, and the air as soon as possible exhausted. No appearance of disengagement of gas was perceptible until the exhaustion was nearly complete; then it was sudden, sometimes considerable, even to overflowing, in the form of bubbles, and it continued some time. The results were not distinctly different, that I could perceive, whether venous or arterial blood was used ; I am disposed to think, on the whole, that less air was disengaged from the arterial blood than from the venous.

When blood allowed to coagulate in the vessels was tried, the results varied a little, and appeared to me instructive. At first, on exhaustion, only a few particles of air were disengaged ; no more, it might be supposed, than were derived from the contact of the end of the stopper. In two experiments such was the appearance for at least five minutes, conveying the idea that no air was extricated : then abruptly a bubble or film burst with some force, as was denoted by the scattering of the particles of blood ; and a bubbling commenced and continued, rendering the indications of extrication of gas unquestionable. And in conformity with this result I may remark, I have never succeeded in obtaining indications of air in the blood in operating on it by the air-pump, if confined in a detached portion of vein, or in the heart of animals, the great vessels, previous to excision, having been tightly secured by ligature. In no instance of this kind have I witnessed any distention, such as occurs if air be admitted previous to the application of ligatures ; clearly indicative, it appears to me, that a very slight compressing force is sufficient to confine the air in the blood, or rather, I should say, prevent its substance assuming the elastic state ; and further, the probability, that the quantity of air so condensed is small.

I have stated that in the majority of instances the indications of the disengagement of air from blood in vacuo have been manifest. Exceptions, however, have occurred, and those clear and decisive ; inducing me to believe that the quantity of air condensed in the blood is variable ; that there are times when it is in excess, and times when in deficiency, and when totally wanting, connected with regularly changing states of the functional system. I hope on another occasion to be able to recur to this part of the subject, on which at present I have collected but a few facts. I may add, that such facts tend in part to reconcile some of the discrepancies referred to in the beginning of this section.

V. *Of the air or gases contained in the blood capable of being extricated by the air-pump.*

As already mentioned, M. MAGNUS has stated that these gases are carbonic acid, oxygen and azote in notable quantities

Taking the mean of ten of his experiments on the blood of the
horse and calf (five on arterial and five on venous), the total
quantity of the mixed gases he obtained was, in the instance of
arterial blood, 10·4 per cent. per volume, and in that of venous.
7·6 per cent.; the former consisting of about

> 6·5 carbonic acid,
> 2·4 oxygen,
> 1·5 azote;

the latter of

> 5 5 carbonic acid,
> 1·1 oxygen,
> 1·0 azote.

On a subject of so much importance, it is very desirable that
the experiments of M. MAGNUS should be repeated and verified :
for until this is done, considering the physiological history of
the blood, it will be difficult to avoid doubt, and to depend on
them with that degree of confidence which is justly due only to
well-authenticated facts.

The ingenious apparatus employed by M. MAGNUS being
difficult of construction, and not easily used excepting in a well-
appointed laboratory, I must leave the repetition of his highly
interesting experiments to those enquirers who are more happily
situated than myself for engaging in them. On the present
occasion I shall limit myself to the detail of some experiments
instituted with a view of testing M. MAGNUS's general results.

As a solution of potassa has the property of absorbing carbo-
nic acid gas, it follows that if mixed with the blood previous to
being subjected to the air-pump, it will prove in some measure
a test of the kind of air which the blood is capable of affording.

With this intent two vials were prepared, the same as before
used, one filled with distilled water, the other with a weak aque-
ous solution of caustic potash, both carefully deprived of air by
the air-pump. Observing the same precautions as before, a
portion of venous blood from a sheep was received into each of
them. When less than half of the water and of the solution,
as well as could be guessed, was expelled, the vials were closed
with the glass stoppers belonging to them, and instantly im-
mersed in water, and as soon as possible subjected to the air-
pump. The results of exhaustion in the two instances was
perfectly distinct. From the blood mixed with water gas was
disengaged ; there was a continued ascension of bubbles. From
the blood mixed with the alkaline solution no gas was liberated,
excepting a bubble or two, which might fairly be considered as
entangled air derived from contact of the blood with the
stopper.

A similar comparative trial was instituted with arterial blood
of the sheep, and the results also were similar and equally well
marked.

Considered as test experiments, the first inference from these results is, that carbonic acid gas, or an air absorbable by a solution of potash, is disengaged both by venous and arterial blood in vacuo ; and next, that no other gas is disengaged from either of them, neither oxygen nor azote, each of which is unabsorbable by the solution in question. I have repeated the experiment twice on the venous blood of man, and twice on the venous blood of the sheep, and twice on the arterial blood of the latter, without variation of results ; and they are more to be depended on, as the alkali has the effect of preserving the blood liquid.

I may mention another method which I have employed as a test experiment ; and first in relation to carbonic acid. If the blood contain a notable portion of this gas capable of being extricated by the air-pump, it necessarily follows, that when subjected to the action of the air-pump, and deprived as far as possible of its fixed air by this means, it will be capable of absorbing a larger quantity of carbonic acid than previous to the exhaustion. The following table contains the results of three comparative trials on the venous blood of the sheep, its fibrin separated or detached in the usual manner.

Table showing the absorption of carbonic acid gas by

Venous blood subjected to the air-pump.			Venous blood not subjected to the air-pump.		
Volume of blood used.	Volume of carbonic acid introduced.	Volume of gas absorbed.	Volume of blood used.	Volume of gas introduced.	Volume of gas absorbed.
27	46	34	27	47	37
27	45	39	27	45	39
27	46	35	27	45	34

Although I have thought it right to notice these results, and although they are in accordance with the preceding, I do not attach much value to them, excepting as tending to show that the quantity of carbonic acid gas extracted by the air-pump, when the blood affords it in vacuo, is small.

The same mode of reasoning suggested comparative trials of the absorbent power of arterial blood for oxygen, before and after exhaustion by the air-pump, as a further test experiment, whether arterial blood contains oxygen in a free state, that is, admitting of being extricated by the removal of atmospheric pressure. The result of this trial also has been negative ; its power of absorbing oxygen has not appeared to be at all increased by exhaustion ; this at least was the result of one experiment carefully conducted.

VI. *Is any oxygen contained in the blood not capable of being extricated by the air-pump ?*

Before I was acquainted with the researches of M. MAGNUS I had instituted some experiments to endeavour to determine whether any oxygen in a free state, or in a condition approximating to that state, exists in the blood, and especially in the arterial, admitting of being detected by means of substances possessing a strong attraction for oxygen, or of being expelled by substances of greater solubility in blood than oxygen. Hydrogen, phosphuretted hydrogen, sulphuretted hydrogen, nitrous oxide, and nitrous gas, it appeared advisable to try, as belonging to the first class of substances, and carbonic acid gas as belonging to the latter.

The results with hydrogen, sulphuretted hydrogen, nitrous oxide, and carbonic acid gas, were of a negative kind. Neither using arterial blood, nor blood which had been agitated with oxygen, and which had absorbed or made to disappear a certain quantity of this gas, could I detect any indications of its presence either by combination or expulsion. In the instances of nitrous oxide and carbonic acid, however, it may be worthy of remark, that the blood which had been agitated with oxygen absorbed less of either of these gases than it did before it was so treated.*

The results with phosphuretted hydrogen, the spontaneously inflammable species, were of an ambiguous kind, not sufficiently clear to deduce from them any satisfactory conclusion. In one trial, serum of the venous blood of the sheep absorbed nine per cent. of this gas ; venous blood 11·3 per cent. ; and arterial 5·8 per cent.†

The results with nitrous gas were of a different kind, and may be deserving of being specially noticed.

The blood used was that of the sheep, prepared in the usual manner. The experiments were made during the very cold weather which prevailed in the beginning of the year, and the difference of colour between the venous and arterial blood was very strongly marked.

1st. *On Arterial Blood.*

1. Fifty-three measures of this blood were agitated over

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* Nitrous oxide I find is absorbed in about the same proportion by venous and arterial blood, and by the serum of blood, and also in about the same proportion by water. Thus, at the temperature of 45° over mercury, using the blood and serum of the same animal (the sheep), thirty-two measures of each absorbed twenty-two measures of this gas, and thirty-two of distilled water absorbed 21·5.

† Supposing the gas decomposed, the phosphorous uniting with the oxygen in the blood, the apparent smaller absorption by arterial than by venous blood is what might be expected, on the idea that the former kind of blood contains most oxygen.

mercury with forty-six measures of nitrous gas; there was a diminution of volume of seventeen measures.

2. Fifty-three measures of the same blood (another portion) were agitated with nine of oxygen; two measures were absorbed.

3. Fifty measures of this blood, so treated with oxygen, were agitated with forty-seven of nitrous gas; there was a diminution of twenty-two measures.

2nd. On Venous Blood.

1. Fifty-three measures of this blood were agitated with fifty of nitrous gas; there was a diminution of ten measures.

2. Fifty-three measures (another portion) were agitated with ten of oxygen; five measures were absorbed.

3. Fifty-one measures of blood so treated were agitated with forty-nine of nitrous gas; there was a diminution of seventeen measures.

The residual air in each instance was examined and was found to be a mixture of nitrous gas and azote without carbonic acid gas. The azote, it may be presumed, was introduced with the nitrous gas; it was in the same proportion as that which adulterated it, viz. about four per cent. That the residual air was free from carbonic acid was inferred from the circumstances, that in comparative experiments with and without addition of a portion of solution of caustic alkali, there was no difference in the proportion of nitrous gas absorbed: and it was corroborated by another circumstance, viz. that after the absorption of the nitrous gas, the blood was capable of absorbing seventy-five per cent. of carbonic acid gas*; and further by the result that when nitrous gas is added not to saturation, the whole of it is absorbed†.

As regards the blood itself, the colour of both venous and arterial was altered; both were rendered darker and browner, as if a minute quantity of nitric acid had been added to them, a change long known to be occasioned by nitrous gas. In the degree of change there was however a difference; in the instance

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* To some venous blood of a sheep which absorbed 182 per cent. of carbonic acid gas, so much of a solution of pure hydrate of potash was added, that it absorbed 218 per cent. of the acid gas; seventeen measures of this blood with excess of alkali, agitated with fifty-one of nitrous gas, absorbed 5·5 measures; sixteen of the blood without the excess of alkali absorbed five measures: thirty-four measures of carbonic acid gas were added to the latter, the excess of nitrous gas being left in the tube; on agitation twelve measures of the carbonic acid were absorbed.

† In one experiment eighty-four measures of the venous blood of the sheep were agitated with ten of nitrous gas over mercury; the whole of the gas was absorbed: twelve of oxygen were added; on agitation again there was no further absorption.

of arterial and of oxygenated arterial blood, it was more strongly
marked than in that of the venous.

In conjunction with the unsuccessful attempts with the other
gases already mentioned, do not the results just described indi-
cate that a portion of oxygen exists in the blood, not capable of
being extracted by the air-pump, and yet capable of entering
into combination with nitrous gas, and which exists in largest
proportion in arterial blood ? Unless this conclusion is adopted,
it must be supposed either that the nitrous gas which disappears
is decomposed, or that it combines directly with the red parti-
cles ; neither of which suppositions is well supported by facts.
The circumstance that no azote is disengaged, is not favourable
to the idea that the nitrous gas is decomposed ; and the differ-
ence of effect in the instances of venous and arterial blood, and,
after and before agitation with oxygen, is not in accordance
with the notion of direct combination.　I may mention another
fact which seems to have the same bearing ; serum, which does
not absorb oxygen, I find also does not absorb nitrous gas, ex-
cepting in about the proportion in which water absorbs it : and,
further, in corroboration, I may mention, that as blood putrefies,
whether arterial or venous, its power of absorbing nitrous gas
diminishes; and that it is also diminished by being agitated
with phosphuretted hydrogen.　Thus the arterial blood of a
sheep, which before agitation with phosphuretted hydrogen ab-
sorbed 45·3 per cent of nitrous gas, after agitation with it
absorbed 7·4 per cent less ; and, after it had become putrid, it
absorbed twenty per cent. less.　According to my observations,
arterial blood does not lose its peculiar florid hue under the
action of the air-pump.　Is not this also in favour of the above
inference, that a portion of oxygen is retained by the blood re-
sisting extraction by the air-pump ?　I find also that when
venous blood is agitated with oxygen and subjected to the air-
pump, it, in like manner, retains its acquired florid vermillion
hue, and likewise a power of absorbing an additional quantity
of nitrous gas.

VII. *When oxygen is absorbed by the blood is there any pro-*
duction of heat ?

To endeavour to determine this point, of so much interest in
connexion with the theory of animal heat, a very thin vial, of
the capacity of eight liquid ounces, was selected and carefully
enveloped in bad conducting substances, viz., several folds of
flannel, of fine oiled paper, and of oiled cloth.　Thus prepared,
and a perforated cork being provided, holding a delicate ther-
mometer, two cubic inches of mercury were introduced, and
immediately after it was filled with venous blood, kept liquid as
before described.　The vial was now corked and shaken ; the
thermometer included was stationary at 45°. After five minutes
that it was so stationary, the thermometer was withdrawn, the

vial closed by another cork was transferred inverted to a mercurial bath, and $1\frac{1}{4}$ cubic inch of oxygen was introduced. The common cork was returned, and the vial was well agitated for about a minute; the thermometer was now introduced, it rose immediately to 46°, and continuing the agitation it rose further to 46·4, very nearly to 47°. This experiment was made on the 12th of last February on the blood of the sheep.

On the following day a similar experiment was made on the venous blood of man. The vial was filled with eleven cubic inches of this blood, its fibrin broken up in the usual manner, and with three cubic inches of mercury; the temperature of the blood and mercury was 42·5, and the temperature was the same after the introduction of three cubic inches of oxygen. The temperature of the room being 4.8, a fire having shortly before been lit, the vial was taken to an adjoining passage where the temperature of the air was 39°. Here the vial was well agitated, held in the hand with thick gloves on as an additional protection. After about three quarters of a minute the thermometer in the vial had risen a degree, viz. to 43.5.

On the 14th of the same month a third experiment was made on venous blood from the jugular vein of a sheep. The vial was filled with 3·5 cubic inches of mercury and eleven cubic inches of blood. The thermometer in the bottle, left five minutes, was stationary at 49°; the temperature of the mercurial bath was 49°; the air of the room was 52°; a thermometer with its bulb moistened was 45°, which I mention because the outer covering of the vial was moistened with some blood which had overflowed. After three cubic inches and a half of oxygen had been introduced, before agitation, the thermometer was still 49°. The bottle was briskly shaken for about half a minute; now, on observing the thermometer, it was found at 50°; the vial was again agitated; there was no further increase of temperature: after ten minutes it had fallen to 49°.

I shall relate one experiment more, and that on arterial blood. It was made on the 14th of February, and in the same manner as those on the venous blood. Before and after the introduction of the oxygen, the blood, which was from the carotid artery of the sheep, was 45°; after agitation with oxygen it rose to 45·5: this was done when the temperature of the air was 39°.

In a former part of this paper I proposed to recur to the question, Is the fixation of oxygen in the blood attended with the formation of carbonic acid gas? The change of colour accompanying the fixation of oxygen by the blood, so different from that produced by carbonic acid, and the effect of nitrous gas before and after, seem to be most in favour of the idea, that the oxygen, in the first instance, is simply absorbed, and that the heat evolved is merely the effect of its condensation; or,

that if any of it enters into immediate union with the carbon, it is only a small part of the whole.

VIII. *Conclusion.*

Should the results detailed in the preceding pages be confirmed on repetition, they can hardly fail having some effect on the theory of respiration and animal heat.

As regards the former, they appear to me to tend to show that the lungs are absorbing and secreting, and perhaps exhaling organs, and that their peculiar function is to introduce oxygen into the blood and separate carbonic acid from the blood.

As regards animal heat, they appear to favour the idea, that it is owing, first, to the fixation or condensation of oxygen in the blood in the lungs in conversion from venous to arterial ; and secondly, to the combinations into which it enters in the circulation in connexion with the different secretions and changes essential to animal life.

In illustration of what I imagine the secreting power of the lungs, I may mention the difference of effects in an instance of death by strangulation, and another by exhaustion of air from the lungs by the air-pump. A full grown Guinea Pig was the subject of experiment in each. The one killed by strangulation died in about a minute after a cord had been drawn tightly round its neck ; the other, placed on the plate of the air-pump and confined by a receiver just large enough to hold it, lived about five minutes after the exhaustion had been commenced, the pump the whole time having been worked rapidly. The bodies were immediately examined. The heart of the strangled animal was motionless ; it was distended with dark blood ; twelve measures of the blood, broken up and agitated with twenty-nine of carbonic acid gas, absorbed eighteen measures, or 150 per cent. The heart of the other Guinea pig was also distended with blood, but of a less dark hue. Its auricles were feebly acting; the lungs were paler than in the former, and more collapsed : ten measures of blood from the heart, broken up and agitated with fifty of carbonic acid gas, absorbed thirty-seven measures, or 370 per cent. !

Further, in illustration of this supposed secreting power of the lungs, I might adduce the condition of the blood in disease, and in instances in which I have examined it after death from disease, in the majority of which I have found the blood loaded with carbonic acid, as indicated both by the disengagement of this gas, when the blood was agitated with another gas, and by the comparatively small proportion of carbonic acid which the blood was capable of absorbing. This condition of the blood, in relation to carbonic acid, I believe to be one of great interest and importance, and capable, when further investigated, of throwing light on many obscure parts of pathology, and es-

pecially on the immediate cause of death, and that happy ab-
sence of pain in dying which is commonly witnessed.

As regards an exhaling power, which I suppose the lungs
may possess, I conceive it may·be exercised occasionally under
peculiar circumstances—circumstances, in the first instance,
favouring an accummulation of carbonic acid gas in the blood, as
undue pressure of any kind, and, in the second instance, cir-
cumstances of a different nature, connected with the removal
of undue pressure, admitting thereby the excess to pass off.

The view which I have alluded to relative to the production
of animal heat, is, I believe, capable of explaining very many
particulars of animal temperature in different classes of animals,
and both during life, in health, and disease, and in a state of
hybernation and after death.   If correct, this it must necessa-
rily do, theory being merely an expression of facts, and truths
in nature being perfectly consistent.

Fort Pitt, Chatham, May 30, 1838.

Experimental and Theoretical Researches in Electricity, Magne-
   tism, &c. Sixth Memoir. By WILLIAM STURGEON, *Superin-*
   tendent and Lecturer to the Royal Victoria Gallery of Practi-
   cal Science, Manchester : formerly Lecturer on Experimental
   Philosophy at the Hon. East India Company's Military Aca-
   demy, Addiscombe &c. &c.

315. The present memoir will contain theoretical explana-
tions of several electro-magnetic phenomena, in continuation
of those applications of the theory which I have endeavoured
to develope in the first volume of these Annals : and also des-
criptions and theoretical explanations of some other experiments
not hitherto published, excepting in my lectures on these subjects.

326.   It is well known that if an electric current be trans-
mitted through a pendant wire W, fig. 1, hanging by a
loop, to another wire P ; and its lower extremity dips into
an annular cistern of mercury, through the centre of which
passes the magnet S, N ; the wire, with its current, will
rotate round the magnetic pole to whose influence it is exposed :
and the *direction* of rotation will depend upon the direction in
which the current flows through the pendant wire, and upon
the character of the magnetic pole under whose influence that
'current is placed.   If the direction of the current be down-

wards, as indicated by the
arrow, and the marked end
of the magnet be upwards,
as in the figure, the direction
in which the wire will rotate,
is towards the left hand of a
person situated in the centre
of motion, or in the situa-
tion of the magnet in the
figure. But if the current
should flow upwards through
the pendant wire, while the
position of the magnet re-
mains unchanged, the wire
will rotate in the opposite di-
rection to the former, or to-
wards the right hand of a
person situated in the centre
of motion. And, on the other
hand, if, instead of changing
the direction of the current
from downwards to upwards
in the pendant wire, we were
only to invert the position of

Fig. 1.
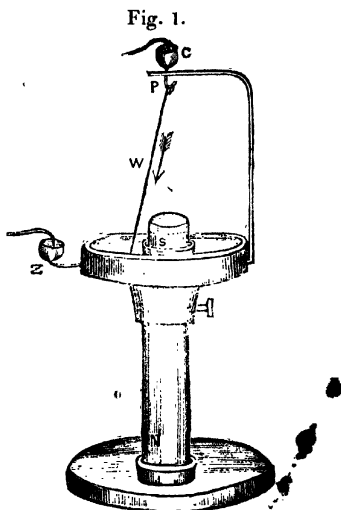
the magnet presenting its *unmarked* pole to the wire, the direction
in which the wire would rotate, would be as decidedly inverted
as by an inversion of the current whilst subject to the action of
the marked pole only.

317. These rotations of a wire carrying an electric current,
were first made by Dr. Faraday, and were supposed to be
favourable to an hypothesis suggested by the late Dr. Wollas-
ton; in which he supposed that the electrical current proceeds
by a circuitous route round the conducting wire, producing
what that philosopher termed *vertiginous force*. But Professor
Barlow, I believe, was the first person who gave any satisfac-
tory explanation of these rotatory motions in the 2nd edition of
his admirable work on "Magnetic Attractions, &c." Mr.
Barlow considers the force in the wire to be tangential to every
point on its bounding surface; and I have already shown, in
the theory before mentioned, the probability of this force being
identical with that which constitutes the powers of ferruginous
magnets and of loadstone: and by admitting that identity, the
electro-magnetic, and magnetic electrical phenomena, become
as susceptible of satisfactory explanation as any other class of
phenomena within the whole range of experimental science:
whereas, independently of such *magnetic* tangential force, I am
not aware of any hypothesis that becomes generally applicable
to the phenomena in question.

318. The effect of the longitudinal forces of two magnets operating on one another may be exemplified by the following experiment. Let the magnet N S, fig. 2, be suspended over the

Fig. 2
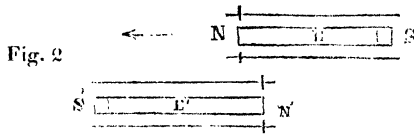
magnet N S in such a manner that it can move freely in a horizontal plane, whilst the lower magnet N S remains fixed. It will commence moving in the direction indicated by the small dart; and if not checked it will be taken in that direction until its pole N has passed, to some distance over the pole S, of the lower magnet: it will then return, and after a few oscillations it will remain at rest directly over the lower magnet, having their equators E and E' in the same vertical plane. The straight lines with cross heads on the outside of the two magnets would represent the resultants of their longitudinal forces: (see plate ix r. 61, 68, vol. 1.), which balance each other when the magnets have assumed a state of repose in their last mentioned positions. There is still, however, a lateral force exerted between the two magnets, which would bring them into contact with each other were they at liberty to do so.

319. The longitudinal and the lateral forces of two steel magnets, as above described, play precisely the same part on each other as the tangential magnet forces of two wires transmitting electric currents, as shown in fig. 71, plate ix, vol. 1 : or as the longitudinal and lateral forces of a steel magnet and the tangential forces of a conducting wire; as illustrated by the fig. 3.

Fig. 3
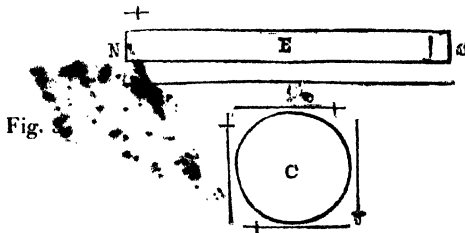
Let the circle C represent the section of a conducting wire, carrying an electric current *from* the spectator to below the paper on which the figure is drawn. The resultant tangential

magnetic forces of that wire would be represented by the four lines, with cross heads, placed about the circumference of the section. If, now, this wire were to be exposed to the magnetic forces of a bar of steel, N, E, S, with the equator E of that magnet in the same plane as that in which the axis of the wire is placed, as represented by the figure ; then the longitudinal forces of the magnet, and the vicinal tangential forces of the wire would be in equilibrio ; and the wire would have no tendency to move either towards the pole S, or towards the pole N, of the magnet. But if the wire were perfectly free to move in other directions also, it would be drawn towards the equator of the magnet, in a line perpendicular to the axis of the latter ; or, if you please, in the plane of the magnetic equator of the steel, till they come into contact with one another, in precisely the same manner as two magnets are drawn together by their lateral forces, as above described. If, however, the conducting wire C, were to be removed from the equator E of the magnet, towards either of the poles N or S, and then set at liberty ; it would again return to its former position in the plane of the ferruginous magnetic equator, where it would repose as before. These phenomena are in strict uniformity with those displayed by two ferruginous magnets, when in operation on each other, and are, no doubt, governed by the same laws.

320. The tangential magnetic force of the conducting wire, and the longitudinal force of the steel bar, conspire, in all cases, to give a tendency to place the axis of the current in the plane of the magnetic equator of the steel : as was shown by M. De la Rive, with his floating miniature battery. Hence it is, that, by a series of fruitless attempts, in consequence of the mode of suspension of the wire W, in the already describ[ed] experiment, fig. 1, to place the axis of the current in the p[lane] of the equator of the magnet, there results a rotation of that current, and the pendant wire through which it traverses.

321. When the current is very powerful, the pendant wire is frequently thrown with great violence out of the mercury; which, of course, cuts off the current for a m[o]ment, until the wire falls down again and closes the circuit i[n]   [l]y ; at which time, the current recommences a[nd]   [r]eceives a new impulse, which drives it again out of t[he] me[rcury] &c., and by a series of these impulses, the wire prog[resses] round the magnet, and thus performs a kind of ju[mping rot]atory motion. When, however, the current is not too powerful, the wire is never thrown out of the mercury, and the rotation is pretty steadily performed.

322. A very uniform rotation of the electric current roun[d] the pole of the magnet is obtained by dividing it into two chan[n]els, in the manner shown by the apparatus represented by fig. 4

where each of the two pen-
dant wires W W, W W, carry
a portion of the electric mat-
ter, from the cup C to the
mercury in the annular trough
through which the magnet
passes. By these means, the
resultant forces which tend to
place one of the wires at right
angles to the axis of the mag-
net are prevented from throw-
ing that wire out of the mer-
cury, by similar counteract-
ing forces on the opposite side
of the magnet; hence, both
pendant wires rotate on the
point P, with a very steady
uniform motion.

323. If, instead of both
branches of the wire being
turned downwards, as in fig.
4, one of them be turned up-
wards as in fig. 5, the pendant
branch may be nicely coun-
terpoised by moving the light
ball on the upper branch, to a proper dis-
tance from the pivot, allowing the depending
branch W. W. to preponderate a little in
order that its lower extremity may touch the
mercury in the annular cistern when the
whole is at rest. By this arrangement the
electric current may be made to traverse the
pendant wire W, W, without interfering with
the other. If, now, the copper of a single
connected with the upper cup,
with the cistern of mercury,*
the current will proceed downwards through
the wire W, as indicated by the arrow : and
the wire will commence its rotation round the
magnet; but instead of proceeding steadily as
by the apparatus represented by fig. 4, it now
makes a jumping motion, in consequence of
a series of deflections which throw its lower
extremity out of the mercury, into the posi-
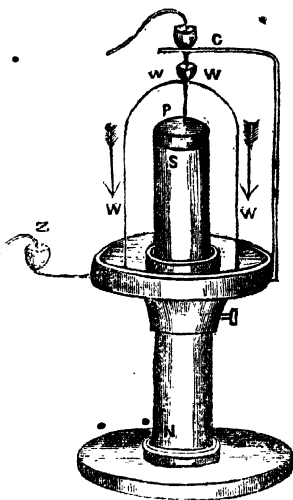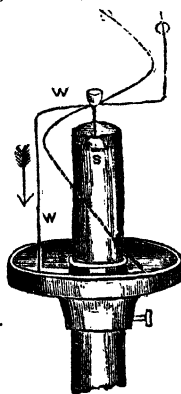tion shown by the dotted line. When the wire,

Fig 4.

Fig. 5.

* Instead of making a direct connection between the battery and mercury
in the cistern, it is often convenient to have a short wire passing through the
side of the cistern, furnished with a small cup for holding mercury, as
seen at Z, fig. 4.

as in this case, has left contact with the mercury in the cistern by the first deflection, the electric current is cut off, and the wires gravitating propensity brings it down again to the mercury, and restores the contact. This done, the current again flows, a new impulse is given, and the wire again thrown out of the mercury as before ; and so on alternately making and breaking contact ; and by a continuation of these deflections, the wire is carried round the magnet, in the same direction as when both branches dip into the mercury.

324. When the electric current is too energetic, the wire is frequently thrown off the pivot. or so tossed about as to spoil the beauty of the experiment ; but by employing an arrangement like that represented by fig. 6, the fact is beautifully exhibited. The moveable part of fig. 6 consists of a horizontal straight brass wire W W, supported on a pivot at its centre, having a suitable pivot hole on its lower side. On the extremities of this wire are suspended two other wires, one on each ; which can revolve on the horizontal one as an axle, in the same manner as carriage wheels revolve upon an axle. In this apparatus, however, the lower branch of each wire is a little heavier than the upper one, by which means it is kept in a vertical position when under the influence of gravity only : but, in consequence of the delicacy of suspension, can be deflected out of that position by the application of a very slight horizontal force. When the positive pole of a voltaic battery is made to communicate with the centre of the horizontal wire W W, and the negative pole with the mercury in the cistern ; the electric current will proceed in both directions along that wire, and down both vertical wires to the mercury, &c. The lower points of the vertical wires are immediately thrown out of the mercury, as represented by the dotted lines in the figure : and by these deflections the whole of the moveable system of wires receives an impulse which produces motion on the pivot. Gravity brings the deflected wires down again to the mercury, and the current again traverses the wires ; another deflection is produced, and another impulse communicated to the moveable system on the pivot: and in this manner the whole is carried round the pole of the magnet. The results produced by this piece of apparatus have a very singular effect on the mind ; for as the lower points of the pendant wires are always thrown out of the mercury in the same directions as that in which the whole progresses round the magnet, and as they are not always thrown out at the same moment, they give the idea of a man walking, or pumping in a circle round a fixed central object.

Fig. 6.
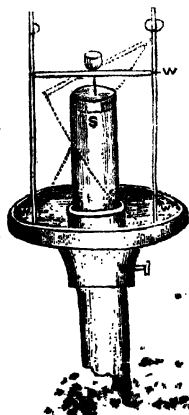
325. The experiment may again be varied in a very pleasing manner, by applying a wheel, instead of a wire, at each extremity of the horizontal piece which revolves on the magnetic pole. By this arrangement the wheels revolve in the mercury, whilst the whole system is carried round the magnet; and that which appears most singular in this experiment is, that the wheels rotate in the mercury in the opposite direction to that in which they would on a solid floor or road, to be translated in their orbit round the magnetic centre in the same progressive direction. Fig. 7 is a representation of an apparatus of this kind; which, with the arrangement there shown revolves round the magnet from right to left: whilst the wheels revolve on their pivots in the direction of the arrows.

Fig. 7.
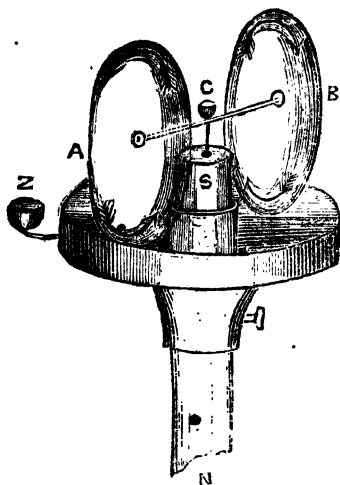
326. As the revolutions of magnets around fixed conductors carrying electric currents, are effected by the same forces as those which are productive of the above named phenomena, the same laws apply in both cases: and the two classes of phenomena may be varied accordingly. When I first rotated a large magnet round a conductor, by suspending it on a pivot, I met with considerable difficulty in keeping it parallel to the conductor round which it was intended to revolve, until I hit upon the method of bridleing it to the conductor by a loop of thin copper wire as shown at K in figure 8, which represents the apparatus that I employed for rotating two magnets round two fixed conductors, at the same time. Before I adopted this plan, the magnets were thrown oblique to the axis of the conductor, the moment that the battery connexions were made; and although absolutely taken round the conductor, they were tossed about in a singular manner, and their revolutions were far from being steady and uniform.

327. This piece of apparatus, which, amongst many others, I presented to the society of arts in the year 1825, is now very well known; being manufactured by almost every instrument maker in London from that period till the present time. The following is a description of the apparatus. A A, are two vol-

Fig 8.
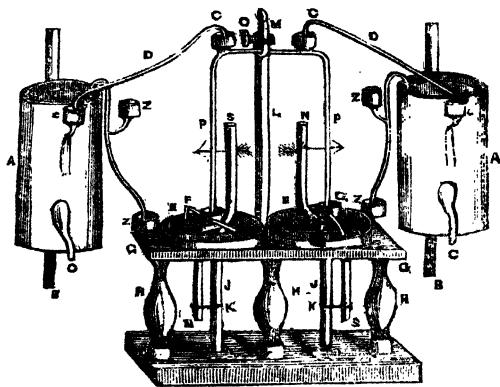
taic batteries, each of which consists of a cylindrical copper jar,
in which is placed a cylinder of zinc, properly insulated from
from the other metal. To the side of each copper jar is sol-
dered a short wire C, and a small brass cup C for holding mer-
cury. Each zinc cylinder is also furnished with a copper wire
and brass cup Z Z, which wires and cups are for the purpose of
making connections between the battery and various pieces of
apparatus. Each battery is also furnished with two brass loops,
through which pass freely the stout brass rod B; by means of
which it can be adjusted to any required height, and kept firmly
in any position by means of a set screw, which works in the
upper loop. The vertical brass rods are screwed into heavy
wooden feet, but are broken in the figure to prevent an unne-
cessary occupation of the paper. The base of the apparatus is ·
a stout rectangular mahogany board, from which rise three
boxwood pillars H, H, H, which carry a rectangular stage G, G,
in which are two circular holes for the reception of the annular
cisterns E, E. From one side of the stage, rises a brass
pillar L, M, for the support of the bent wire P, P, which can
be adjusted to any required height by the thumb screw G.
The bent wire P, P, is also furnished with two cups C, C, for
holding mercury. The annular cisterns E, E, are also for hold-
ing mercury, which is connected with other portions of mercury
in the small cups Z, Z, by means of short brass wires, which
pass through the sides of both the cups and the cisterns. N, S,
on the right, and S, N, on the left, are two steel magnets, which
by being bent, as shown in the figure, and having a pivot hole
on the lower side of the bend, are supported by, and rotated

upon, the fine-pointed wires J, J. These magnets are kept in a vertical position by means of looped wires, K, K, which are fixed to the magnets, but not to the supports J, J, which pass freely through the loops. On the upper side of the bend of each magnet is soldered a brass cup for holding mercury, and to this cup is attached a thin wire F, pointed at its outer extremity and bent, as in the figure, so as to be immersed in the annular mass of mercury in the cistern E. If now the two batteries be connected with this apparatus, as shown in the figure, the electric current will run from the copper jars along the conducting wires D, D, to the cups C, C ; thence downwards through the wires P, P, to the mercury in the small cups on the bend of the magnets. Thence through the bent wires F, F, to the mercury in the cisterns E, E ; thence to the mercury in the cups Z, Z, and up the wire Z to the zinc cylinder in the battery. The revolutions of the magnets round their respective wires P, P, are performed in opposite directions, because of *different* poles being exposed to *similar* electric currents. By changing the direction of the currents in the wires P, P, of course the magnets revolve in the opposite direction to the former.

318. The batteries may be so managed as to have their forces divided into two currents each, one down the wire P, and the other up the support J. By these means both poles of the magnet are operated on at the same time, and the revolving motion is much brisker than when only one pole is under the influence of an electric current.

329. The theory of electro-magnetism which I have proposed serves to explain very satisfactorily the vibrating motions of a conducting wire, hanging pendant between the poles of a horseshoe magnet, as first shown by Mr. Marsh. Let us permit the circle C, fig. 9,

Fig. 9.

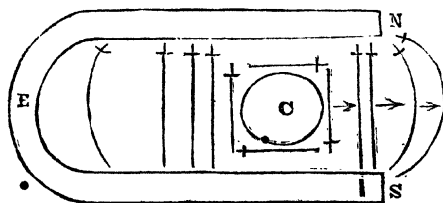to represent a transverse section of the pendant conducting wire, hanging between the north and south polar branches of the horse shoe magnet N, E, S, and that wire to be carrying an electric current *downwards*. By such an arrangement, the lower end of the pendant wire would be thrown out of the bed of mercury, in which it was immersed, in the direction of the three small darts. This deflection of the wire breaks the elec-

tric circuit, and the wire falls down to the mercury and again closes the circuit. This done another impulse is given to the wire which is again deflected, and again the circuit broken : and in consequence of a succession of alternate closings and openings of the electric circuit, the wire is kept exhibiting its vibratory motions. If either the position of the magnet, or the direction of the current be reversed, the pendant wire is impelled in the opposite direction, or towards the bend E of the magnet.

340. Now to account for these motions of the wire, we have only to look at the arrangement of the resultant lines of the two systems of magnetic force, and examine their operations on each other, upon the principles of purely magnetic action. The four right lines with cross heads around the section of the wire C, may represent four resultant tangential electro-magnetic forces, and the long lines, (some straight and some curved) with cross heads, may represent resultants of the magnetic forces of the steel. (See also, figs. 62 and 63, plate ix. vol 1.) Now as the two electro-magnetic resultants which are parallel to the two branches of the magnet, have their similar poles in opposite directions, they neutralize each other as regards the action of the magnetic resultants of the steel : and have, therefore, no influence in moving the wire either one way or the other. Hence the two electro-magnetic resultants which are parallel to the magnetic resultants of the steel, are they only which are influenced by the latter, and tend to give motion to the wire. Then accordingly with the laws of magnetics, we find that on the right hand, as the tangential electro-magnetic resultant has its poles in the opposite direction to those magnetic resultants of the steel, which are situated on the same side of the wire, they will mutually attract each other, and as the wire is free to move, it will be drawn in that direction which is indicated by the three small darts. Again, as it is found that, on the other side of the wire, the tangential electro-magnetic resultant has its poles placed in the *same* direction as those of the vicinal magnetic resultants of the steel, they mutually repel each other, and consequently tend to drive the moveable wire C, in precisely the *same direction* as that in which it is drawn by the attractive forces on the other side of it. Hence as it is impelled in one and the same direction by both the attractive and the repulsive forces to which it is subjected, it must necessarily move in that direction alone.

If the current were to be reversed in the wire C, its electro-magnetic forces would be reversed also, and the attractions and repulsions between those forces and the forces of the magnet would be transferred to the opposite sides of the wire, and would conspire in carrying it towards the equator E, of the magnet. And it is very obvious, by mere inspection of the figure, that any change in the position of the magnetic resultants of the steel with regard to the electro-magnetic resultants of the wire,

would be productive of corresponding changes in the direction of the movements of the wire.

341. To apply this theory to the explanation of the rotations which a magnet performs on its own axis when subjected to the electro-magnetic forces of a current, requires a knowledge of a fact which I produced some years ago, whilst contemplating those rotations. As the rotations of a magnet on its axis are performed in the same direction as its revolutions round a fixed conductor, when the electric current is in one and the same direction in both cases, it appeared highly probable that both phenomena are produced by similar operations of the two systems of magnetic forces : and if so, then the electric current must necessarily traverse the axis of the magnet ; and operate on the magnetism of its exterior, as if the latter were composed of a surrounding system of small parallel magnets, whose individual axis would be all parallel to the axis of the steel, which could not only be the axis of the magnetic system, but also coincide with the axis of the electric current. Hence, by such a system, both the current and the magnets rotated together in the same piece of metal ; and if this were absolutely the case, I could see no reason why a system of magnets fixed to a central conducting wire, should not rotate upon the axis of the whole.

342. To put this hypothesis to the test of experiment, I fitted up an apparatus which fig. 10, may very conveniently represent.

To a stout vertical brass wire two bar magnets N S, N S, are fixed, but without being in metallic contact with it. Round the centre of the magnets is placed a brass equatorial band or hoop C, which has a metallic connexion with the axial wire : but is insulated from the magnets. The axial wire is finely pointed at both extremities for the purpose of rotating freely in pivot holes ; one of which is in a little brass stud screwed into the base board, and the other in the lower end of a brass screw that passes through the horizontal arm of the brass support Z Z. The upper end of that screw is furnished with a cup for holding mercury. When the zinc side of a single voltaic pair is in connexion with the extremities of the axial wire, and the copper side in connexion with the equatorial band, by the spring wire C, which presses gently against it, two distinct electric currents will traverse the axis of the system, each current occupying one half of it from the equatorial to the poles or extremities of the

Fig. 10.
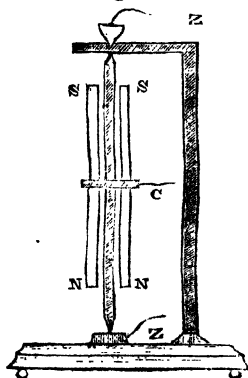
axial wire: and the whole system commences a rapid rotation, on its axis, and in the same direction as I first showed that a bar magnet rotates on its axis by similar currents traversing it from its equator to its poles*. .

343. Having succeeded so satisfactorily with two magnets attached to the wire, I was desirous of carrying the analogy still farther, by making the system of magnets more complete. For this purpose I made eight narrow steel magnets, and distributed equally around the axial wire, by tying them against the curved surfaces of two cylinders of cork, through the centre of which the axial wire passed. The north poles of these magnets were all placed in one and the same direction ; and, consequently, all the south poles in the other. By this arrangement I had a compound hollow octagonal magnet ; whose axis was the axis of the vertical wire.

The rotations with this system were precisely the same as with the former system of two magnets. When the system is inverted without altering the battery connexions, as is easily done by the apparatus represented by fig. 10, the rotation appears to a spectator to be inverted also : though, in reality, it still proceeds in the same direction with regard to its own poles.

344. From the uniformity in the results with all these differently formed magnets, it is fair to conclude that the rotations of a solid bar magnet on its axis, are accomplished by a similar distribution of the magnetism of the bar around axial currents, to that in the compound systems above described : (341, 342, 343), and that the whole of this class of phenomena result from a series of mutual efforts between the magnetic forces of the steel, and those of the conducting wire or its currents, to balance one another : or, in other language, from a mutual tendency of the magnet and the electric current to place the axis of the. latter in the plane of the equator of the former.

345. It is now some years since I first published my views respecting the pulsatory character of electric currents : I now find that others are entertaining similar notions. By admitting electric currents to be pulsatory, we should have another element to assist us in the explanations of electro-magnetic rotations; but I have not availed myself of that element lest it should not yet be generally admitted.

Royal Victoria Gallery of Practical
    Science, Manchester.

* See vol. 43, of the Transactions of the Society of Arts, Commerce, &c., also Phil. Mag.

On the Electricity of Effluent Steam. No. II. By W. G. Arm-
strong, Esq.*

In concluding my last communication on the above subject,
I alluded to experiments which had then been commenced to
try the effect of insulating the boiler and entirely condensing the
issuing steam. These experiments have since been completed,
and many others have also been tried which subsequently sug-
gested themselves, all of which I shall now proceed to describe.

The insulation of the boiler was effected by lifting the en-
gine with screw-jacks, until the wheels were raised about six
inches above the rails, and then supporting it upon four insu-
lators which rested upon logs of timber. Each insulator con-
sisted of three separate pieces of baked wood, coated with pitch,
and having layers of pitch and brown paper placed between
them. The middle piece was made larger than the other two,
so as to project beyond them, and thereby increase the surface
without adding to the height of the insulator, which would have
been dangerous; and the three pieces when put together formed
a block.

As soon as the engine was placed on the insulators, the boiler
was filled with water, and the fire lighted, and as the pressure
gradually rose in the boiler, the steam was occasionally suffered
to escape.

The engine indicated no electricity whatever so long as the
steam was confined in the boiler, but became negatively electri-
fied as soon as any escape was permitted. A very trifling escape
proved sufficient to render the negative electricity of the boiler
sensible, and when the steam was very freely discharged the
negative development became exceedingly powerful. The sparks
never much exceeded an inch in length, but were very large
and brilliant, and, owing no doubt to the magnitude of the body
from which they were drawn, they produced effects fully equal
to those obtained by the use of an average-sized Leyden jar.
Some idea of their potency may be formed from the fact, that
when not more than half an inch in length, they easily ignited
a piece of cotton wool filled with powdered resin.

, The greatest care was taken to ascertain whether the extent
of the electrical development was at all dependent upon the
density of the steam in the boiler; and it was found that when
the discharge from the valve was so regulated in the different
trials as to render the actual quantity or *weight* of steam ejected
in a given time as uniform as possible, the negative electricity
of the boiler increased *a little* with the pressure, but the posi-

* From the Philosophical Magazine.

tive electricity, drawn by the pointed conductor from the issuing
steam, increased *enormously* as the density of the steam was aug-
mented. After the fire had been extinguished at the close of
the experiments, and the pressure had subsided to six or eight
pounds on the square inch, sparks could no longer be obtained
from the conductor held in the steam, but the negative electri-
city of the boiler continued to produce sparks until the steam
was entirely exhausted, or as nearly so as possible. These re-
sults appear to indicate that a jet of high-pressure steam is not
in reality much more electrical than one of low-pressure steam,
but merely that the electricity of the high-pressure jet is more
easily collected.

The insulation of the boiler undoubtedly had the effect of
diminishing the positive electricity obtained from the steam, but
not to such an extent as might have been anticipated.

A glass tube A (fig. 1), with a cock C affixed to it, having
been inserted in the boiler in the manner described in my last
communication,* another glass tube B, about four feet long, was
attached to the cock, and supported at the end furthest from the
boiler by a glass rod fixed in the insulating stool. A small brass
cylinder D having a number of pointed wires projecting from
one end into the inside, as shown in the section (fig. 3), was

Fig. 1.

Fig 3
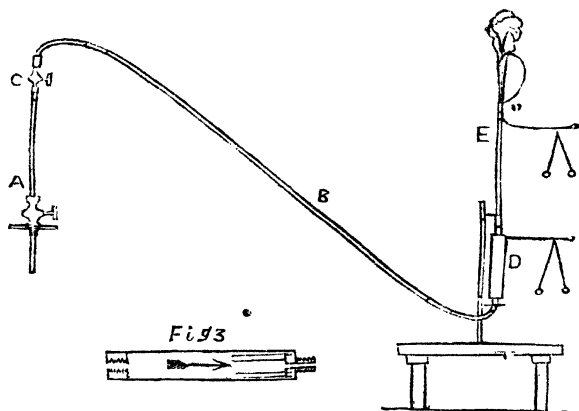
then joined to the glass tube B, and to this cylinder was added
a third glass tube E, so as to extend the channel through
which the steam was to be conveyed. A wire with forked

points was affixed to the top of the tube E for the purpose of collecting the electricity of the jet, and from this wire a pair of pith-balls was suspended. Another pair of pith-balls was in like manner suspended from a wire screwed into the brass cylinder, all which arrangements will be clearly understood by reference to the figure. The cylinder, and a great part of each of the tubes, were enveloped in flannel, so as to prevent condensation as much as possible. The chief object of this apparatus was to test the electrical condition of the steam before it issued into the air, both when the boiler was insulated and when it was connected by a conductor with the earth. An iron bar was then placed against the engine to establish a communication between it and the ground, and the cock being opened, a current of steam mixed with water issued from the tube E. Under these circumstances both pairs of balls remained stationary, the electricity being probably carried off by the water which escaped with the steam. As the tubes became heated the quantity of water discharged with the steam was reduced to a mere spray, and both pairs of balls then slightly diverged with positive electricity, the upper pair expanding rather more than the lower pair; but upon partially closing the cock both pairs of balls expanded fully *three times as much* as they had done when the cock was fully open; and then gradually converged, arriving at their original position after the lapse of about a minute.

I was at first exceedingly puzzled how to account for this singular effect of attenuating the steam, but I now think it may be explained as follows. It is probable that the moisture of the steam when the cock is fully open is sufficient to enable it to conduct a great part of the electricity of the jet back again to the boiler; but that as soon as the temperature of the steam is reduced by the attenuation below the temperature which the tubes have previously acquired from the high-pressure steam, its dryness is so far increased as to prevent the transmission of electricity from the jet to the boiler, but not from the jet to the cylinder, which is a much nearer object. As the tubes cool down to an equality with the steam the dampness returns, and the electricity is again in a great measure carried back to the boiler.

The iron bar was then removed from its contact with the boiler so as to restore its insulation, and the engine was rendered intensely negatively by a copious emission of steam from the safety-valve. The cock being again fully opened, both pairs of balls diverged strongly with *negative* electricity; and by diminishing the escape from the valve to a certain point, and then partly closing the cock, the upper balls indicated *positive*, and the lower ones *negative* electricity. When the escape from the valve was entirely prevented, and the cock fully opened,

both pairs of balls remained in a collapsed state, but on partly opening the cock they both diverged for a short time with positive electricity to much the same extent as they had previously done while the boiler communicated with the earth by means of the iron bar.

It is important to state that when the balls were repelled with *negative* electricity, they collapsed very considerably when the cock was touched by the hand, but when they diverged with *positive* electricity no effect was produced by touching the cock, which strongly favours the supposition that the *positive* electricity manifested by the lower pith-balls, was conducted to the cylinder from the jet.

A coil of lead pipe, immersed in a glass jar filled with wet snow, was then placed upon the insulating stool and connected with the tube B, after the brass cylinder had been detached ; and the iron bar was again placed against the boiler to suspend the insulation. Upon opening the cock to the full extent, little or no electricity could be discovered in the coil, but when the cock was partly closed, positive electricity appeared for a short time and then faded away exactly as in the experiments with the brass cylinder. Upon removing the connexion between the boiler and the earth, and raising the valve, the coil became highly negative, but upon closing the valve the negative electricity vanished.

I have little doubt that the predominance of the *negative* over the *positive* electricity in the above experiment is attributable to the conducting power of the steam causing more negative electricity to be conveyed to the coil from the boiler, than the coil would acquire if the steam were a non-conductor. When negative electricity was in like manner so strongly manifested by the pith-balls in the experiments with the brass cylinder, it was noticed, that after closing the cock while the interior of the tubes were bedewed with moisture, scarcely any negative electricity was transmitted to the balls, so that the conducting power must have been in the steam, and not in the mere dampness on the glass.

A vertical glass tube, about an inch in diameter, and between two and three feet long, was then screwed on to the cock in substitution of the tube B. The lower part of this tube contained a number of pointed wires for the purpose of abstracting and imparting to the cock any electricity which the steam might possess on entering the tube. When the cock was now fully opened, flashes of light began to dart through the whole length of the tube, from the cloud above it to the cock, and continued to do so as long as the cock remained open, both when the boiler was insulated, and when it was connected with the earth. The steam in the tube was perfectly transparent, and no moisture could be seen on the inner surface of the glass.

The visible transmission of electricity from the jet to the cock, in this experiment, furnishes convincing proof that the positive electricity which we find in the jet is not developed until the steam assumes the form of a visible vapour; and it shows also that the steam, even in its transparent state, is, as I have already surmised, a tolerable conductor of electricity.

I will now venture to offer such an explanation of the electrical phenomena of effluent steam, as I conceive to be most consonant with the experiments I have described in this and my preceding communication.

Independently of the experimental proofs which have been adduced of the neutral state of the steam up to the instant of its transformation into an opake vapour, the water and the steam must be so thoroughly intermixed in the boiler of a locomotive engine, as to render it impossible for the steam to become electrified with positive electricity without immediately imparting it to the water.

I assume, then, as a fact, established both by reason and experiment, that the steam is in a neutral state in the boiler; and I think that I am equally supported in saying, that it does not exhibit positive electricity, after leaving the boiler, so long as it retains its aeriform nature.

We learn from experiment that a development of negative electricity in the boiler accompanies the emission of the steam; and since the negative development in the boiler is obviously independent of the subsequent condensation of the steam, it follows, that if the effluxion of the steam could be effected without permitting any condensation to take place, we should then have a development of *negative* electricity in the boiler without any simultaneous development of *positive* electricity; and in like manner if the ejected steam were subsequently condensed into water, we should then have a liberation of *positive* electricity without a concomitant liberation of *negative* electricity.

These conclusions are entirely incompatible with the theory of two electric fluids, but are quite reconcileable with the hypothesis of a single fluid. It seems perfectly rational and consistent with analogy to suppose, that the immense augmentation of volume which takes place when water expands into steam of any ordinary density, should occasion a greatly increased capacity for electricity; and consequently, that the quantity of electricity which suffices to produce a neutral or saturated state in water, should be inadequate to sustain that condition when the water is converted into steam. Upon this supposition the steam as it forms in the boiler will absorb electricity from the adjacent conductors in order to acquire a *neutral* or *saturated* state, and when by condensation it again becomes water, the

electricity thus absorbed will necessarily be set free ; and hence the positive electricity which we find in the jet.

Upon the same principle, if the boiler be insulated, the water, the boiler, and the uncondensed steam, will all be rendered negative, provided the steam be permitted to escape, but not otherwise ; for if the steam be confined within the limits of the boiler, the evaporation will not be attended with any *increase of volume*, and the absorption of electricity will in consequence be prevented.   In all these respects the theory exactly accords with observation.

I am bound, however, to admit that the explanation which I have here advanced, involves certain conditions which appear somewhat at variance with experiment.   In the first place the condensation of a given weight of *low-pressure* steam ought, if the absorption of electricity depends upon increase of volume, to liberate more positive electricity than the condensation of an equal weight of *high-pressure steam ;* and in the second place, the expansion of steam from one degree of density to another should, on the same principle, be accompanied by a development of negative electricity.   Notwithstanding, however, what has been advanced in favour of high-pressure steam containing more electricity than steam of low density, I think it quite possible that the reverse of this may be the fact ; for it is not at all improbable that a low-pressure jet may conduct so large a portion of its electricity back again to the boiler as to make it appear to liberate less electricity than a high-pressure jet, while in reality it may develope a great deal more. It is quite reasonable to suppose that the conducting power of steam should be increased by rarefaction ; and besides, in order to make a fair comparison between the quantities of electricity liberated by two jets of steam, it is indispensible that they should discharge equal *weights* in equal times, and to effect this condition it is necessary to discharge a much larger *volume* when the steam is of low pressure, than when it is of great density, and by so doing we unavoidably increase the quantity of unvaporised water swept out of the boiler in conjunction with the steam, and thus improve the communication between the jet and the boiler, and at the same time cause a considerable dispersion of the electricity.

With regard to expansion producing negative electricity, I have certainly never detected such a result; but I much question whether in any of the above experiments expansion was effected without an aqueous condensation sufficient to produce a countervailing effect; or the insulation was ever sufficiently perfect to prevent feeble negative electricity being carried off, or overwhelmed by the positive electricity developed in the jet.

Mr. Nicholson has zealously co-operated with me in the experiments I have described, and it is our intention to make a further attempt to clear up the difficulties which still embarrass the subject.

In connexion with the experiments which I have recently published on the electricity of effluent steam, it occurred to me that it would be well to inquire whether similar effects to those I have described, could be produced by compressing common air in a receiver, and then suffering it to escape in a jet, in the same manner as steam had been discharged from a boiler in the experiments alluded to. With this view, therefore, I condensed about eight atmospheres into a strong vessel, the capacity of which was nearly six quarts; I then insulated the vessel, and discharged the air through a glass tube which I had previously inserted for the purpose.

On the first trial I obtained no indications of electricity whatever, but upon repeating the experiment on a subsequent day, the insulated vessel became so highly electrified, when the air was discharged, as to yield sparks fully a quarter of an inch long. I afterwards tried the same experiment a great number of times, and, strange to say, the electricity of the vessel though, generally *negative*, occasionally proved to be *positive*. Sometimes the electricity was very strong, and sometimes very weak, and frequently I could get no electricity at all. By means of an insulated conductor terminating in a number of points, I also obtained electricity from the ejected air, and found it to be *positive* every time I tried it.

I more frequently succeeded in producing an electrical development when the receiver was cold, and contained a little moisture, than when it was warm and dry, so that it is not improbable that evaporation may even here be the source of the electricity. I am by no means sure, however, that my better success when the receiver was cold and damp, was not mere chance, and I only mention the circumstance as a suggestion to persons who may think proper to repeat the experiment.

Electricity of Steam. By Mr. Condie.

Mr. Condie, says, that, for the purpose of trying for the electricity of steam from one of the boilers belonging to the Blair Iron Works, he placed himself on an insulating stool, consisting of a board resting on three glass bottles. Having in one hand a long iron rod, furnished with four sharp points at the farther extremity, which he held in a jet of steam issuing from the safety valve of the boiler. When the points were held about

10 or 12 inches from the valve, sparks of half an inch in length could be communicated from the knuckles of the operator to those of the surrounding observers : but as the pointed extremity of the rod was raised in the cloud of steam to about six or eight feet above the valve, the sparks became more vivid and increased in length to about an inch and a half ; and were nearly as stunning in their effects upon the arms as the shocks from a moderately sized Leyden jar. In the evening, when the experiment was resumed, the corners of the foot board presented beautiful brushes of electrical light of two or three inches in length, and the hairs of the head and of the clothing or the experimenter became tipped with the luminous matter.   Sparks of two inches long, and of great power were then obtained. These very interesting experiments were made upon the steam of boilers of 32-feet long by 6-feet diameter, each.   The first set of experiments were made when the steam pressure was 12lb ; and afterwards with a pressure of 25lb, per square inch.   The effects obviously increased with the increase of pressure*.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

ROYAL SOCIETY.

Paper read November 19, 1840.

On the Theoretical Explanation of an apparent new Polarity in Light. By George B. Airy, Esq., M.A., F.R.S., *Astronomer Royal.*

In a paper published in the second part of the Philosophical Transactions for 1840, the author explained, on the undulatory theory of light, the phenomena observed by Sir David Brewster, and apparently indicating a new polarity in light.   That explanation was founded on the assumption that the spectrum was viewed out of focus ; an assumption which corresponded with the observation of the author and of other persons.   But the author having, since the publication of that memoir, been assured by Sir David Brewster that the phenomena was most certainly observed with great distinctness when the spectrum was viewed so accurately in focus that many of Frauenhofer's finer lines

* We have made several experiments on the steam from boilers of steam engines, the particulars of which will be given in the next number. EDIT.

could be seen, he has continued the theoretical investigation for that case, which had been omitted in the former memoir, namely, when the spectrum is viewed in focus; and he has arrived at a result, which appears completely to reconcile the seemingly conflicting statements, and to dispel the obscurity in which the subject had hitherto been enveloped.

Description of the Electro-Magnetic Clock. By C. WHEATSTONE, Esq., F.R.S.

The object of the apparatus forming the subject of this communication, is stated by the author to be that of enabling a single clock to indicate exactly the same time in as many different places, distant from each other, as may be required. Thus, in an astronomical observatory, every room may be furnished with an instrument, simple in its construction, and therefore little liable to derangement, and of trifling cost, which shall indicate the time, and beat dead seconds audibly, with the same precision as the standard astronomical clock with which it is connected; thus obviating the necessity of having several clocks, and diminishing the trouble of winding up and regulating them separately. In like manner, in public offices and large establishments, one good clock will serve the purpose of indicating the precise time in every part of the building where it may be required, and an accuracy ensured which it would be difficult to obtain by independent clocks, even putting the difference of cost out of consideration. Other cases in which the invention might be advantageously employed were also mentioned. In the electro-magnetic clock, which was exhibited in action in the apartments of the society, all the parts employed in a clock for maintaining and regulating the power are entirely dispensed with. It consists simply of a face with its second, minute and hour hands, and of a train of wheels which communicate motion from the arbor of the second's hand to that of the hour hand, in the same manner as in an ordinary clock train; a small electro-magnet is caused to act upon a peculiarly constructed wheel (scarcely capable of being described without a figure) placed on the second's arbor, in such manner that whenever the temporary magnetism is either produced or destroyed, the wheel, and consequently the second's hand, advances a sixtieth part of its revolution. It is obvious, then, that if an electric current can be alternately established and arrested, each resumption and cessation lasting for a second, the instrument now described, although unprovided with any internal maintaining or regulating power, would perform all the usual functions of a perfect clock. The manner in which this apparatus is applied to the clocks, so that the movements of the hands of both may be perfectly simultaneous, is the following. On the axis which carries the scape-wheel of the primary clock a small disc of brass is fixed,

which is first divided on its circumference into sixty equal parts; each alternate division is then cut out and filled with a piece of wood, so that the circumference consists of thirty regular alternations of wood and metal. An extremely light brass spring, which is screwed to a block of ivory or hard wood, and which has no connexion with the metallic parts of the clock, rests by its free end on the circumference of the disc. A copper wire is fastened to the fixed end of the spring, and proceeds to one end of the wire of the electro-magnet; while another wire attached to the clock-frame is continued until it joins the other end of that of the same electro-magnet. A constant voltaic battery, consisting of a few elements of very small dimensions, is interposed in any part of the circuit. By this arrangement the circuit is periodically made and broken, in consequence of the spring resting for one second on a metal division, and the next second on a wooden division. The circuit may be extended to any length; and any number of electro-magnetic instruments may be thus brought into sympathetic action with the standard clock. It is only necessary to observe,'that the force of the battery and the proportion between the resistances of the electromagnetic coils and those of the other parts of the circuit, must, in order to produce the maximum effect with the least expenditure of power, be varied to suit each particular case.

In the concluding part of the paper the author points out several other and very different methods of effecting the same purpose; and in particular one in which Faraday's magneto-electric currents are employed, instead of the current produced by a voltaic battery: he also describes a modification of the sympathetic instrument, calculated to enable it to act at great distances with a weaker electric current than if it were constructed on the plan first described.

ROYAL VICTORIA GALLERY OF PRACTICAL SCIENCE,
MANCHESTER.

CONVERSAZIONE, FEBRUARY 4TH, 1841.

J. A. RANSOME, Esq., in the Chair.

On the Human Eye, and Vision. By R. T. HUNT, Esq., *Surgeon, &c.*

Mr. Hunt's lecture, was illustrated by some very beautiful coloured drawings and diagrams, exhibiting the external and internal structure of the human eye; the form of the pupil at different ages; the inverted images of outward objects; views of the cornea and pupil in different positions, &c. and also by an anatomical model of the eye, and a little instrument like the human eye, and constructed on the same optical principles, which exhibited the inverted images of external objects.

Mr. Hunt commenced by noticing the external form of the human eye, composed of two segments of spheres of different diameters ; and he then entered into a popular description of the several coats and humours of which the internal eye was formed, the coats being the containing parts, the humours the parts contained. The humours were the aqueous, the lens (or crystalline), and the vitreous. The dark membrane was a very beautiful structure, being required for the purpose of obscuring all the internal parts of the eye for optical purposes, just as in the camera obscura the interior of the box was blackened. Next within this was the nervous structure of the eye, the retina, upon which depended the communication of those impressions to the brain which constitute vision. The retina was connected with the optic nerves, connecting the eyeball with the brain, by means of which communication vision took place. The retina which lined the eye from the external part of the lens, entirely through the whole back part of the eye, was similarly external to the vitreous humour, and was perfectly transparent in the living eye, so that the black membrane was seen through it, giving the black colour to the centre of the pupil. Having thus noticed the chief circumstances connected with the general arrangement of the eyeball, he would next advert to the manner in which they appeared to be made conducive to vision. It was not his inteninto to enter into any general account of optics : it would be sufficient to state, that light entered the eye, and passed into its interior ; and when it arrived at the lens, it was caught up in such a manner, that, in an instrument like the eye, a perfect image was formed of every object placed opposite to the front of it, by those rays which could enter through the interior transparent part and the chambers of the eye, so as to fall upon the retina. Nor should he enter upon the theories of light, as to whether it was a material substance which entered into the eye in a material form, passing to the retina, or whether it consisted of undulations or vibrations which acted upon the membrane in front of the eye, and passed through the aqueous humour to the lens, and thence through the vitreous humour, so as to make an impression upon the nerve of vision, which conveyed this impression to the brain constituting sight. Probably the use of the vitreous humour was chiefly to keep the delicate retina spread out and free from all wrinkles, so that objects might be represented equally throughout. He repeated, the retina was not opaque like ground glass, as it was generally described, but perfectly transparent, and it was the only instance in any animal body of a nerve being perfectly transparent. If it were not, it would interfere with vision. [The lecturer then explained the optical instrument made to resemble the eye, and to produce an inverted image.] Any one looking into the eye of a friend looked quite through the retina, and saw the black membrane which lined the entire in-

terior of the eye. There was one common error to which he
would allude, and which was even continued in able works,—
viz. that a distinct picture of the object is formed on the retina.
This he must totally deny. He would admit that a picture was
formed in an instrument constituted like the eye, and even in
the human eye after death ; but, during life, the supposition of
the appearance of any picture on the retina was totally at vari-
ance with the truth. It would take too much time to enter
into the arguments in support of this view to any great extent ;
but he felt it incumbent upon him to state what he considered the
manner in which vision took place. All the senses were alike in
one respect : they consisted of a number of material organs me-
chanically arranged, for the purpose of transmitting particular
impressions to the brain, by means of the nervous system. But
these organs could only convey material impressions ; and the
only difference between touch, considered one of the simplest,
and sight, one of the most complicated, organizations of sense,
was, that in touch the object or substance which produces the
impression is applied to the nerve, and placed in contact with
it ; but in sight, as well as in sound, the objects which produce
the impressions are not applied to the nerve ; but a material
substance which we call light, which in some way or other
conveys different impressions, which produce the effect we
call vision, to the retina, and that conveys them to the brain
through the intervention of the optic nerve. If we can trace
distinctly that it is not necessary for a picture to be formed
upon the retina, of course that would account at once for what
would otherwise be a contradiction, that the apparent structure
of the retina allows the eye to act as a perfect optical instru-
ment, and that all rays of light impinge upon the retina, but
when they come there, do not stop ; but by means of a material
impression being conveyed to the optic nerve, that nerve con-
veys the impression to the brain. This may be distinctly shown
by the fact of material pressure, or even a blow, not causing
any pain or feeling of contact or touch, but causing a distinct
impression of light to the individual. He would notice another
beautiful arrangement :—When looking at an object, we did not
appear to be looking out of a window or through a frame, at
the whole scene, but we look at one part more, and at the sur-
rounding parts of the landscape less, distinctly ; and, if
we want to see the other parts, we must alter the position
of the eye, so as to throw the object sought, upon that
part of the retina best adapted to receive the impression. And
the structure of the retina was found to correspond with this
state of things. Another circumstance connected with the ap-
pearance of objects was that of the inversion of the image of
the objects seen. This took place in the instrument made to
resemble the eye, and in the eye itself ; and it had been, and
perhaps was yet, difficult to account for the inverted image.

The idea of the law of visible direction, and that of the habit of correcting this disposition, were equally erroneous; and no one, who had attended to the phenomena of vision, would think either sufficient to account for this appearance. There was no doubt whatever that we saw objects upright; and all the reasoning ever brought forward, either in support of the opinion that this depends upon the habits of the individual, or upon the laws of visible direction, would never convince any one to the contrary. But it had latterly been attempted to be shown, that the arrangement of the structures between the eye and the brain was of such a curious nature, that we might account for the rectification of position, by referring to those structures. The impression of visible objects, passes not only to the lower part but to the back part of the brain. After it has entered the skull, the nerve of one side unites with the nerve of the opposite side; and from this union there is a continuation of the nerves backward to the posterior side of the brain, with which they are connected: and immediately adjoining that part, the brain, which is in two halves, is united by a distinct bridge of nervous matter. The way this bears on the phenomena of vision is, that, if we trace the fibres as far as the junction between the two optic nerves, we find that there they change place; the fibres which came from the upper part of the retina proceed to the lower part of the sensorium, and *vice versâ*. Besides this, as the nerve passes backward, there is again a union just at its point of junction with the brain, or of the two sides of the brain to the part where this peculiar nerve arises. These circumstances have not only led some to suppose, that the inversion of the object is rectified by the optic nerve at this point, but also that the obliquity or crookedness of objects, because of the concavity of the retina, was also rectified. It was their opinion, that at the back part of the brain, near the junction, there was also some intricate arrangement which regulated or rectified these incorrect forms of impression; and we know that we see and distinguish the difference between a straight and a curved line, though all the impressions upon the retina must be curved. Another interesting circumstance connected with this junction of the nerves, till of late years, has not received much attention. We were in the habit of looking constantly at objects with both eyes, and yet we saw but one object. If we present but one eye, we can see two objects; but in perfect sight it appears as if we saw but one. This arrangement in the interior of the brain at the junction of the nerves, is probably connected with the rectification of curved lines; but the posterior junction of the brain is supposed by some writers, particularly by Dalrymple, to be the junction which combines, as it were, the two impressions into one. In looking at any object, there is as distinct an impression made on the right as on the

left eye, so as to make two impressions; they are conveyed along to the junction, and afterwards, when they come to that part of the brain upon which the consciousness of vision depends, they are combined and united into one. And what gave great probability to the suggestion was, that, in the other senses of hearing. taste, and smell, the nerves were connected in the brain by similar junctions. It was most probable, therefore, that this posterior junction was the one which had relation to seeing objects singly, and that the interior junction altered the position in which impressions of objects were formed upon the retina, not only with respect to their inversion, but also as to their curved appearance. How the optic nerve transmitted to the brain the impression produced on the retina, causing the particular sensation of sight or vision, we know not; we merely know that the eye is a living optical instrument, which receives impresssions of outward objects; and that there is a beautiful arrangement of nervous structure, which carries them to the brain; but here we must stop: how the impression is transmitted must probably always remain hidden, as it appears beyond human power to comprehend. This showed, however, how very insufficient any consideration was, that the mere arrangement of mechanism could give that degree of consciousness necessary to the performance of this beautiful function. He had also to notice a very beautiful mechanical arrangement in the iris (that part which gave to the eye its colour of black, gray, blue, or hazel); the pupil was not always the same size, it increased and diminished, or, in technical language, contracted or dilated; but in either operation it always maintained the same form, by means of the mechanical adaptation of a number of radiating fibres, running from the centre towards the margin of the iris; which by their action dilated the pupil, by drawing the edge to the outer part, and other fibres contracted the pupil by drawing the margin nearer the centre. Now, a bag, if drawn together by a number of strings, would be puckered; and nature seems to have prevented this puckering in the pupil, by means of numbers of short fibres connected with the radiating fibres, and thus prevent the puckering which would otherwise take place. These fibres were not distinctly seen in the human eye; those appearances which were visible being merely occasioned by the blood vessels and nerves of the eye. Light entering the eye through its interior chambers, the use of the dilatation and contraction of the pupil was to moderate and regulate the quantity of light admitted; so that, if the light was so very strong and brilliant that it would be injurious to enter the eye fully, the pupil contracts, to limit the quantity of light to be admitted. If the light be dull, the pupil dilates, in order to collect and admit of as much light as possible. Throughout the eye, then, there were everywhere visible the most beautiful order and arrangement. A strong outer case, including all the parts, having a window in front to admit the light, which entered so as to produce an exact impression of outward objects upon the nerves of the eye by means

of its different fluids, the aqueous, crystaline, and vitreous humours; so as to bring the light (whether a material body or a mere vibration) in that exact state upon the retina, which should produce those changes which were necessary, when the impression was conveyed to the brain, to constitute perfect vision. The eye, as he had said, was lined with a black membrane round it, which absorbed all the light that would otherwise interfere with perfect vision, and the perfectly transparent nerve of the eye lay within this black membrane. These might be regarded as the material or mechanical arrangements for vision; for the transmission of an object on the retina by means of the optic nerve to the brain was perfectly mechanical, and continued so till it reached the place where consciousness was produced, and there, at least, it was perhaps a wise provision of the Author of our being that we should know nothing further; as, after all our researches into this and other kindred subjects, we could only come to one conclusion, that " we are fearfully and wonderfully made."—(Applause)

Mr. P. H. Holland was sure but one opinion could exist as to the propriety of giving the thanks of the meeting to Mr. Hunt for his interesting communication ; and the directors and members of the institution were the more particularly indebted to the lecturer, when, as in this case, one not immediately connected with it, devoted his time and talent to their service.— Mr. G. S. Fereday Smith seconded the motion, which passed unanimously.—The chairman, in conveying the thanks of the meeting to Mr. Hunt, said, he should be glad to hear any remarks from any one present ; and he was sure Mr. Hunt would be glad to answer any questions, or to give further explanations, if any part of his lecture was not clear, which, however, was not very likely.—Mr. Hawkshaw wished to ask in what respect the living eye differed from the dead one, so that no image was formed on the retina of the living eye. He could not precisely see the use of the black membrane, if no image was formed in the living eye. Of course, he was aware there must be a point in all these inquiries, where mechanism must stop, and mind must begin to operate ; but, if the mechanical operation stopped before an image was formed, then he could see no necessity for the black lining or membrane.—Mr. Hunt said, that, if he wished to produce an image in the camera obscura, we should never choose a black screen ; for, if the image were thrown upon a perfectly black ground, the colours were confused or made indistinct ; and therefore, in the original camera obscura, a darkened room, with a wall at a right focal distance from a hole in a window shutter, a white screen was used. Then if it were true that an image was formed upon the retina ; if a white surface was opposed to the retina, it ought to produce a white surface on the retina ; and if the whole side of a room were white, this whiteness ought to be seen as the colour of the retina

by the eye of an observer looking in front.   This did, in fact,
take place in any instrument which resembled the eye, and in
the eye of the cow or the sheep, if examined a certain time after
death.   But this white appearance could not be produced in
the retina of the living human eye, and it was an experiment
that might easily be made.   He did not believe, that it was at
all material that an image should be formed on the retina, be-
cause no one would suppose that all images formed on the retina
were conveyed to the brain; and, therefore, it must be some
action that takes place in this nerve which produced the im-
pression in the brain to which we owed the sense of sight.   If
that was the case, then there was no necessity for an image on
the retina.   He feared he had been misunderstood, has the sub-
ject was a difficult one; but he believed, that whether light
entered the eye, passed through the transparent structure, and
touched the retina, or whether mere vibrations or undulations
from the light impinged on the front of the eye, this impression
was quite sufficient to account for vision, and this impression
was made exactly as an image would be made.   Rays from a
large object entering the eye would produce impression at dif-
ferent distances from smaller objects ; but he thought there was
some foundation for saying, that no image was actually produced
on the retina.

    Mr. Makinson said, that, as individuals advanced in years,
they were obliged to use spectacles or glasses.   Was this owing
to the conformation of the eye, or the crystalline lens, or the
aqueous humour, being altered ?

    Mr. Hunt said, some of his diagrams exhibited views of the
lens of the human eye at different ages—one the form at birth,
the second at about six years of age, and the third at a maturer
period of life.   A similar degree of development occurred in the
other structures and tissues of the eye.   Up to maturity the lens
retained its orbicular form but afterwards it became very much flat-
tened, and assumed a different form in the later period of life.
But perhaps what led very much to the alteration of sight was the
alteration in the front of the eye or corners.   This being more or
less convex produced a different vision.   If very convex, the in-
dividual was near-sighted : and if nearly in the natural state, it
was flatter rather than the reverse ; one cause whence it arose was
from the cornea not being so fully filled in old age.   If the eye is
too much used at an early period [at a short focal distance]—if an
individual is engaged at an early period of life, in learning of any
kind, which requires the constant application of the eye to small
print, he thought this would account for a tendency to near-
sightedness ; and he thought many gentlemen present would bear
him out in the notion, that the present rage for the over-cultivation
of the infant mind was one reason why there were now so many
more cases of near-sightedness than formerly ; as the eye was en-

gaged in examining minute objects before it was fully developed.

Mr. William Read asked whether the same or analogous processes of vision obtained with all animals in the same medium as man.

Mr. Hunt said, the eyes of birds differed considerably from the human eye, and those of insects still more so. The eye of the dragon fly contained, he might say, some hundreds of united eyes.

Mr. P. H. Holland said, that an explanation had been attempted why the inverted image on the retina should not convey an inverted image to the brain. Mr. Hunt's explanation was quite as ingenious as others of the same character, which had been started by a succession of physiologists, to explain a fact which it appeared to him (Mr. Holland) required no explanation. We saw objects in the right way because we had no notion of up and down, than what arose from the relative situation of objects to the surface of the earth. That this was the correct explanation might be told from the fact, that when the position was changed, and the individual was lying on his side, he was never in the least incommoded, though the image on the retina was from right to left. He at once referred the bottom of the object to the ground, and the top to the sky. It appeared to him not to require any anatomical arrangement to reverse the image. We were not conscious of any impression upon the retina. We knew nothing at all but that we saw; that we saw by our eyes was a matter of reasoning, and it appeared to him that physiologists had created the difficulty.

Mr. Hunt said, that Mr. Holland's exposition had prevailed for many hundred years; it had been thoroughly investigated by men every way competent to conduct such an enquiry, and it had now been thrown aside for a length of time. No one could convince him, by reference to habit or to any circumstance of education, that the inverted image or impression gave a direct sensation of the upright image, unless by means of some anatomical arrangement. Mr. Holland, if satisfied of the truth of his views, should also be fully satisfied that there was no such thing as perspective. He (Mr. Hunt) believed that the sense of sight required a great power of reasoning, which took place at a time when the individual was not in a state to give the result of his reasoning to the world—viz., before the time of speech. He believed there was a very rapid development of the eye in the first years of life. We saw young children try to lay hold of things beyond the reach of their hands; so in the same manner they were led to connect distance with the appearance of the object. The case of Cheselden's, which had been related so often, where an individual born perfectly blind was restored to sight by an operation, and where, the disease being congential, he had never learned to see, showed that such an individual believed every thing he saw to be like a picture,

and he had no more idea of a tree a mile off, than of one only three inches distant. He believed every thing to be a flat sur- face, as if painted on a wall. Those circumstances should be taken into account before doubt was thrown upon a new view of physiological facts.

Mr. Holland said, he was not bound to deny the existence of perspective; which, so far from being contrary to the views he had stated, was the very argument he would have used to support them. We saw distant objects appear smaller and more indistinct than when nearer; and the inference that the distant objects are as large as the near ones, is solely a matter of reasoning, conscious or unconscious as the matter might be. We see an object; we infer at a glance that it is at such a dis- tance; we refer it to the distance at which we know it would assume such appearances, and we thus rectify what would be an incorrect inference if we went by sensation only. Mr. Hunt had referred to a case in Cheselden; but might not every body see every thing flat, and infer it was not flat because they had been taught otherwise. That objects were not all plane sur- faces was matter of education. If objects were exactly drawn, and shaded, as we knew by the instance of the diorama, it re- quired a great effort of the mind to think we saw them flat.

Dr. Fleming differed wholly with the last speaker as to the image on the retina being transmitted thence to the brain. He did not see that this was at all a legitimate subject for the exer- cise of hypotheses. He believed truth was never more advanced in the world than by taking facts in opposition to reason. We had the fact, that the nerve receiving the impression of the upper part of the object was traced to the same position in the brain, or sensorium; which to him seemed a sufficient explana- tion, and one which did not leave room for hypotheses.

Mr. Hawkshaw said he should be glad to have explained why the image seen on the retina after death was not seen in the living eye.

The Chairman said this had been already explained by Mr. Hunt, who had stated that the retina, instead of remaining transparent after death, became opaque, or like the screen of ground glass in a camera obscura. He believed that Mr. Hunt was quite correct in stating it as a fact that no image was formed within the dark chamber of the eye. He (the chair- man) could mention an analogous case occurring in the new process of the daguerrotype. If the images in the camera were thrown on a white screen, they were found to be beauti- fully distinct; but when the prepared plate was introduced, being of a darker colour, viz., a golden hue, the distinctness of the image was very much diminished; so much so, that, when the objects were not strongly illuminated, he could not see

them at all; yet, giving it a proper time, the impression of the objects which we could not see was nevertheless so completely conveyed to the surface of the plate, as to enable us to form beautiful specimens.

Mr. Walker, surgeon, said he had made experiments with the eye of the sheep, by removing a small portion of the posterior part of the eye; and he found, that, even after the removal of the retina, the image was still visible. He thought that all the contrivances which were so apparently for the production of the image, would not have been found, if really there was no image there.

The Chairman said, the image could not be seen, of course, in the internal part of the surface of the membrane, as it could not be received upon a dark membrane; and as the retina itself was transparent, where were we to look for the image?

Mr. Hunt said he thought it would be found, that the parts of the eye which remained, and through which the light passed, were perfectly transparent; and it would be necessary to form a screen of some kind for the image to be fixed upon; and therefore, although the aqueous humour, the lens, and the vitreous humour, would act in the same way as usual with respect to converging and diverging the rays of light, yet we must place a piece of white paper or some other material to receive the impression upon it. He did not intend to controvert the fact, that the image was formed in an instrument like the eye, or in the dead eye; but it could be shown distinctly, that, if it did take place in the living eye, it would lead to confusion of vision. Mr. Walker had said, that he did not see the use of all this arrangement for producing particular impressions upon the retina, unless a distinct image were formed there. But, if such an impression was not provided, we never could see; and he had stated distinctly, that the material impression upon the retina was conveyed by the optic nerve to the brain by which the impression was transmitted to the mind. All these impressions were arranged exactly according to the laws of optics; a large object would make a large impression upon the retina, a minute object a small impression; and, therefore, the converging powers of the lens and the vitreous humour were necessary in order to arrange the impression in that way upon the retina which would convey to the brain all those circumstances which were necessary in order to ensure perfect vision.

Mr. Walker said, that the fact remained that there was an image, which he had seen after the retina had been removed; an image in the eye exactly corresponding with the objects in the external world: and, therefore, the position as to the existence of such an image was partly tenable.

Mr. Hawkshaw said, that Mr. Hunt considered the impres-

sion made was not a visible impression ; but of course an im-
pression must be made.

The Chairman said, the impression was not visible to another.
As to the experiment where the retina became opaque, we must
expect to find the vitreous humour also become opaque. There
must be a screen of some sort, or there could be no image ; it
was necessary in phantasmagoria to create a sort of screen of
smoke upon which to throw the image ; and in Mr. Walker's
experiment he should rather suspect some little opacity in the
membranes.

Mr. Sturgeon said, I am exceedingly glad, Mr. Chairman, to
hear you state, that facts will always have a great weight in
discussions on such subjects as that before us ; and, therefore,
I will venture to point out a fact of considerable importance,
concerned in the formation of images by lenses, which I have
not heard noticed this evening.   I believe it is admitted by all
parties who have entered into this interesting discussion, that,
within the eye, and close to the posterior side of the iris, there
is a lens, which receives those rays of light that have passed
through the pupil.   That being conceded, then, as that lens is
a *double convex*, it will exercise the same functions in the form-
ation of images as those exercised by convex lenses generally.
Its being a fluid lens is a matter of no consequence ; for it is well
known, that images are as decidedly formed by fluid lenses as
by lenses of solid glass.   Now the fact I have to mention is this :
the functions of the crystalline lens of the eye are entirely inde-
pendent of the presence of the retina ; and images would be
formed as correctly without the retina as with it.   Indeed,
the most correct images that convex lenses are capable of
forming, are in those cases in which no screen is employed.
The images formed in the tube of a telescope, and in the body
of a compound microscope, are far more perfect than any that
can be formed on a screen.   These images are formed in the
air, and would be as decidedly formed if no air were present.
Hence, a screen on the posterior side of the eye-ball is not essen-
tial to the formation of images by the crystalline lens.   Indeed,
the most perfect image that the eye is capable of producing
must necessarily be formed in the transparent vitreous humour
in front of the retina ; and when the image is formed at the
most suitable distance from the retina for the functions of that
membrane to be brought into proper operation, so as to collect
the necessary impressions for conveying the intelligence to the
sensorium, through the medium of the optic nerves, it is highly
probable that distinct vision will be formed.

The Chairman said, that Mr. Hunt did not deny that an
image was formed, but only that it was visible to others.   Mr.
Hunt's image was very like that which Mr. Sturgeon spoke of
as being formed in the air.   It was essential to the well acting

of the telescope   but no one except the person looking could see it.

Mr. Sturgeon said that the eye in that case would be the exact position of the optic nerve with respect to the image in the vitrious humour.

Mr. Hunt said, he had never heard any intelligent person deny the formation of the image as presented to the dead eye, but still he held that it was not by means of a coloured image, supposing there were one ; he did not believe that was necessary to vision, so far as the brain was concerned ; but the impression conveyed by light to the retina was thence conveyed by the optic nerve to the brain, and this caused vision. No one would say, that the image itself was actually conveyed to the brain. Mr. Sturgeon would not surely say, that the different colours were conveyed by the optic nerve into the brain ; but rather that the nerve had some particular action which conveyed the impression to the brain, and so rendered it conscious of the image upon the retina. If the optic nerve had this power, another nerve might have the power to receive the impression, and convey it forward. If the retina were an opaque white struc- ture, all those different objects would be represented upon it as in the camera obscura.

Dr. Black was exceedingly indebted to Mr. Hunt for his interesting lecture and statements. He thought some difference of opinion might arise from the different meanings attached to words and ideas. Though there might not be a distinct picture in colours imprinted on the retina, there must be that arrange- ment of the impression of light, which gave the sensation of colour. There was no doubt (whether light were material or a mere undulation), that there must be upon the concave surface of the retina, the impressions of these different *foci* of the un- dulations sufficient to give a picture, which is either seen di- rectly by the sensorium, or else was conveyed to it mediately. One fact might give the idea that it was a distinct picture im- printed on the retina, independently of the fact of its being seen upon it ; as he had frequently seen in dissecting the dead eye. When we look at an object very intensely for some time, and then withdraw our eyes, the picture is still visible for a short time ; and the impression or the image remains for a con- siderable time upon the retina of the sensorium, as we see in the case of a lighted brand thrown round very quickly in a circle ; we see no intervals, but it appears to be a perfect circle of light. And upon this idea a philosophical toy had been constructed representing figures in motion ; and to the eye they appeared to be in motion ; thus showing, that the image had been impressed upon the retina for a short period, and not im- mediately removed. So, looking at a strong light, as that of the sun, if we withdrew our eyes, we had for a short time

afterwards an image of the sun visible to us.   It was the opinion of many, and of him selfamongst the number, that a very
strong impression was left upon the retina, whether in colour
or not he would not say, but at any rate the impression of light
was very strong for a time.   There was considerable modification of the eye in different animals.   The eye of birds did not
differ from the human eye so much in the lens as in the form
of the sclerotic coat, or outward shell of the eye.   It was not
so compressible in front, and perhaps was so adapted by nature
to creatures flying through trees and bushes, and therefore requiring greater protection to the tender internal parts than we
did.   In the eyes of fish the lens was perfectly spherical, in
order to obtain a greater degree of refraction ; because the surrounding medium of water was nearly of the same density as
the water in the interior chamber of the eye, and consequently
there could be no refraction there.   The very great spherisity
compensates for the want of the cornea.   The eyes of amphibious fish, the *cetaceæ* for instance, and various kinds of whales,
were adapted to see both in and out of the water, in two different media.   There were various other adaptations of the
eyes of rapacious animals, to the purposes of vision ; but to
notice these would occupy too great a length of time.

The Chairman said the time was now expired, and he hoped
that another opportunity would occur for renewing the discussion.—(Applause.)

*On the characters of the Fossil Trees lately discovered near Manchester, on the line of the Manchester and Bolton Railway ;
and on the formation of Coal by gradual subsidence.* BY JOHN
EDDOWES BOWMAN, Esq., F.L.S., *communicated by the President.*

The paper commences with a few preliminary remarks on the
theory of repeated subsidences of the land during the carboniferous æra ; and on the drift theory, the author being of opinion
that the former receives much support from the phenomena
presented by the fossil trees found near Manchester, and that it
affords in return great assistance in explaining the peculiarities
of their position.   Mr. Bowman does not deny that plants may
have been carried into the water from neighbouring lands, as in
the instances of fern-fronds and other remains scattered through
the sandstones and shales ; but he conceives it is difficult to

understand whence the vast masses of vegetables necessary to form thick seams of coal could have been derived, if drifted ; and how they could have been sunk to the bottom, without being intermixed with the earthy sediment which was slowly deposited upon them.   He is of opinion also, that without a superincumbent layer of mud or sand, to retain the hydrogen during the process of bituminization, ordinary caking coal could not have been formed.   Another difficulty, connected with the drift theory, Mr. Bowman says, is the uniformity of the distribution of the vegetable matter, throughout such great areas as those occupied by the seams of coal, extending in the instance of the lower main seam of the great northern coal field, over at least 200 square miles ; and in that of a thin seam below the gannister, or rabbit coal, in a linear direction of thirty-five miles from Whaley Bridge to Blackburn.   On the centrary, he believes that it is much more rational to suppose, that the coal has been formed from plants, which grew on the areas now occupied by the seams,—that each successive race of vegetation was gradually submerged beneath the level of the water, and covered up by sediment, which accumulated till it formed another dry surface for the growth of another series of trees and plants,— and that these submergences and accumulations took place as many times as there are seams of coal.   He also explains the thinning out of the seams and other strata of the coal measures, by irregularities in the mode or extent of the depressions.

Mr. Bowman then proceeds to the examination of the phenomena presented by the fossil trees discovered on the line of the Manchester and Bolton Railway, and described by Mr. Hawkshaw in his paper read on the 5th of June, 1839, and in a preceding communication ; it will be necessary to notice therefore only those points which did not claim that gentleman's more particular attention.   Mr. Hawkshaw describes generally the markings on the internal casts of the trees ; but as it is difficult to convey a correct notion of their waved and anastomosing characters either verbally or by reduced drawings, Mr. Bowman applied paper to the surface of the stems and carefully traced the grooves or furrows by following them exactly with an instrument.   The only indications of scars, which he could find after a long and close search were at one point near the base of the largest tree, and though indistinct, his practised eye recognized them to be those of a Sigillaria.   He detected also in some parts, on the ribs of the same tree, the fine wavy lines so often visible on decorticated specimens of that family.   In describing the second tree, he alludes to a deep wedge-shaped rift on the south-east side, which had been coated with coal, and is strongly

marked with wavy lines, like those on the surface of the albur-
num of a gnarled oak.   On the fifth tree, he discovered a longi-
tudinal concavity on the north side, and he states that it resem-
bles the impression which would be left in a dicotyledonous
tree, by the pressure of a parasitic plant.   The characters of
the roots are also detailed at considerable length, particularly
their mode of bifurcation, and position with respect to the
horizon.

From a careful consideration of the phenomena presented by
the fossils, Mr. Bowman is convinced that they stand where they
originally flourished ; that they were not succulent, but dico-
tyledonous, hard-wooded forest trees ; and that their gigantic
roots were manifestly adapted for taking firm hold of the soil,
and in conjunction with the swollen base of the trunks to sup-
port a solid tree of large dimensions with a spreading top.

Towards the close of 1838, in forming the railway tunnel at
Claycross, five miles south of Chesterfield, a number of fossil
trees were found, standing at right angles, to the plane of the
strata.   The tunnel passes through the middle portion of the
Derbyshire coal measures, which there dip about 8° to a little
north of east.   The bases of the trees rested upon a seam of coal
fifteen inches thick. The exterior of the stems consisted of a thin
film of bright coal, furrowed and marked like the *Sigillari reni-
formis ;* and the interior consisted of a fine-grained sandstone.
Mr. Conway, who supplied Mr. Bowman with an account of the
discovery, infers, from the information which he obtained,
that there must have been at least forty trees found, and
judging by the area excavated, he is of opinion that they
could not have stood more than three or four feet apart.
There were no traces of roots, the stems disappearing at the
point of contact with the coal.   Several specimens of *Stigmaria
ficoides* were also noticed by Mr. Conway, lying horizontally,
and about three feet in length.

With reference to fossil trees in general, and especially to
those near Manchester, Mr. Bowman proceeds to show still
further : 1st, that they were solid, hard-wooded, timber trees,
in opposition to the common opinion that they were soft or
hollow ; 2nd, that they originally grew and died where they
were found, and consequently were not drifted from distant
lands ; and 3rd, that they became hollow by the decay of
their wood, from natural causes, similar to those still in ope-
ration in tropical climates, and were afterwards filled with
inorganic matter, precipitated from water.

1. In stating his reasons for believing that the coal measures'
casts were solid timber trees, Mr. Bowman alludes to the rifting
of the bark of modern forest trees, in consequence of the ex-
pansion caused by the annual addition of a layer of wood be-
tween the bark and the alburnum ; and to the thickening or

swelling of the base of the trunk and main roots, and the apparent lifting up of the latter out of the soil, in old trees, by the greater annual increase of the upper part or that nearest to light and heat. These phenomena in vegetation were illustrated by a diagram, which exhibited the form of the base of the stem and the root of a sapling, and of a full-grown tree. The author, in applying these characters to the fossils of the Manchester and Bolton Railway, alludes to the irregular, longitudinal and discontinuous or anastomosing furrows on their surface, to the swelling out at the base of their stems, and to the divergence as well as the angle of dip or downward direction of their roots. These characters, he says, are not observable in soft monocotyledonous trees, their stems never expanded laterally, and being as thick when only a few years old and a foot high, as when they attain the height of 60 or 100 feet. The roots also, instead of being massive and forking, generally present a dense assemblage of straight succulent fibres, like those of an onion or hyacinth. Analogy, therefore, as far as outward shape and habit are concerned, he adds, is strongly in favour of the fossils having been solid timber trees.

Mr. Bowman then combats the view, generally entertained, that fossil stems with perpendicular furrows, as in the Sigillaria, were succulent or hollow plants.* He states, that good specimens of decorticated Sigillaria exhibit fine straight, and curled or gnarled striæ, similar to those on the alburnum of many modern forest trees; and that this character, in conjunction with others, renders it almost certain that the fossils had a separate bark,—a feature which is considered in vegetable physiology to be a proof of a woody structure. He also alludes to the existence in many of the decorticated parts of these fossil trees of little prominences like those in barked timber; likewise to the scars left by the disarticulation of leaves: and he accounts for the general absence of the latter on large and old trunks by their having been obliterated, in consequence of irregular expansion from the deposition of new layers of wood; he notices moreover the absence in small Sigillaria of the irregular furrows observed on large specimens, and due in his opinion to the unequal expansion by the addition of new layers of wood. In support of these proofs of the original solid nature of the trees, Mr. Bowman exhibited polished slices mounted upon glass of portions of a similar fossil tree discovered in sinking a shaft 300 or 400 yards N.W. of those found on the line of the railway. The slices were made from a portion which exhibited

* Specimens of recent dicotyledonous wood from New Zealand, lent to the author by Mr. R. Brown, were exhibited on the table of the Meeting Room. They displayed both upon the bark and the naked wood, longitudinal ribs and intermediate furrows as regular as those on Sigillaria; and therefore prove that these characters are not incompatible with a dicotyledonous structure.

within the carbonized bark a patch browner, heavier, and more
compact than the rest.   In these slices, made under Mr. R.
Brown's direction, that gentleman discovered in the transverse
section, the uniformity of vascularity which is evidence of the
coniferous structure ; and in the longitudinal section parallel to
the medullary rays, the existence of these rays.   The slices
therefore exhibit proofs of discotyledonous structure, and con-
siderable probability of that structure being coniferous.   The
important evidence, however, of coniferous structure deducible
from discs in sections parallel to the rays, was not obtained, the
vessels having apparently undergone some alteration.

2. With respect to the second point, that the trees grew and
died on the spots were they are now found, and that they were
not drifted from distant lands, Mr. Bowman says, the argu-
ments in favour of the formation of beds of coal by a series of
subsidences of the surface on which the vegetables that produced
the coal grew, naturally lead to the inference that the trees as-
sociated with the coal also flourished on the same spots.   In op-
position to the opinion that trees would naturally float in an
upright position in consequence of the greater specific gravity
of the base and roots, he asserts, that the trees would maintain
that position only as long as they floated, and that they would
fall and lie prostrate when grounded on shoals or cast ashore.
He agrees with Mr. Hawkshaw in the opinion, that it is more
difficult to account for a number of great trunks being deposited
in the position of the fossils in the Manchester Railway, than
to imagine that they grew on the surface of the bed on
which they now stand.   Their position on a bed of coal is
another proof, Mr. Bowman conceives, that the trees were
not drifted, for if they had been transported by currents of
water they might equally have been imbedded in the alternating
shales or sandstones.   If beds of coal are the accumulated
remains of many generations of a luxuriant vegetation, the rich
compost thus formed, Mr. Bowman argues, would be well suited
for the growth of trees.   Again, the angle at which the roots of
the fossil trees, particularly of that distinguished by him as No.
2, dip towards the bed of coal, is considered by the author evi-
dence of the trees being in their original position, because, had
they been drifted, the roots would have been bent upwards, by
the downward pressure of the trunk, when the water had left
them.   The appearance of the roots being cut off, where in contact
with the coal, he is of opinion, may be explained by the fer-
mentative process having dissolved the vegetable texture below
the surface.   The stems and upper portions of the roots stand-
ing above the coal, he explains by reference to similar pheno-
mena in peat marshes, in which the bases of the trunks of an-
cient forest trees stand with the roots exposed, owing to the
shrinking of the surrounding peat.

3. In discussing the third point, that the trees became hollow

from the decay of their wood, and were filled with sedimentary matter after their immersion, Mr. Bowman refers to the facts recorded in the preceding paper by Mr. Hawkshaw (see vol. xvii. p. 54,) ; and in confirmation of them states, that Mr. Schomburgk during his four years' travels in Surinam repeatedly observ d similar phenomena, Mr. Bowman then proceeds to explain the processes by which he conceives the fossil trees were gradually submerged—their upper branches torn off, their interior removed by natural decay,—their bark converted into coal,—their central cavities filled with sediment ; and the whole buried beneath the stratum of shale or sandstone in which the trees were discovered. He afterwards applies the phenomena which he believes these processes produced to the condition and position of the trees, and the arrangement of the surrounding sedimentary matter. The author then enters into the inquiries,—1st, the time which the trees may have required to attain their dimensions ; and consequently the minimum of years requisite for the accumulation of the vegetable matter ; and, 2udly, what thickness of vegetable matter was necessary to form the stratum of coal nine inches thick, over which the trees stand. Mr. Schomburgk is of opinion that a dicotyledonous tree which would require in temperate climates one hundred years to attain a certain diameter, would arrive at the same dimensions within the tropics in sixty or eighty years. The largest of the fossil trees forming the immediate subject of the paper is equal in circumference to an oak of 130 years growth in this climate, or about 100 for a climate equal in temperature to that of the tropics. Allowing therefore that some time elapsed after the commencement of vegetation on the surface of the then dry land before the trees began to grow, Mr. Bowman infers, that 100 years must be the minimum of time which would be required for the production of the vegetable matter out of which the nine inches of coal were produced. With respect to the depth of the stratum of vegetable matter from which it was formed, Mr. Bowman takes for his data, the thickness of the bed of coal, nine inches ; the distance between the top of the seam and the bottom of the trunk under the arch formed by the roots, fifteen inches ; and for the distance to the surface of the ground, four inches, or in all twenty-eight inches ; whereby he infers that the thickness of the solid coal is equal to about one-third that of the vegetable matter out of which it was produced.

## ELEMENTARY LECTURES ON ELECTRICITY.
LECTURE VI.

Having shown the structure and operation of the electrophorus, it will next be necessary to examine the various parts of that apparatus, in order to ascertain their electric conditions :

but before I proceed in that undertaking, I will endeavour to make you acquainted with another kind of electroscope, which is capable of indicating a much feebler electric force than that we have hitherto employed. It is the invention of the Rev. Abraham Bennet, M.A., who in the year 1786, gave the following description of it :—

"This electrometer consists of two slips of gold leaf, suspended in a glass. The foot may be made of wood or metal : the cap of metal. The cap is made flat on the top, that plates, books, evaporating water, or other things to be electrified, may be conveniently placed upon it. The cap is about an inch wider in diameter than the glass ; and its rim about three-quarters of an inch broad, which hangs parallel to the glass to turn off the rain and keep it sufficiently insulated. Within this is another circular rim, about half as broad as the other, which is lined with silk velvet, and fits close on the outside of the glass : thus the cap fits well, and may easily be taken off to repair any accident happening to the leaf gold. Within this is a tin tube, hanging from the centre of the cap, somewhat longer than the depth of the inner rim. In the tube a small peg is placed, and may be occasionally taken out. To the peg which is made round at one end and flat at the other, two slips of gold leaf are fastened with paste, gum water, or varnish. These slips, suspended by the peg, and that in the tube fast to the centre of the cap, hang in the middle of the glass, about three inches long, and a quarter of an inch broad. In one side of the cap there is a small tube, to place wires in. It is evident, that without the glass the gold leaf would be so agitated by the least motion of the air, that it would be useless : and if the electricity should be communicated to the surface of the glass, it would interfere with the repulsion of the gold leaf : therefore two long pieces of tin foil are fastened with varnish to the two opposite sides of the internal surface of the glass, where the gold leaf may be expected to strike, and in connexion with the foot. The upper end of the glass is covered and lined with sealing wax as low as the outermost rim, to make its insulation more perfect."

Figure 5 is a representation of Bennet's electroscope, c. c. being the glass cylinder, F.F. the wooded foot, and B.B. the cap. The gold leaves are represented divergent, as when under an electric influence. A short brass wire is usually screwed into the hole in the cap of the instrument, having its upper extremity finely pointed. A brass ball also screws on the top of the wire to conceal the point, for some enquiries.

Fig. 5
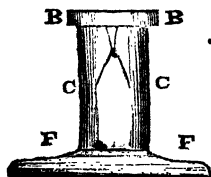
The way to use Bennet's gold leaf electroscope is similar to that we have pursued with the other: but it is better on all occasions when we want to communicate an electric action of some duration, to the instrument, to unscrew the ball from the vertical wire rising from the cap, and expose the point with which the wire terminates upwards. The point, in this case, receives the fluid from an electro-positive body presented to it, and gives off fluid to an electro-negative body. For instance, excite a tube of smooth glass by warm silk, and afterwards hold it over the point of the wire of the electroscope; the gold leaves will immediately diverge, and will remain divergent after the tube is taken away; and by testing the electric action left in the instrument, it is found to be positive. Now discharge this electric force by touching the point with a finger. It is very likely that the first touch of the finger is not sufficient to remove all the electric action from the electroscope, because it very often happens that a portion of the glass near to the cap becomes electrical; and when this is the case, the glass being a bad conductor, prevents any rapid movements of the electric fluid over its surface, so that whether positively or negatively electrical, *time* is required to equilibre its electric powers.

When the gold leaves no longer open after the finger is removed from the point of the wire, screw on the brass ball, and the point will again be concealed, and its functions in facilitating the ingress and egress of the electric fluid entirely annihilated. Now present an excited glass tube to the ball of the electroscope; and you will observe that the gold leaves diverge as before: but if you immediately remove the tube from the instrument, the gold leaves will collapse and will hang together in as neutral a condition as if they had never been under an electrical influence. But if you permit the excited glass tube to remain for some time near to the ball of the electroscope, then on withdrawing it, the gold leaves will first collapse and afterwards open with a negative electric action, which is of an opposite character to that of the tube which was presented to the cap of the electroscope. Hence you will perceive that the electric fluid can be driven out of the lower extremities of the gold leaves, by the repulsive action of the fluid superinduced on the surface of the glass tube, as decidedly as it was driven out of the asperities on the surface of the pith balls, in the former described electroscope. The fine edges of the gold leaves also admit of the ingress of the electric fluid when the instrument becomes electropolarized by the approximation of a negative stick of sealing wax to the ball, or to the cap of it.

The late Mr. Singer, who was a very clever electrician, made a very great improvement in Bennet's electroscope, which in its original condition was somewhat difficult to keep in order

Mr. **Singer's** improvement consists in passing the wire to which the gold leaves are appended, through a glass tube about four inches long and much wider than the diameter of the wire : which is held fast in the axis of the tube by two bosses or coils of sewing silk wound round the wire at the distance of about three inches from one another. These coils of silk not only hold the wire steadily in the tube, but assist in insulating it from the brass cap of the instrument. The insulation is still farther perfected by covering both the inner and outer surface of the tube with a good coat of lac varnish.* The glass tube is cemented into a brass ferrule, which has a thread cut on its outside for the purpose of screwing into the metallic cap. The wire of this instrument is generally surmounted with a brass disc whose plane is perpendicular to the axis of the wire. The electroscope thus fitted up is represented by fig. 6

Having now described a very sensitive electroscope, we will next proceed with our experiments on the electrophorus, in combination with it. Excite the electrophorus again, and place the cover on the resinous cake, taking care to have the glass handle of the former perfectly dry and warm, and the hand as far from the metal as you can, to prevent conducting connection. Now, without touching the cover with the finger as in our former experiments with this apparatus, lift up the cover from the cake,

Fig. 6.

and apply it to the point in the cap of the gold leaf electroscope, and you will find that it is slightly positive, owing to its receiving a small portion of the electric fluid from the atmosphere, by the instrumentality of the asperities on its upper surface. If the cover be permitted to remain on the cake for a few minutes, it will have acquired a considerable degree of positive electric action ; and by a still larger residence on the cake it will produce a spark.

When you are perfectly satisfied that you have ascertained the electric condition of the cover when untouched by the finger, or by any conducting body that could convey to it any electric action ; again place it on the resinous cake, and afterwards touch it with your finger. You now experience a smart prickling sensation on that part of the finger which touched the cover, in consequence of a spark passing from it to the cover when at a short distance from it. Now take up the cover by its glass handle, and apply it to the electroscope. You see the

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* **Lac** varnish is made by dissolving shell-lac, or seed lac, in spirit of wine, in a phial, which is better for being wide necked, to admit the brush freely.

gold leaves diverge with great force, and would have absolutely
struck the sides of the glass cylinder, had they not been too
short to reach it. Now test this electric action which the cover
has left in the electroscope, and you observe that it is of the
positive kind, in confirmation of what I told you before in our
last lecture ; and for those reasons which I then mentioned,
namely, the cover becomes electropolar by the action of the
negatively excited resinous cake, having its negative pole on the
upper surface ; which surface receives a spark from your finger
when presented to it, and thus becomes surcharged with fluid,
most of which it retains whilst under the influence of the resi-
nous cake, but is ready to part with it to the nearest conductor
when removed from that situation.

Another method of testing the electric action of the cover is
first to communicate some known electric force to the electro-
scope, and afterwards bringing the cover over the cap. If the
action communicated to the gold leaves be positive, they will
diverge still further when the cover of the electrophorus is
held over the cap of the electroscope ; but if the communicated
electric action be of the negative kind, the gold leaves will col-
lapse by the positive electric action of the cover when held over
the cap of the instrument. By this latter method the kind of
electric action of the resinous cake may be ascertained, which is
found to be negative.

Having now examined the electric action of the various parts
of the electrophorus, I will proceed to another beautiful appli-
cation of the principle of electro-polarization in an instrument
called the condensor ; also an invention of the celebrated Volta.
The condensor is usually appended to the double-leaf electro-
scope last described. It consists of two metallic discs of about
six inches diameter each ; one of which is insulated by being
supported by the glass cylinder of the electroscope, as at P, fig.
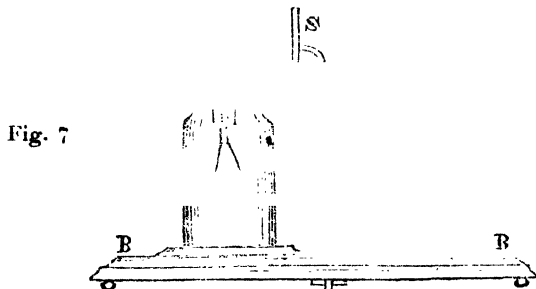7. The other plate S, of the condensor, is supported by a

Fig. 7

brass rod, which can be slided to and fro in a groove made for

the purpose in the base-board B B.   This piece of apparatus is
used only in those investigations in which the electric action is
very feeble, and in a manner which I will now point out.

Bring the moveable plate S, into close contact with the plate
P., and if the instrument be properly made, the two plates
will just cover each others inner surfaces, which ought to be
perfectly vertical.   Now separate the plates a little, by with-
drawing the moveable one S, until you have an exceedingly thin
plate of air between them.   It is now obvious from what you
have before observed; that any electric action communicated to
the cap of the electroscope would be partly transferred to the
plate P, even if the plate S were not present.   But now as the
plate S is close to the plate P, which, by being connected with
the ground, gives a greater facility for the polarization of the
plate L : so that when a feeble positive electric action is communi-
cated to the cap of the electroscope, an accumulation of the electric
fluid will take place in the plate P, without its affecting the gold
leaves ; which would remain nearly neutral.   But if we now with-
draw the plate S from the vicinity of the plate P, the polarization
ceases, and the fluid which rested, principally in the plate P, now
becomes almost equally distributed over every metallic part of the
electroscope ; and if the force is sufficiently powerful, the gold
leaves will diverge.   In many cases, however, and indeed in all
those in which the condensor can be much used to advantage, it
requires several communications between the body under examina-
tion and the cap of the electroscope, to convey a sufficient degree
of electric force to diverge the gold leaves even in the least degree
appreciable.

It may now be necessary to say something further with respect
to the terms *positively electrical,* and *negatively electrical,* bodies.
These terms ought, in all cases, to be understood as implying the
electrical condition of bodies *relatively* to the electric condition
of other bodies, and in no other manner whatever.   Let me en-
deavour to give you an illustration of what I mean by the term
relative.   I will suppose that three metallic spheres, A, B, and C,
of precisely the same diameter, uniformity of polish, &c. are insu-
lated and placed on different parts of the table.*   I will communi-
cate ten particles of the electric fluid to A, seven particles to B,
and five particles to C.   Now although the whole of these three
bodies are positively electrical with regard to the surrounding
group of bodies in the room, they are, in reality, in different elec-
tric conditions with respect to each other.   A, which has the
quantity ten, is positively electric with respect to the other two ;
and C, which has only the quantity five, is negatively electrical
with respect to both A and B.   And, for the very same reason, the
body B is either positively or negatively electrical, accordingly as
it is compared with C or A respectively.   This is obviously a pro-
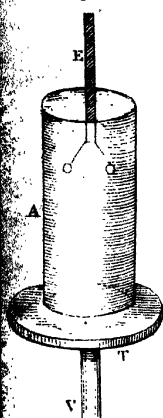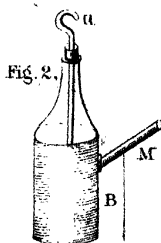minent case, and may very easily be comprehended.   But there
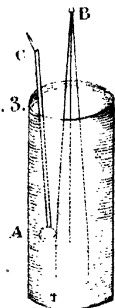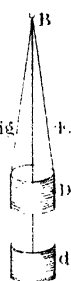
Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.
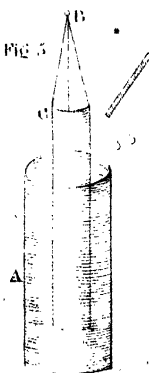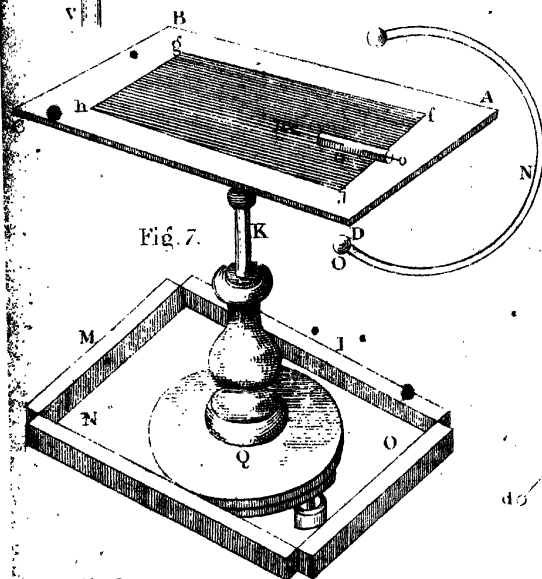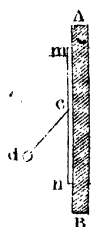
Fig. 5.

Fig. 6.

Fig. 7.

An Experimental Inquiry into the Influence of Nitrogen on the Growth of Plants. By Robert Rigg, Esq. *Communicated by the* Rev. J. B. Reade, M.A., F.R.S.

About two years ago I had the honour of laying before the Royal Society an experimental inquiry into some of the chemical changes which occur during the germination of seeds and the decomposition of vegetable matter. On the present occasion I purpose to confine myself to an extensive series of experiments which have reference to the presence of nitrogen, earths, and salts in vegetable compounds, with a view of directing attention to the influence of nitrogen in the growth of vegetables.

As my inquiry is purely experimental, I may premise that I have had recourse to the well-known method of ultimate analysis, and the equivalent numbers which I employ are, carbon 6·12, hydrogen 1·0, oxygen 8·0, and nitrogen 14·0. That we may the more readily apply the proportionate quantity of nitrogen to our immediate purpose, I shall make one column in each analysis, which will represent by weight the quantity of nitrogen when compared with 1000 parts of carbon in the same compound. I also designate by the term *residual* those earthy and saline ingredients which are not decomposed during the analysis. In some of the experiments this residual may contain

a little *foreign matter*, for in preparing the different compounds for analysis I seldom had recourse to any process of ablution, rather choosing to have a little foreign matter present, than to remove any part of that which was more particularly the object of research. That I might also examine the compounds as nearly as possible in their natural state, I very rarely exposed them to a higher temperature than 100° FAHR., inclosing them in very thin paper, and afterwards allowing them to acquire the hygrometric state of the atmosphere.

The first series of experiments to which I shall refer tends to show, that in that part of the seed where germination takes place nitrogen preponderates, when compared with its quantity in the other part of the seed. This result is derived from the analysis of the germ and cotyledons of beans, peas, barley, wheat, &c., a large excess of nitrogen being invariably indicated in the germ.

Thus, for instance, it appears from the table of analysis, that the germ of beans and peas contain by weight about 200 parts of nitrogen for 1000 parts of carbon, while the cotyledons contain only from about 100 to 140 parts.

A second series of experiments disposes me to think, that those *seeds of the same kind* which contain the largest quantity of nitrogen germinate the earliest. Barley of the growth of 1835, containing 46 parts of nitrogen for 1000 of carbon, germinated in thirty-six hours after being taken out of the water in which it had been steeped; whereas barley of 1837, and containing only 35 parts of nitrogen, steeped in water at the same time, and kept under the same circumstances as the former, germinated in forty-eight hours.

TABLE I.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water.	Total for 1000	Nitr. for 1000 Carb.
Germ of the garden bean	42·68	1·19	...	8·63	1·80	45·8	= 100	200
Cotyledons of the garden bean	39·27	...	2·66	5·65	2·40	50·02	= 100	140
Germ of early garden peas	41·9	0·2	...	8·3	0·8	48·8	= 100	198
Cotyledons of early garden peas	40·1	...	6·5	4·2	1·3	47·9	= 100	104
Germinating ends of barley	39·6	0·2	...	1·9	0·6	57·7	= 100	48
The other parts of barley	39·2	...	...	1·0	0·8	59·0	= 100	25
Germinating ends of wheat	41·2	0·9	...	2·1	0·7	55·1	= 100	51
The other parts of wheat	40·6	0·3	...	1·6	0·8	56·7	= 100	39

TABLE II.

	Carb.	Hydr.	Oxygen	Nitr.	Resid.	Water.	Total.	Nitr. for 1000 Carb.
Barley of 1835	43·93	0·71	......	2·02	1·30	52·04	= 100	46
Barley of 1837	39·57	......	3·45	1·38	1·30	54·30	= 100	35
Mustard seed	50·74	2·36	......	3·55	3·90	89·45	= 100	70
Cress seed	46·8	1·5	......	3·3	4·8	43·6	= 100	71
Rape seed	55·3	3·4	......	2·7	9·1	35·5	= 100	50
Turnip seed	55·4	3·5	......	8·6	8·1	34·4	= 100	65
Radish	55·34	8·48	......	5·03	3·4	32·75	= 100	·90
Celery	50·39	2·35	...	2·87	6·6	38·29	= 100	47

Similarly, I find that of the seeds, mustard, cress, rape, turnip, radish, and celery, those which contain the largest quantity of nitrogen and residual, germinate the earliest when kept under equal circumstances. It is necessary to state, that in these analyses the seeds were examined in the mass.

The chemical constitution of the *rootlets of seeds* before the *plumula* extends the whole length of the seeds, as in the instance of malted barley, differs from that of the malt, and also from the constitution of the barley in its original state. In these we have the rootlets containing a large quantity of nitrogen at a period when they will have to perform important offices in preparing the food for the young plant. That there is a similar difference between the chemical constitution of the roots and trunks of trees will abundantly appear from the annexed Table. And I may also add, that my experiments dispose me to infer that the quantity of nitrogen is largest in the spring, and diminishes with the season.

But not only is the nitrogen more abundant in the roots of plants and trees ; the residual also, when compared with the quantity in the trunks, will be found in excess in the roots.

Now if we admit the principle, that nitrogen is a powerful agent in favouring chemical action upon vegetable and animal matter, and that this residual is essential to the healthy performance of every function of the roots, as well as every other part of the plant, and forms, as it were, a most perfect skeleton of the whole ; we have in these roots that which will favour such action in an eminent degree when compared with the other part of the tree.

It would be leading us into other subjects more extensive than the one now before us, if I were to go into, or treat upon the chemical action which takes place by the agency of the roots, the compounds formed thereby, the heat produced by such action, the arrangement of the residual, &c. It will be

Table III.

	Carb.	Hydr.	Oxygen	Nitr.	Resid.	Water.	Total.	Nitr. for 1000 Carb.
Barley in its original state	39·6	.....	3·5	1·3	1·3	54·3 =	100	32
The malt made from the same	41·7	.....	1·8	2·1	1·4	53·0 =	100	50
The roolets of the malted barley	40·5	0·4	.....	4·8	3·5	51·8 =	100	106
Root of an apple-tree with the bark	41·5	.....	3·7	·7	1·2	52·8 =	100	16
Trunk of an apple-tree without the bark	42·8	.....	4·8	·8	·8	51·8 =	100	7
Root of a plum-tree with the bark	42·4	0·4	.....	·8	1·6	54·8 =	100	18
Root of the same without the bark	42·8	0·2	.....	·4	0·6	56·2 =	100	9
Root of cherry-tree with the bark	45·5	.....	4·3	1·1	·8	52·4 =	100	18
Trunk of cherry-tree without the bark	43·9	.....	.....	·6	·2	51·0 =	100	10
Ash in a very dry state without the bark	46·6	.....	.....	0·5	·8	52·1 =	100	11
Ash root without the bark	35·2	0·8	.....	2·6	9·4	52·0 =	100	74

Note.—The apple, plum, and cherry-trees were all of them very small ; they had been in the ground several years, and had been rooted up because of their general unhealthiness. In a healthy state of the trees the nitrogen of the root is in a larger proportion.

By Mr. Robert Rigg.   345

sufficient, that in following up this part of the inquiry, we state as the result of experiment, that the *sap wood* is very differently constituted from the more perfect part, *the heart wood,* an excess of nitrogen being invariably found in the former.

TABLE IV.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water.	Total for 1000 Carb.	Nitr. for 1000 Carb.
Young English oak sap wood	42·40	·18	......	·55	1·6	55·27	= 100	13
Heart wood of ditto	42·40		......	·16	0·7	56·74	= 100	4
Quebec oak sap wood	44·2	·14	......	·81	1·3	54·05	= 100	7
Heart wood of ditto	44·59		......	·18	0·6	54·68	= 100	3
English elm sap wood	41·20	·40	......	1·6	2·5	54·3	= 100	39
Heart wood of ditto	41·1		......	0·7	1·7	56·5	= 100	17
Acacia sap wood	48·79		2·49	·55	·50	52·6	= 100	13
Heart wood of ditto	41·36	·42	6·67	·50	·20	51·27	= 100	12
Cedar from Africa sap wood	42·06		......	·39	2·8	54·33	= 100	10
Heart wood of ditto	39·92		......	·36	0·7	59·02	= 100	9
Chestnut sap wood	41·16		1·86	·38	·40	56·20	= 100	9
Heart wood of ditto	40·18		7·96	·29	·20	51·87	= 100	7

It will be unnecessary for me to say that the sap wood more readily passes into a state of decay than the heart wood. Here again the nitrogen and the residual being present in larger quantities in the former than in the latter, we have them exerting their influence as promoters of decomposition.

We have also the greatest quantities of nitrogen and residual in those timbers which grow the quickest: and further than this; for directly as the quantity of nitrogen and residual taken collectively, so do we appear to have the decay of timber, all other circumstances being equal. The following is the analysis of several kinds of timber which favour this inference.

Thus, for instance, the nitrogen in the satin wood may be considered almost inappreciable; and the same may be said of the residual in the Malabar teak, the nitrogen being also small in this timber. In Dantzic and English oak the quantity of nitrogen and residual are both very small. In American birch

TABLE V.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water.	Total.	Nitr. for 1000 Carb.
Satin wood	47·20			·04	·50	52·26	100	1
Danzaic oak	44·90			·11	·50	54·49	100	2
English oak	43·20			·20	·60	56·00	100	4
Malabar teak	46·82	4·8		·26	·18	52·26	100	5
Rangoon teak	47·93	3·4		·48	·22	51·08	100	9
Spanish mahogany, fine-grained ...	49·0			·10	·50	50·4	100	2
Honduras mahogany, coarse-grained	40·07			·90	1·50	56·63	100	23
Lignum Vitæ	51·22	1·21		·66	·60	46·41	100	11
Box	46·4	·50		·50	·80	51·80	100	11
Rose wood	51·1			·10	2·50	45·8	100	12
Black ebony	42·4			1·50	·50	51·1	100	85
American birch	45·0			2·2	1·5	51·3	100	40

the nitrogen and residual are in large quantities, and, as is well known, this timber decays very quickly.

But it is not enough for us to find a difference in the proportionate quantity of nitrogen in the different parts of the same plant or tree; we must also observe that the quantity appears to be proportional to the functions which the parts of the plants have to perform in vegetation. For instance, if the agency of any part of the plant be great in the scale of vegetable physiology, so is the quantity of nitrogen, and *vice versâ.* So apparent is this, and so universal is the operation of this law over the whole sphere of inquiry in which I have been engaged, that we might almost consider this element, when coupled with the re-sidual, to be the moving agent, acting under the influence of the living principle* of the plant, and moulding into shape the other elements. We have this beautifully instanced in the chemical constitution of the different parts of wheat, barley, oats, common grass, turnips, cabbages, carrots, potatoes, &c., found by subjecting their various parts to analysis *of different periods of their growth* (See Table VI.). For by thus subjecting the

* What is " the living principle."? EDIT.

different parts of the same plant to analysis at different periods of growth, we acquire much valuable information upon vegetation generally, and respecting the influence of nitrogen and residual in particular.

TABLE VI.

	Carb.	Hydr.	Oxyn.	Nitr.	Resid.	Water	Total	Ntr. for 100 Carb
Flour of wheat not nearly ripe	41.2	1.8		2.9	2.0	52.1	100	70
Flour of the same kind nearly ripe	40.6	0.5		2.3	1.9	55.0	100	57
Leaves of the wheat not nearly ripe	37.8		8.1	3.1	4.2	46.8	100	87
Leaves of the same when nearly ripe	38.4	0.8		2.1	4.6	54.9	100	55
Stems of the wheat not nearly ripe	39.8	•		3.5	4.0	51.9	100	87
Stems of the same when nearly ripe	38.8			1.3	4.0	55.9	100	33
Chaff of wheat not nearly ripe	35.5	1.6		1.8	10.8	44.6	100	50
Chaff of the same when nearly ripe	31.2		7.3	1.3	11.0	54.8	100	42
Common grass not growing freely	41.1		1.7	4.4	5.5	45.9	100	107
Common grass gathered at the same time, growing very freely	39.5		3.1	5.6	6.5	46.8	100	141
Turnip when attacked by the fly	39.5	1.6		6.0	13.4	43.1	100	224
Cabbage leaf not eaten	39.5		4.8	8.1	5.9	41.7	100	303
The parts eaten by insects	39.7		13.8	5.7	8.0	38.8	100	43
The insects themselves	38.0	1.3		6.3	14.0	43.4	100	175
Green part of another cabbage leaf	39.9	0.8	3.6	6.5	4.7	45.1	100	162
White part of the same	38.2	2.7		8.0	4.9	47.1	100	205
Tendril of the same	38.8	1.7		6.4	9.3	46.8	100	198
Very centre part of the cabbage	33.0	1.4		4.1	4.3	57.2	100	194
Root of the same plant	39.2	0.4		5.6	9.8	49.4	100	141
Red clover stems	38.6		7.7	3.6	5.0	54.5	100	38
Leaf	38.0	1.4	10.2	2.9	5.0	50.8	100	149
Flower	30.4			9.1	3.4	45.3	100	119
Potato itself	37.1	0.5	18.1	8.5	16.0	38.4	100	79
Stem itself	28.3			3.9	9.4	41.3	100	123
Leaves of the same	39.8		16.4	3.3	4.5	41.8	100	914
Apple of the same	29.9	2.2	8.5	4.6	4.4	45.0	100	117
Corolla of the same	38.8	1.5		2.9	9.4	47.4	100	86
Pistils of the same	36.2	0.8		5.5	8.5	54.0	100	189
Young carrot, quarter of an inch in diameter	33.1		2.8	2.9	8.0	36.1	100	88
Leaves of the same	30.4			2.7	14.0	52.5	100	88
Stems of the same	39.7			1.7	11.2	53.6	100	59

There appear indeed to be various chemical actions taking place, in which these two elements are evidently concerned, viz. in the preparation of the food of the plants by the roots, and in combining this food with the other elements and fitting the whole to the various purposes of the plants.

Throughout the whole course of my experimental inquiry, I have not met with one instance wherein we have a large proportion of nitrogen and residual, that we have not violent chemical

action and quick growth of the plants, all other circumstances
being favourable.

By analysing *the leaves of trees* we may throw further light
upon the operation of nitrogen. Of the almost numberless
vegetables which cover the face of the earth, there are very few,
if any, whose growth and produce afford us more information
upon the chemical changes which occur during the growth of
plants and the decomposition of vegetable matter than the vine.
Its abundant flow of sap in the spring yields us a most important
product for determing its food. Its foliage furnishes us with a
plentiful supply of leaves for examination at different seasons:
and by allowing these leaves sometimes to remain on the trees
until they are very abundant, and then removing a considerable
portion thereof, leaving the rest to grow, we have at intervals
of very few days an opportunity of chemically examining this
very important and indispensable part of vegetable production
under very different circumstances. By carefully dissecting
these leaves, we are enabled to discover by analysis important
changes produced in very few hours. From the proneness on
the part of these leaves to pass into decomposition, at favourable
temperatures, we have a feature brought before us which claims
our best attention. And we have the fruit of this plant afford-
ing us, in its conversion into wine and other substances, an op-
portunity of examining into many important chemical changes,
and I may add, of making the accuracy of many popular
theories more than questionable.

The vines which more generally afforded me materials for
examination are those which produce the white and black sweet-
water grapes. They are in the open air, and are nailed to the
south side of a brick wall. A series of experiments upon the
leaves of these vines are given in Table VII., showing in a
striking manner that nitrogen is in large quantities when they
first make their appearance; that as they are developed, it de-
creases in proportionate quantity; that it is in excess during
the period of their most rapid growth; and that towards the
close of the year it is comparatively small.

TABLE VII.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water.	Total.	Nitr. for 1000 Carb.
The first small leaves on the white grape vine	37.7	0.5	......	5.1	9.5	47.2	100 =	134
Leaves from the same about a month afterwards	42.2	0.4	......	5.3	3.8	48.3	100 =	126
Leaves from the same in July	39.8	...	......	3.5	3.8	48.7	100 =	88
Leaves from the same in August	39.1	...	4.2	2.9	6.6	45.3	100 =	74
Leaves from the same in November	41.9	...	6.1	2.3	9.2	46.6	100 =	55
The first leaves on the black grape vine	41.8	1.4	......	7.8	10.3	38.7	100 =	185
Leaves from the same in June	42.8	...	3.8	5.4	3.8	44.2	100 =	126
Leaves from the same in July	41.5	...	1.1	3.6	3.0	50.8	100 =	88

With a view of ascertaining whether or not these peculiarities in the chemical constitution of the leaves of plants and trees were universal, I have had recourse to extensive analyses thereof, gathering the leaves from a great number of trees at different stages of their growth. The results hereby furnished may be obtained from the experiments in Table VIII.

The analysis of the different parts of the flowers of plants are full of interest. The parts not only differ in chemical constitution with their state of developement, as appears in Table IX., in the instance of the rose, where the full-blown petals contained twenty-four parts of nitrogen, and the unexpanded and central petals contained sixty-six parts; but the various portions differ very materially from each other, and when taken in connexion with the germination of seeds, the growth of plants, their aliment, &c., throw much light upon the whole subject.

Without adding to the number of experiments already furnished, I would observe, that I have not analysed any product in a natural state wherein I have not found both nitrogen and residual; and, of the great number that I have subjected to this process, those which are embodied in this paper may be considered as approximating to an average of the whole, as regards both this gaseous element and the incombustible matter.

TABLE VIII.

	Carbon.	Hydr.	Oxygen.	Nitr.	Resid.	Water.	Total.	Nitr. for 1000 Carb.
First small leaves from the lime tree in May	41·9	1·0	......	7·1	6·5	43·5 =	100	169
From the same in September 17	34·3	...?..	11·3	3·6	6·4	45·4 =	100	105
From the same in October 12	38·8	?	3·4	2·9	5·6	54·3 =	100	76
Acacia leaves, August 26, 1836	43·3	0·4	......	6·2	4·4	45·7 =	100	144
Acacia leaves, October 20, 1836	39·8	......	......	3·4	6·4	50·5 =	100	82
Almond leaves, August 26, 1826	37·5	......	11.7	4·4	8·8	42·6 =	100	118
Almond leaves, September 27	37·0	......	......	2·8	4·0	55·2 =	100	102
Plane tree leaves, September 26, 1836	41·4	......	18.7	2·7	4·4	32·8 =	100	65
Plane tree leaves, October 26	45·3	......	......	2·4	3·8	48·5 =	100	53
Red currant, May 3	40·1	......	.2·0	6·2	3·5	48·2 =	100	155
Red Currant, August 25	44·6	......	......	5·7	3·6	46·1 =	100	129
Very young ivy	40·4	1·6	......	3·6	4·6	49·8 =	100	90
Full grown ivy	41·6	0·4	......	3·2	5·2	49·6 =	100	78
Decaying ivy	42·4	......	......	2·2	5·8	49·6 =	100	52
Oak leaves, July 1836	40·8	0·6	......	4·3	3·9	50·4 =	100	104
Oak leaves, August	38·4	......	......	3·8	4·0	53·8 =	100	100

TABLE IX.

	Carbon.	Hydr.	Oxygen.	Nitr.	Resid.	Water.	Total.	Nitr. for 1000 Carb.
Full-blown rose pedals	42·2	......	......	1·0	2·0	51·2	= 100	24
Rose petals not expanded, gathered at the same time, and from the same tree .. }	45·2	1·1	......	3·0	2·6	48·2	= 100	66
Petals of the dahlia.......	25·5	......	10·2	2·7	4·0	47·6	= 100	77
Pistils of the same	34·8	1·3	13·5	4·2	4·3	55·4	= 100	120
Petals of the white lily	36·4	......	......	1·9	5·2	43·0	= 100	53
Pistil of the white lily	38·1	0·2	......	3·6	4·5	53·5	= 100	94
Pollen of the white lily	55·4	5·5	......	5·6	5·8	27·7	= 100	101
Stems of the anthers of white lily	40·5	......	......	2·2	5·0	52·3	= 100	55
Chrysanthemum, expanded petals	39·2	3·0	......	2·1	3·7	55·0	= 100	54
Chrysanthemum, unexpanded	39·2	1·6	......	2·9	2·4	52·5	= 100	74
Pollen of the same........	43·2	0·6	......	3·0	1·8	50·4	= 100	69
Leaves of the same........	40·2	......	......	2·8	8·2	48·2	= 100	70
Leaves gathered June 16........	41·4	......	4·1	6·0	4·2	45·3	= 100	121

In conclusion, I would observe that the mode of analysis which I have adopted in the examination of organic compounds, so far as determining the quantity of carbon, hydrogen, oxygen, and residual are concerned, is the one described in the paper or

vegetable decomposition to which I have already referred.
Respecting the mode of determining the quantity of nitrogen, a
very brief account of the plan which I have adopted is given in
the Philosophical Magazine for January last ; and by combining
these two methods of ultimate analysis, I am enabled, in reca-
pitulation, to detect very minute errors, and therefore to speak
with certainty as to the accuracy and value of every experiment.

ROYAL VICTORIA GALLERY OF PRACTICAL SCIENCE
MANCHESTER.

CONVERSAZIONE, JANUARY 30TH, 1841.

ALFRED BINYON, Esq., in the Chair.

On the Improvement of the Mersey and Irwell Navigation.

The Chairman, in opening the proceedings, said he hoped only
to occupy the chair for a very few moments, as they were expect-
ing the attendance of several civil engineers and other gentlemen
much better acquainted with the subject of the evening's discussion
than himself; and he should therefore be happy to vacate the chair
as soon as he saw them enter the room. He then read the notice
of the meeting.

Mr. Thomas Ogden Lingard, agent to the Mersey and Irwell
Navigation Company, who had consented to explain the object and
advantage of the projected improvements, then stated, that he had
undertaken to explain the subject merely from a wish to satisfy the
members. He wished it to be understood, that any observations
he might make were entirely unconnected with the company which
he was agent for. The opinions of the committee of the company,
and of every individual proprietor, he believed, were as various as
those held by the public at large. However, it was a subject in
which he had taken a very lively interest, since he knew any thing
of the navigation between Liverpool and Manchester, and one
which could be carried out with great advantage to the latter
town. I hope, gentlemen, you have not all forestalled me in
what I am about to undertake, that is, in reading the report of
Mr. Palmer.; for I am quite sure that those gentlemen who have
not read that report will listen to it with as much attention and
interest as I have done myself; and I hope that my reading this
report, instead of offering any opinion of my own, will meet
the approbation of the present assembly.

At this period of the proceedings, G. W. Buck, Esq., Chief En-
gineer of the Manchester and Birmingham Railway, W. Fairbairn,

Esq., and several other scientific gentlemen, entered the room. Mr. Binyon was about to vacate the chair, and proposed that it should be taken by Mr. Buck; but that gentleman declined, and Mr. Binyon was induced to retain his seat.

Mr. Lingard then read the principal portions of Mr. Palmer's report, illustrating it by a reference to a large plan of the river which was suspended against the wall behind him. In the course of reading, he made two or three remarks in reference to different parts of the report. He observed that the late Duke of Bridgewater, at the time he contemplated the construction of the tide-way, held in such little repute the advantages of the river navigation, that he refused to give to the Mersey and Irwell Company, the sum of £5,000. for the river, with all its appurtenances. He was in possession of several plans, which had been prepared by Mr. Radford, showing what the river was in its present state ; but they were not on a sufficiently large scale to be seen at a distance by the audience. By the proposed alterations, the course of the river would be shortened fifteen miles. In reference to the aqueduct at Barton, by which the Duke of Bridgewater's canal is carried over the river, he observed that this was the only considerable difficulty they would have to contend with between Liverpool and Manchester ; there were others, but they are of minor importance. He had endeavoured to devise means for passing the canal at Barton ; but he really did not expect that this great undertaking, which was so very desirable for the interests of Manchester, would ever be carried out without an amalgamation of the Bridgewater Trust and the Old Quay Company, or without the two navigations were given up to the public generally : he did not mean to say a gratuitous giving up, but they ought to be so disposed that the town might derive the greatest possible advantage ; an undertaking of this kind ought not to be trammelled by any conflicting interests.—Applause. The difficulty arising from the Barton aqueduct only applied to vessels with standing masts ; steam-vessels might come under it as it now existed, it was of sufficient height. He had had a statement put into his hand, showing the size of some iron steam-vessels lately built in Liverpool : one was to go to China ; its tonnage was 600 tons ; its power, 220 horses : length, 168 feet ; beams, 29 feet ; hold, 11 feet ; and the draft was only 6 feet. But Mr. Palmer had provided for depth of 12 feet all the way to Manchester. Another vessel, called the *Albert*, which was going to the river Niger, was 440 tons burden, 70 horses' power, 130 feet long, 27 feet beams, and 10 feet hold ; drawing 5 feet 9 inches of water. Another was 250 tons, 35 horses' power, 111 feet long, 22 feet beams, 8 feet 6 inches hold ; drawing only four feet of water. We had a sufficient depth of water at present to bring up vessels drawing from four feet six inches to five feet ; and here was a vessel of 250 tons and 35 horses' power, drawing only four feet ; but she was constructed of iron. In reference to the proposal of a drawbridge on the Grand

Junction Railway, he observed that this would not be a solitary case; for there was one over the river Ouse on the Hull and Selby Railway.

Mr. Fairbairn said there was one on the London and Greenwich Railway.

Mr. Buck.—And one on the London and Birmingham line, over the canal that enters Weedon Barracks.

Mr. Lingard said, the next apparent impediment was the county bridge at Warrington; but he thought that no impediment at all; because formerly, when it was travelled all hours of the day, there were two swivel bridges on it. Mr. Lingard then explained Mr. Palmer's proposal with reference to the locks, but said he thought the best plan would be to do away with the locks at Throstle Nest and Mode Wheel, and place the first lock at or near Barton. This would have the effect of increasing the depth of the water (and the banks would allow of it) ten feet below Mode Wheel, and five feet between Mode Wheel and Throstle Nest. Between Throstle Nest and Manchester it would have to be dredged to the depth of ten feet: but this would be easily done: he had by him a drawing of a dredger capable of removing 700 tons in 10 hours. He had received a letter from Mr. Rhodes, an engineer, who happened to be in the committee room of the House of Commons, when the Old Quay Company were opposing what would now have been a great obstruction if it had been obtained—the erection of a bridge over the river at Fidler's Ferry, by the Grand Junction Railway Company with a view to shorten their line. Mr. Rhodes, after alluding to some dredging which had been performed under his direction in the rivers Ouse and Derwent, said he was quite of opinion that the Mersey was capable of improvement, and that Warrington might be made a great port simply by dredging and deepening the river, and building walls, which might be effectually done, so as to allow vessels of 300 or 400 tons burden to reach that place, and vessels of 150 or 200 to come up to Manchester. It might not be altogether irrelevant to the subject, to state the saving that would accrue to the town, provided they could get rid of the town and port dues charged at Liverpool on all vessels which merely passed that port. In the committee on inland bonding last session, it had been admitted, and was never disproved, that the revenue derivable to the port of Liverpool from the merchandise brought up to Manchester was very much underrated when taken at one half. The dock and town dues in Liverpool, in 1756, amounted to £2781; in 1766, to £3653; in 1776, to £5064; in 1786, to £7508; in 1796, to £12,377; in 1806, to £44,560; in 1816, to £92,646; in 1826, to £131,000: and in 1836, to £221,000.—(Hear, hear,) So that if Manchester could get rid of that tax, even assuming only that one half this revenue was derived from what came up to Manchester, and what went back again, for the same duty was paid both ways, it would warrant the expenditure of two millions of money

to carry out this undertaking; and Mr. Palmer's estimate for accomplishing the whole of it was only about £370,000.—(Applause).

The Chairman said he was sure they were all deeply indebted to Mr. Lingard. The subject was one of vital interest to the town of Manchester; but it would be better for them, as a society for the promotion of practical science, to confine their observations to the practical difficulties that were in the way, and not to enter into any of the commercial relations of the question.

Mr. Lingard would venture to make another observation. A gentleman with whom he was happy to have the opportunity of forming an acquaintance, had been some time ago employed in making a survey for the Mersey and Irwell Company, and, during his engagement, the idea had struck him that something might be effected by making a Weir across the river at Runcorn Gap, instead of at Cuerdley Marsh. He (Mr. L.) had mentioned the plan to Mr. Palmer, after seeing his report; that gentleman said the same idea had struck him, but he had a very great objection to putting Weirs in tidal rivers. The gentleman he had alluded to was in the room, and would perhaps explain his plan to the audience.—(Applause.)

Mr. Bateman, Civil Engineer, said that, in the spring of last year, he had the honour of being employed by the Mersey and Irwell Company to investigate a question of some difficulty at Runcorn; and in that investigation he was led to consider what was the best means of improving the navigation, which he knew to be then under discussion. The corporation of Liverpool had had many previous reports on the best means of securing to them their outfall into the sea; and all those reports coincided in stating that no encroachment whatever ought to be allowed in the tidal bay above Liverpool; they attributed a great deal to the water impounding itself above Liverpool, and, on its return, scouring out the river all the way to the sea. He had also ascertained, from actual inquiry on the spot, that the early part of the ebb tide did very little indeed in scouring out the deposit, and carrying off the silt; this was not effected till the tide was about half-way down; owing to its going over the sand-banks, and carrying a good deal of sand along with it. It was not until the water was pretty nearly confined to the channel itself that any scouring was effectually produced. Taking this into account he thought if any means could be devised, by which any considerable portion of the first half, instead of going out useless to the Sea, could be impounded until the tide was half down, and then discharged *en masse*, the scouring effect would be considerably increased. There did not appear any great difficulty in making the navigation from Warrington to Manchester; the river was in many places ten feet deep, and not less than 100 yards wide; the principal difficulty was the entrance; for it was no use making a navigable river if the mouth were choked up. He had been led to think that it was not advisable to interfere with the tidal water at all, but to discover some other means of effecting the same

Junction Railway, he observed that this would not be a solitary case; for there was one over the river Ouse on the Hull and Selby Railway.

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forming an acquaintance, had been some time ago employed in
making a survey for the Mersey and Irwell Company, and, during
his engagement, the idea had struck him that something might be
effected by making a Weir across the river at Runcorn Gap, instead
of at Cuerdley Marsh. He (Mr. L.) had mentioned the plan to
Mr. Palmer, after seeing his report; that gentleman said the same
idea had struck him, but he had a very great objection to putting
Weirs in tidal rivers. The gentleman he had alluded to was in the
room, and would perhaps explain his plan to the audience.—(Ap‑
plause.)

Mr. Bateman, Civil Engineer, said that, in the spring of last year,
he had the honour of being employed by the Mersey and Irwell
Company to investigate a question of some difficulty at Runcorn;
and in that investigation he was led to consider what was the best
means of improving the navigation, which he knew to be then un‑
der discussion. The corporation of Liverpool had had many pre‑
vious reports on the best means of securing to them their outfall
into the sea; and all those reports coincided in stating that no en‑
croachment whatever ought to be allowed in the tidal bay above
Liverpool; they attributed a great deal to the water impounding
itself above Liverpool, and, on its return, scouring out the river all
the way to the sea. He had also ascertained, from actual inquiry
on the spot, that the early part of the ebb tide did very little indeed
in scouring out the deposit, and carrying off the silt; this was not
effected till the tide was about half-way down; owing to its going
over the sand-banks, and carrying a good deal of sand along with
it. It was not until the water was pretty nearly confined to the
channel itself that any scouring was effectually produced. Taking
this into account he thought if any means could be devised, by
which any considerable portion of the first half, instead of going
out useless to the Sea, could be impounded until the tide was half
down, and then discharged *en masse*, the scouring effect would be
considerably increased. There did not appear any great difficulty
in making the navigation from Warrington to Manchester; the
river was in many places ten feet deep, and not less than 100 yards
wide; the principal difficulty was the entrance; for it was no use
making a navigable river if the mouth were choked up. He had
been led to think that it was not advisable to interfere with the tidal
water at all, but to discover some other means of effecting the same

object. He had gone into a number of calculations, from data
which he had acquired, to ascertain what would be the actual effect
of what he proposed ; he found that the area of the upper part of
the river from Runcorn to Bank Quay, was about one-seventeenth
of the entire area from Rock Perch, and its capacity about one-
twenty-fifth, or one-thirtieth ; therefore, it formed a very small
portion indeed of the whole estuary ; and if the whole of this part
of the river were taken away, or stopped up, it would not produce,
perhaps, any perceptible effect on the mouth of the river.   By
throwing an embankment across at Runcorn Gap, the whole of the
water might be kept there till the tide had fallen eight feet, which
it did to half ebb ; then, by discharging the whole of it, as much
would flow out in two hours as now did in six, and the scouring
power would thereby be trebled.   That might be effected by flood-
gates 100 yards in length ; by gates of sixty yards, the water might
be discharged in three hours, and its scouring power doubled. The
whole navigation would then be rendered easy : it would be deep
water at all times from Runcorn up to Warrington, except when
the scouring was taking place, which might be done when the water
rose higher than twelve feet, and the discharges might be made on ·
an average three or four times a week.   Mr. Palmer proposed to
narrow the river, making the width at Liverpool the maximum,
and contracting it in a kind of funnel shape up to Runcorn Gap,
his reason being to throw the flood and ebb into the same course,
as, if he could constrain the tides always to run over the same
ground the whole of the silt would run out of the channel, and it
would not only be deepened, but be kept deep.   Taking it merely
from Liverpool upwards, no doubt he was perfectly right ; but he
(Mr. B.) was doubtful whether this would not have an injurious
effect upon the deeps at Liverpool.   The great tidal wave from the
Atlantic Ocean was first broken at the Land's End and Cape Clear,
in Ireland ; part of it ran round the west and north-west coast of
Ireland ; and part of it ran up the Bristol Channel and Saint
George's Channel. elevating the tides there to the enormous height
of thirty-feet at Swansea, and increasing, higher up the channel,
to 70 feet at Chepstow.   The direct course of the tidal wave was
at right angles to the entrance of the Mersey ; it would strike
against Formby shore, and its tendency, as it ascended the river,
would be towards the north bank. At present the antagonist forces
of the tides and the flowing out of the river, were so evenly ba-
lanced, that a tolerable channel was preserved at Liverpool; but
if the whole body of water in the estuary were reduced to one-
fourth or one-fifth its present volume, as it probably would be by
Mr. Palmer's proposition, it seemed to him that it would not be
able to offer a sufficient antagonistic force to the effect of the tide ;
and though the navigation of the upper part of the river might be
improved very much, its mouth at Liverpool might eventually be-
come choked up.   To run the risk of spoiling the harbour at Liver-
pool would be a dangerous experiment.   It appeared that very
large vessels might now be brought up without altering the navi-

gation at all below Runcorn ; and by commencing the improvements there it might be completed in the best way up to Manchester.—(Applause.)

Mr. W. Fairbairn said, it appeared to him that Mr. Palmer proposed to narrow the river upon the same principle as had been done in the Clyde ; and, instead of allowing the tidal waters of the Irish sea to flow through the narrow neck of the river at Liverpool, and fill all the bay between there and Runcorn, to narrow the channel all the way down, and bring the upland floods through that narrow channel, in order to scour out the debris. Mr. Palmer had concluded that the deposits were brought in by the sea, and were not from the high land range above the Mersey. If such were the case, the inference was, that the flood tide had the greatsst scouring power; and it would appear that Mr. Palmer was right with regard to narrowing the channel, as had been done in the Clyde; where, as by that means, particularly in floods, a great discharge was obtained. Provided these improvements were carried into effect, steam-vessels might navigate all the way up to Cuerdley Marsh, and even to Manchester. It was quite clear that the scouring force by which the estuary was to be kept open must be exactly in the ratio, either of the land floods, on one hand, or the tidal force, on the other. He should like to know the velocity with which the tide flowed into estuary, and that with which it retired.

Mr. Bateman said, that at Runcorn it flowed about tive miles an hour, and ebbed about the same. From that it went down to nearly nothing.

Mr. Fairbairn said it appeared to him that if the preponderance of force were with the in-coming tide, the deposits would be carried up the river; if the other way, all the minute particles would be driven out to sea, while the larger particles would be deposited in the bay. From what Mr. Lingard had stated, the tide flowed in at the rate of seven miles an hour, and the ebb at the rate of five and a half.

Mr. G. W. Buck.—At what point ?

Mr. Lingard.—At Liverpool ; on the rock.

Mr. G. W. Buck said he doubted whether that was correct, he believéd that the flood tide came in at five miles an hour, and went out at seven in round numbers, and therefore the velocity would be greater out than in.

Mr. Bateman.—I don't know how it may be at Liverpool; this has reference to Runcorn.

Mr. Buck.—Oh, it will be reversed at Runcorn, the momentum there is so much lessened.

Mr. Fairbairn.—If the scouring power of the ebb tide be so much greater, these deposits would be absolutely carried out, and could not come in at all, if there be a diminished velocity inwards.

Mr. Buck.—Yes, I think so.

Mr. Fairbairn.—But assuming that Mr. Palmer is correct in his

supposition, the conclusion he would naturally come to would be that the currents carry these deposits in from the sea ; and I think Mr. Palmer draws his conclusion from the nature of the deposits, which he has examined carefully, and he says they are from the sea.

Mr. Buck.—So they will be near to Runcorn.

Mr. Lingard.—It is not near Runcorn where he made the examination, but just in the bay.

Mr. Fairbairn.   Then a great proportion must be carried inwards, from the sand-banks accumulating in the estuary of the Mersey.   But I am partly of Mr. Palmer's opinion as to narrowing the channel ; and it might be done at a light expense. It is a fact well known, that fifty years ago, in the Clyde, they could not get a vessel up to the Bromielaw—that is Glasgow—of any considerable tonnage ; now the river is navigated by vessels of 600 tons burden ; and this has been done entirely by narrowing the channel.

After a short conversation, in which Mr. Fairbairn stated in answer to Mr. Bateman, that the bed of the Clyde was not silt, but rock, and that the tidal wave flowed directly up the Clyde, not at right angles to it, as in the Mersey, Mr. Fairbairn went on to say, that he thought a great improvement might be effected in the navigation above Runcorn by making new cuts, in order to avoid bends in the river.   By this means, and by having a quay and wet docks at Hulme, vessels of 300 or 400 tons might come up, and lie there, with perfect safety. He did not think there would be much difficulty with the Barton aqueduct ; it would only be necessary for the vessels engaged in the Manchester traffic to be so constructed that their topmasts might be struck.   With regard to the adoption of iron vessels, he had no hesitation in saying, that a new era was bursting upon us, both as regarded ocean and river navigation.   He had no doubt that in twenty-five or thirty years, nearly the whole of the trade of this country would be conducted in iron bottoms.—(Applause.)   And he concurred with Mr. Palmer in thinking that these vessels were, above all others, the most applicable to the navigation of the Mersey and Irwell.

Ultimately, the discussion was adjourned for a fortnight, and a vote of thanks was passed to Mr. Lingard.*

ADJOURNED CONVERSAZIONE, FEB. 11, 1841.

MR. CONSTERDINE in the Chair.

Mr. Lingard recapitulated what he thought necessary of that which he stated at the opening of the discussion.   He was very

* This Conversazione was held on the 28th, and not on the 30th of January, as printed at the head of the Articles.

glad that his task this evening was much relieved in consequence
of many parties being present who were prepared to go into some
discussion on the subject of Mr. Palmer's report.   But, in conse-
quence of its being said at the last meeting, in contradiction to what
he then asserted, as to the velocity of the flood and ebb tide, he did
feel it necessary to give some explanations ; and he was much
obliged to Mr. Buck for calling his attention to the circumstance,
because he found it stated, by Captain Denham, as Mr. Buck
had said, that the flood tide flowed with the greatest velocity.
He had hoped that Captain Denham would have been there
that evening.   The information he had already given was, that
the quantity of water that passed into the estuary, or rather
that passed Liverpool, during the spring tides, was about 779
millions of cubic yards, and that the quantity coming in at neap
tides was 292 millions, the mean quantity being 535 millions,
not giving the odd numbers.   Captain Denham also said, that
the maximum velocity of the flood tide was six miles and three
quarters in an hour, and that the maximum velocity of the ebb
tide was seven miles an hour.   Now this was so different to
what he had understood before, that, since the last meeting, he
had made it a matter of enquiry as to what was the opinion of
those who were better acquainted with the rivers than himself.
He asked three Captains who had been in the service of the
company between thirty and forty years : the first said the flood
tide run the quickest ; the second gave the same answer ; and
the third said, " The flood tide, to be sure."   He asked him
why " to be sure ;" to which the man replied, that every one
knew that the tide came in in five hours and a half, and went
out in six hours and a half ; and that that which did it in the
least time must do it the quickest.   He (Mr. Lingard) did not
rest there, but, while in Liverpool, asked for the oldest ferry-
man : he got one who had been accustomed to the ferries for
the last twenty years, and he gave precisely the same answer,
and the same reason, as the last witness.   He then saw the
Captain of the Woodside steamer, who gave the same opinion.
These opinions, as far as observation went, were pretty conclu-
sive.   Then he found, that Captain Denham supposed, that the
quantity of salt, mud, and sand, suspended in every cubic yard
of flood tide, was 29 cubic inches ; and that every cubic yard
of ebb tide contained 33 inches ; and, by that, would show
that the ebb tide took out four cubic inches more solid matter
than the flood tide brought in.   The effect of that would be, if
Mr. Denham was right, and he (Mr. L.) had gone into calcula-
tions on the subject, that the estuary of the river above Liver-
pool would now have an uniform depth of above 180 feet.   Now
Mr. Palmer stated in his report, from the examination of sand
taken out of the estuary; that the particles which were found
there were entirely of a marine nature ; and consequently, if
the ebb tide took out more than it brought in, it certainly

would not leave a deposit of what it brought in. He might be wrong, and Mr. Denham might be right ; but that was a matter which required the consideration of that meeting. He would state to them a fact which one of the captains had told him, showing that the water of the uplands had some effect in the scouring of the estuary. The captain in question was neaped on Dungeon Bank, about eight miles above Liverpool, for seven days. He was short of water, and took the advantage of low water to take some from the river, and the water was fresh or nearly so ; consequently, the fresh water at that time must have been having some effect on the bed of the river. That was what Mr. Palmer contended, although in opposition to the general opinion of engineers, that the estuary was necessary for clearing out the mouth of the river. As to the Barton aqueduct, a number of people had said to him, that Mr. Palmer had ruined his report by having a canal on the level of the Duke of Bridgewater's from Barton. Mr. Palmer had provided for steam vessels coming all the way up to Manchester, which appeared to him was likely to be the common mode of communication. But vessels with standing masts he could not bring under this viaduct, and he therefore went over it, or went in a line with it. And this was supposed to ruin his report !—a supposition which appeared strange to him in 1841, when science was so far advanced, that they could not construct a canal from Barton to Manchester, a distance of four miles, when in 1736 the Duke of Bridgewater constructed a canal from Worsley to Manchester, on the same level ; and that in 1761, he did not stop at Barton, but absolutely carried the canal on the same level from a place called Water Meetings to Runcorn a distance of thirty miles ; and, in 1826, a ship canal was projected, into which the whole of the water had to be pumped out of the sea, and this ship canal was shown to be a profitable undertaking. It had been suggested to him, that the best way would be to make the Duke of Bridgewater's Canal navigable instead of the river. Perhaps in some respects it would ; but the supply of water was wanted. If the river could be made navigable to Barton, which he had every reason to believe it could, then, instead of this canal which Mr. Palmer had projected, it might be well then to take the Duke's canal, leaving the river for steamers only to come up. That would do away with the great loss of water which otherwise would take place if they had to lock the whole of these vessels up to Manchester.

Mr. Radford, Honorary Secretary to the Institution, then read a letter from Mr. Fairbairn, dated London, February 8th, from which we give the following extracts :—

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London, February 8th, 1841.

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" DEAR SIR,—Finding my engagements in London such as will prevent my attendance at the next conversazione, and feel-

ing a deep interest in the forthcoming discussion on the Mersey
and Irwell improvements, permit me to offer a few observations
on that engrossing and very important subject. Since Mr.
Lingard's communication, I have had an opportunity of reading
Mr. Palmer's report ; and, although I do not entirely concur in
that gentleman's views, I nevertheless think that many parts of
his report are not only valuable, but exceedingly useful in a
practical, as well as a scientific point of view. It has always
appeared to me desirable, that something should be done to im-
prove the navigation between Liverpool and Manchester. Na-
ture has done her part in fixing the relative positions and levels
of the two towns, and she has placed at our disposal rivers of
considerable power, and well adapted for the purposes of navi-
gation.

Without entering upon the theory of currents and transport
of material, I would direct attention to the important fact, that
the bed of the river Irwell, at the New Bailey Bridge, Man-
chester, is only 49 feet (according to Mr. Palmer's report)
above the intersection of the tide at Woolston. Assuming it,
however, to be 50 feet, the difference of level is so inconsider-
able, that the whole might be surmounted by four locks of 12
feet 6 inches each, or at most five locks of 10 feet each.

From this it appears evident that the amount of lockage
would not be considerable, and there being, at all seasons, an
almost superabundant supply of water, is a sufficient guarantee
for the present traffic, even when doubled in extent. Besides,
every succeeding year must add to the supplies, by the forma-
tion of reservoirs in the uplands, and the consequent neutraliza-
tion of low ebbs and high floods in the currents of the rivers.

I feel somewhat desirous of directing public attention to this
circumstance, as the collecting of the flood waters in reservoirs
is not only valuable as a moving power in the first instance, but
these supplies are again transferred to the bed of the river for
the support and maintenance of the navigation during those
periods when the river in its natural state is short of water.
We have yet a great deal to do; and the present imperfect
state of the Mersey and Irwell navigation is, in my opinion,
neither creditable to the company, nor the age we live in.

* In following Mr. Palmer through his remarks and observa-
tions on the deposits in the estuary, I am not quite clear as to
his premises. It is well known to navigators, and all those ac-
quainted with the great tidal wave and currents of the At-
lantic, that, in its approach to these islands, it first breaks
upon the south-west coast of Ireland, and the Land's End,
and is thus divided into three portions, the first moving along
the western side of Ireland, washing the coasts of Galway,
Mayo, and Donegal ; the second rushing up St. George's

Channel ; and the third forcing its way into the German Ocean,
through the straits of Dover, visiting, as it passes, all the in-
termediate ports on the northern shores of France and the
opposite coast of England.   The second of these, however,
bears more immediately upon the present question ; it rolls, in
the first instance, with great force past Lundy Isle, and wedges
itself (if I may use the expression) into the throat of the Bri-
tish Channel.   It is the impetus of this tidal wave forcing itself
into the funnel mouth of the Severn, which produces the tidal
momentum, and raises the water (as stated by Mr. Palmer) to
the height of seventy feet at Chepstow.   In the Dee and the
Mersey we have not, however, to complain of these high eleva-
tions, as the forces are, in a great degree, modified by the main
body of the tide taking the direction of the Solway Firth, and
the Isle of Man. A little beyond this, at the Mull of Galloway,
its progress is stopped by the counter current, which by this
time has doubled Fairhead, and is now rolling down the north
channel between the coast of Antrim and the Mull of Cantyre
This amalgamation or meeting of the tides, accounts for the
commotion and short seas which invariably prevail during flood
tides of the Mull.

From the above description, it would appear that we have
not much to contend against in relation to the tides in the
Mersey, which seldom exceed a height of 30 feet at Liverpool.
Their influence is, on the contrary, of great value, as their
ascent would enable vessels of considerable burthen to ap-
proach, and enter the proposed sea lock, and from thence be
transferred by steamers to the quays and docks at Manchester.

I have before stated, that, in many of Mr. Palmer's observa-
tions, I have great pleasure in bearing testimony to their gene-
ral accuracy and research.   I do not, however, agree with him
as to the nature and direction of the deposits.   In the report,
he maintains that the shoals and sand banks above Liverpool
are brought in from the sea ; and judging from these remarks,
his conclusions appear to be drawn from two causes ; first, from
the excess of force in the flood tide, as it passes the narrows at
Liverpool, and secondly, from the nature of the silt, which, on
examination, he found to be the same as the shoals found in
the estuary.

Now, on a careful examination of these facts, it will be found,
that, although some portion of the outward deposits may, in
some winds, be returned, yet it is clear that all the formations
of banks and shoals now going on at the mouth of the Mersey,
and all other rivers, are derived from the removal of the soil,
and the disintegration of the strata of the uplands.   There is
no doubt that great changes and considerable fluctuations take
place in silting up certain localities ; but it is the business of
the engineers to investigate these laws, and to apply such reme-

dies as the regular and combined action of the different currents will admit.*

Without, however, stopping to investigate these things, I would urgently recommend the public to encourage and promote a more searching inquiry into this subject. It is one of deep interest to the community, and any improvement which would enable vessels of 400 to 500 tons burthen to discharge their cargoes in a *commodious wet dock at Hulme*, would form an epoch of such magnitude in the history of Manchester, as would quadruple her population, and render her the first as well as the most enterprising city in Europe.

Since the above was written, my friend Mr. Bateman has transmitted to me a copy of a report, addressed to the company of proprietors of the Mersey and Irwell navigation. I have perused that document with great care, and would earnestly direct public attention to the clear and sound engineering views of the writer. Mr. Bateman appears to have bestowed considerable time and great attention upon the subject. I perceive his intention is to throw an embankment half way across the narrows at Runcorn Gap, and the remaining* half to be formed of piers and arches, with a revolving gate in each to admit the flood tide, and again to discharge it as a securing power, to open and maintain the channel in that part of the estuary most liable to the deposit of silt from the point of discharge down to the deeps above Liverpool.

Mr. Bateman's plan appears to combine many advantages over that of Mr. Palmer's, as a tidal embankment at Runcorn Gap, with its accompanying self-acting flood-gates, would not only place a large scouring force at the disposal of the proprietors, but would afford to the public the important desideratum of a safe and commodious communication between the town of Runcorn and the opposite shore ; besides I am of opinion that Mr. Bateman's plan would dispense with Mr. Palmer's idea of narrowing the channel in the upper estuary, which, on a careful examination of the map, would probably be attended with an outlay of capital greater than the object to be obtained would warrant.

In these observations I have considered it my duty to direct attention to Mr. Bateman's very able report ; and, provided he would favor the members of the gallery, at their next meeting, with his views on the subject, I am sure he would confer a benefit, not only upon the institution, but upon the public at large.—I am, dear Sir, your faithful obedient servant,

W. FAIRBAIRN.

Richard Radford, Esq., Hon. Sec. Royal
   Victoria Gallery, Manchester.

* On this subject I would beg to refer to the report and surveys of Captain Denham, the best authority now extant on the estuaries on the rivers Dee and Mersey.

Mr. Gibb said, he should be glad if any gentleman in the
society would give his experience whether Mr. Fairbairn was
right; whether the mouths of the rivers where harbours were
maintained, were, in fact, flooded up by the lodgment of sand
from the interior of the country. He was acquainted with one
or two harbours of some magnitude in Scotland, where the
river was small in comparison, he meant as regarded coming
down from the interior of the country, and where the tidal river
was very similar to the Mersey; and in these small rivers he
knew there were large banks always rising. The harbours of
Ayr and Irvine he knew were a good deal infested with banks
of this kind, which could not be from the quantity of sand
coming down from the interior, but were made by the deposits
from the tide. He believed, that the more power there was
given to the water coming from the interior of the country, the
more would the mouth of the river be kept open. What was
the fact with regard to the Clyde? It had been going on for
nearly twenty years, and was a most excellent example for
Manchester; and they had never heard the people of Greenock
or Port-Glasgow say, "You are going to spoil our river by
sending down sand." It had been stated, that the proprietors
of the river Mersey had not done their duty. That was correct.
They could not go upon the continent without finding vessels
of superior power and small dip going up and down the rivers;
this was to be seen in France and Germany, and he did not see
why they should not have the same here. Now in Manchester
they had four feet and a half of water in the midst of summer;
but, notwithstanding that, they could not get any thing like a
respectable steam boat up to Manchester.

Dr. Marshall said he knew the Clyde well; it was now a
depth of 10, 12, or 15 feet, and he conceived that this was
owing to the current of the upland waters.

Mr. Buck, Chief Engineer to the Manchester and Birmingham
Railway, spoke as follows:—

I cannot refrain from taking a part in this discussion, although
it is, I believe, the first instance in my experience, of the report
of a brother engineer having been brought before the public for
discussion. (Hear hear.) With Mr. Palmer's views generally,
so far as they have reference to the improvement of the navi-
gation of the river above Runcorn, I cordially concur. I think
that with the proper application of funds, and judicious engi-
neering, there can be no question that vessels of considerable
burden can be brought up to Manchester, and that Manchester
may enjoy the blessings of a port.

Some years ago the late Mr. Talford was consulted, I believe
by the directors of the Mersey and Irwell navigation, for the
purpose of obtaining his opinion upon it; when he gave an un-

favourable one. But steam navigation, by means of iron vessels, was not then, I believe thought of. If there is sufficient water in the river, no doubt such vessels can come up, but without the aid of steam navigation, I think the proposed improvement ought not to be recommended. Since I came into the room, a piece of a newspaper, (I think the London Times), has been put into my hands; it contains an advertisement to the following effect:—" To be sold, an iron steam boat of great " strength and speed, of 163 tons burden ; 157 feet in length, " 15 in width, and 8 feet 6 inches in depth ; fitted up with spa- " cious saloon, Ladies' cabin, and every other convenience ; " with two marine condensing engines 25 horse power each ; " draught of water three-feet six-inches ; and her speed has " been proved to be equal to 13 miles an hour." (Hear hear). With that evidence staring us in the face, I am quite sure that, with such a river as the Mersey, there can be no difficulty in bringing vessels up to Manchester of twice that size. (Applause).

Having said thus much in reference to the navigation between Manchester and Runcorn, I will make a few observations on that part of Mr. Palmer's report, which applies to the estuary between Runcorn and Liverpool. In that report he certainly ventures to place himself in opposition to all the celebrated engineers in the kingdom ; inasmuch as he says that the estuary is injurious to the port of Liverpool, and that if it be gradually contracted from its present width at Liverpool of 3300 feet, to its width at Runcorn of 1200 feet, making it "trumpet mouthed," as he says, the scouring power will be increased. Here he is also in opposition to Captain Denham.

I came prepared with a few notes on this subject ; and though this is a conversazione, I take it to be more a subject for a paper ; and if you will allow me, I will refer to them (Hear hear).

Mr. Palmer considers the great area of the estuary to be injurious, because its extent is such as to cause the silt and the sands that form the bed of the estuary to be acted upon by the winds and waves, and subject them to change of place ; " hence the channel or line of deepest water varies." He says, shoals are said to accumulate in the upper part of the estuary, and he attributes them solely to sands brought in by the tide. That is a summary of what he says in reference to them.

He says the sands shift in the estuary ; they do so ; and if it were possible to fix those sands in their situation, I am of opinion it would ruin the port of Liverpool. It is because they are acted upon by the current, and are continually changing their places by the action of the winds and waves that they are prevented from becoming consolidated. If they were to become

consolidated they would gradually rise above the water, and therefore the channels would become permanently defined and the estuary would no longer exist: It is possible that there would have been no estuary for ages back if the sands had not been moveable. It has been stated by Captain Denham, that the influx of water is 779 millions of cubic yards in a high spring tide, and that this comes in in five hours and twenty minutes. Whatever those respectable boatmen who navigate the river may say in reference to the comparative velocity of the ebb and flood tide from their observation, I shall certainly place more dependence upon the evidence of Captain Denham, who was, for six years marine surveyor to the port of Liverpool; who is a scientific man, and who communicated the result of his labours to the British Association at two of their meetings. He has measured the velocity of the stream in both directions; but I question if those boatmen ever measured it.

The tide flows five hours and twenty minutes, and ebbs six hours and thirty minutes. The velocity of the flood tide for the first hour is four miles an hour, for the second 6, for the third 7; for the fourth 6; for the fifth 3; and then it diminishes from one mile an hour to nothing. In going out its velocity for the first hour is 4½ miles an hour; for the second, 7¼; for the third 7; for the fourth 5¾; for the fifth 4½: and then it rapidly diminishes. Captain Denham stating the velocity for the sixth hour to be 2½ miles an hour.

The column of water which moves past Liverpool in coming in with a high spring tide is therefore about 26½ miles in length.

Mr. Hawkshaw.—May I just beg to interrupt Mr. Buck to say one word. I think he has probably misstated the facts, in giving the times of the ebbing and flowing of the tides; I think he has made the ebbing of the tide the longer.

Mr. Buck—So it is.

Mr. Hawkshaw—But I understand Mr. Denham settled it to be the other way.

Mr. Buck—No, that is the mistake which occurred here the night before. From the preceding data the length of the column of water which comes in is 26½ miles, and that which goes out is 32½ miles. The mean velocity I find is very nearly seven and one-third feet per second, both coming in and going out; but there is a very extraordinary difference in the velocity at different times. Captain Denham has the honour of having discovered a certain law which affects the tides generally. It is this, that you may discover from the tide itself, the line corresponding with the mean level of the ocean: a certain space of time is always consumed in the ascent of the tide from this line to high water, and its descent from high water to this level al-

ways occupies precisely the same time as its ascent, whatever
the rise of the tide may be. At Liverpool this time is three
hours ; the tide rises above the mean level of the ocean for three
hours ; whether it be a high or a low tide ; a spring or a neap
tide, it is always three hours in rising above this level, and three
hours in descending to it. It is this great mass of water rising
above the mean level of the ocean and occupying the area of
the estuary, which effects the scouring and maintains a channel
for the commerce of Liverpool through that bar, about fourteen
miles distant. If this volume of water were lessened, that bar
would increase, and most unquestionably large vessels could
no longer come to Liverpool ; and if Liverpool were destroyed,
of course Manchester would share its fate, so far as the naviga-
tion is concerned. Therefore it is of the highest importance
that this point should be well sifted, and that there should be
no doubt about it.

I find then that the column of water flowing in during the
last three hours of the tide, which raises the water above the
half tidal range, as Captain Denham calls it, is fourteen miles
long, and that the column which flows out during the first three
hours of the ebb is $18\frac{3}{4}$ miles long. The one comes in and the
other goes out in three hours, consequently, the velocity must be
greater out than in, in the proportion of $13\frac{1}{4}$ to 100. Now I
want to arrive at a comparison of their momenta ; and in this
case the length of the column may be regarded as the expression
for its volume or weight. Mechanically, or mathematically
speaking, if we wish to determine the effect of the disturbing
force of a body in motion it is obtained by multiplying its
weight into its velocity : but the length of the effluent column,
$18\frac{3}{4}$ miles, may represent its weight ; and the mean velocity
of the efflux, during the first three hours is $6\frac{1}{4}$ miles per hour ;
therefore the product of these is 117, which will represent the
momentum of the efflux during the first three hours.

The mean velocity of the column during the last three hours
of the flood is 4 & 2-3rds miles per hour, and its length being 14
miles, the product of these is $65\frac{1}{3}$ miles, which will represent the
momentum of the flood during the last three hours. There-
fore the momentum of the flood is to that of the ebb, during
the superior portion of the tide, when the mass of water is
greatest, in the ratio of 65 to 117, or as 100 to 180. Here
you see are the mighty means by which the port is kept open,
it is the power of the top half of the tide ; if you take away
this enormous momentum of the retiring tide, you destroy the
port. The Victoria Channel, which is eight miles distant from
the Black Rock, about two miles long and half a mile wide, has
been scoured out to a depth of about twenty feet at low water ;
and this effect has been chiefly produced by the force which I
have described. The other channels are probably silting up ;

and it will be better for Liverpool if they should silt up, because the worse they become, the better the Victoria Channel will become ; and it is far preferable to have one good channel, than two or three bad ones.   •

The momenta of the remaining or inferior portions of the tide will stand thus:—The length of the column of the first portion of the flood, that is from low water to half tide range, is 12 and 7-12th miles, which enters with a mean velocity of 5 2-5th miles per hour, and their product representing the momentum is 68 nearly.   The length of the column of the last half of the ebb, namely, from half tide range down to low water mark, is $13\frac{3}{4}$ miles, flowing with a mean velocity of $3\frac{93}{109}$ mile per hour, and their product representing the momentum is 54.   Consequently although the quantity of water discharged is the greater, the momentum of the ebb is less than that of the flood during this inferior portion of the tide, in the ratio of 68 to 54, or as 100 to 79 nearly.   •

Here the momentum of the flood is the greater, but it takes place when the estuary is comparatively empty.   This part of the ebb does not reach the Victoria Channel before it is met by the returning flood.   And because it occupies a much smaller sectional area, its mass, and consequently its *absolute* momentum, is very much less than that of the first portion of the ebb.

If we compare the sums of the several ratios here obtained, in order to take the mean of the entire tide, the momentum of the flood is to that of the ebb as 100 to $129\frac{1}{2}$: but this ratio holds good only on the supposition that the sectional area of the stream is equal at all times, which is not the fact ; and if we suppose that the sectional area of the first half of the ebb tide is twice that of the last half, (this supposition being probably very near the truth,) then the comparative momenta of the flood and ebb during one entire tide will be as 100 to 146.   With this great preponderance in favor of the ebb, it is therefore impossible that the tide can cause the filling up of the estuary.

Mr. Palmer proposes, as I stated before, to narrow the channel from its width at Liverpool to its width at Runcorn.   Now the volume of water, as has been previously stated, which flows out of the estuary as it is, is, in round numbers, 779 millions of cubic yards ; but the quantity which would flow out upon Mr. Palmer's plan would be about 163 millions of cubic yards, being in the proportion of 478 to 100,—call it five to cre.   But what would be the difference in the effect ? The effect would be diminished in a much greater ratio than that of five to one ; because if there were only one fifth of the quantity of water, its velocity would also be reduced to one fifth ; for the small quantity of water would take as much time to flow out as the large quantity, because it cannot go off faster

than the tide falls; it must take the whole time to run out.
Therefore the effect would be diminished in the ratio of the
squares of the velocities; and taking into account the fractions,
it would have a disturbing force, by which the shoals are re-
moved and the bar kept open, equal to only one twenty-third
part of that which now exists. Perhaps all who are present
may not be in the habit of looking at these things mechanically;
but I think I can, in a few minutes, explain why the force will
diminish as the square of the velocity. If, as will be readily
admitted by all, the momentum of a body is represented by its
weight multiplied by its velocity, I will suppose the velocity to
be diminished one half, the weight remaining the same; then
the momentum is clearly but one half of what it was before.
Now suppose the quantity or weight of the body diminished
one half also, and then the momentum will be again diminished
one half, and will be only one fourth of what it was at first.
Therefore, you see, the momentum is diminished to one fourth,
by diminishing the weight and velocity of the body each one
half. So, if we diminish both the bulk of the water and its
velocity to one fifth of what they were before, it is clear that
the effect will be only one twenty-fifth part of the former
effect.

Having said thus much, I will go on to show that the estuary
may be contracted, and that nevertheless the port may be bene-
fitted. Here I take a view of the subject different from that of
my professional brethren of well-known name who have said,
"If you encroach on the estuary at all, you injure the port of
Liverpool,—you injure the scouring effect." Because, they say,
if you encroach upon the estuary, you prevent so much tidal
water from coming in as would fill that space which you will
occupy by your encroachment. If that were true it would in-
jure the port; but I can show that encroachments may be made
to a very considerable extent, without excluding one drop of
tidal water from the estuary. Thus, at the moment it is high
water at Liverpool, or at the moment the tide begins to turn, it
is quite evident that not another drop of water can come in
from that tide; but when it is high water at Liverpool it is not
high water at Runcorn; it takes 55 minutes, say an hour, to
flow up to Runcorn. I procured a man to attend at Runcorn
on Saturday, Sunday and Monday last, it being then spring
tides, to measure how much the tide rose in the last hour of
the flood; and on Sunday night's tide, which was the highest,
it rose five feet in the last hour. Now during the time in
which the tide was rising these five feet, it must have been ex-
panding laterally in the estuary: and if a sea wall or embank-
ment were formed on both sides, coinciding with the line of the
water's edge, at the precise moment of high water at Liverpool,
it is obvious that it would not exclude a single drop of tidal

water, because the water would not touch it until after the tide
was ebbing at Liverpool.   The effect of such contraction would
be the following : the water brought in by the tide would be
compelled to occupy and move in the deeper parts of the estu-
ary, which it would render still deeper ; it must move onwards
towards Runcorn, where it would rise higher, because a reduced
area would be covered by it; its momentum would carry it
further up the river, and its efflux would be accelerated by
reason of its having a greater declivity and a deeper channel to
move in.   I think this is a demonstration, that an encroachment
to a considerable extent, well planned and laid out, might be
made in the upper part of the estuary with great advantage to
Manchester and to Liverpool also.—(Applause.)

The information I have obtained as to the rise of the tide at
Runcorn subsequent to the time of high water at Liverpool, has
enabled me to compute the quantity of water, which, to an
observer at Runcorn, appears to come in during the last hour of
the tide, and it amounts to no less than 77 millions of cubic
yards.

Mr. Palmer, in order to enforce his argument, referred to
some other rivers.   He referred to the Ouse, where a cut was
made two miles and three quarters in length, to prevent a cir-
cuit of six miles ; but the case was not analagous.   The velocity
in the Ouse was much increased by taking a shorter cut ; and
we know that the velocity will be increased inversely as the
square roots of the distance travelled ; in this case very nearly
in the proportion of ten to fifteen, whilst the momentum being
increased with the velocity, an increased scouring would take
place.   But that is not applicable to this part of the Mersey ;
you cannot bring Runcorn and Liverpool nearer together, by
shortening the course, as was the case in the Ouse.   He refers
also to the Severn, and tells us that the tide rises 30 feet at
Swansea, 40 feet at the mouth of the Avon, 50 feet at the New
Passages, 60 feet at the mouth of the Wye, and 70 feet at
Chepstow ; and that he has ascertained this by personal obser-
vation.   Now I know that the tide does not rise 70 feet at
Chepstow, by 25 feet : if it did, a great part of the town would
be overflowed.   Between twenty and thirty years ago I was
employed at Chepstow in the erection of the cast iron bridge
there, and during the progress of the work the depth of water
was registered every tide, when it was ascertained that the rise
of a spring tide is 45 feet, and not 70 feet as stated by Mr.
Palmer: the highest tide which took place during the erection
of the bridge, gave a *depth* of 52 feet 8 inches, *measured from
the bed of the river*, with a depth of about 4 feet at low water,
making a lift of about 48 or 49 feet at that tide, which was the
highest known for about twenty years.

I will here mention a curious fact, although it does not bear

exactly upon the point.   The highest tide at Chepstow usually occurs at the fifth tide after the full and change of the moon, but in this case it was the seventh tide, which ought not to have been a high one; but the tide had actually begun to ebb after high water, and it returned again producing this remarkably high tide: this effect was produced by the wind; a hurricane from the south west having come on a short time before high water, which caused damage to the extent of about £10,000 along the coast of the river.   I believe I have no further observations to make : my intention was principally to contend against the very fallacious reasoning of Mr. Palmer, in reference to the estuary of the Mersey ; as to the improvement which he has suggested in the navigation of the river above it, so far as I understand them, I entirely concur.   (Applause).

Mr. Gibb said, he should like to ask Mr. Buck how it happened that the engineers to the ·Clyde trust had recommended walls to be made, and which had been made for the very purpose of doing away with the estuary which he said was requisite for the Mersey.

Mr. Buck said the Clyde was a different river.   It was a great river, with a trumpet mouth the same as the Severn.   It was similar to the Thames, which did not want an estuary. The river Thames was of itself one great estuary.   The Nore was forty miles from London, and the action in the one would be very similar, if not precisely the same as the other.

Mr. Gibb : You would not compare the Clyde with the Thames.

Mr. Buck : Then I could not compare it with any thing.

Dr. Marshall said, for a considerable number of years they had been narrowing the Clyde, and a great deal of money had been acquired by the proprietors of the land, which had been reclaimed.

Mr. Hawkshaw, the Engineer of the Bolton railway, said, he feared, from the number of their northern brethren present, that, if they got on the Clyde, they should forget there was such a river as the Mersey.   He had not had the opportunity of seeing Mr. Palmer's report before last night, and therefore what he had to say would be principally commentary upon the observations which had already been made.   He wished to state, that he, in common with every one living in Manchester, wished to see the Irwell made navigable ; and, therefore, the observations he was going to make were, not because he was opposed to the measure, but merely on this ground,—that, if such a work was to be undertaken, it would naturally meet with a very strong opposition ; and it was quite clear there would be no chance whatever of succeeding with it, unless it was attempted in the best manner.   It would be utterly impossible

for Manchester to obtain powers for making the upper part of the river navigable, if it was attempted in a manner which would hinder the entrance to the Mersey. Now he did see very considerable difficulty in combining the two. He did not mean to say this difficulty was insurmountable ; but still he did not think Mr. Palmer's plan would accomplish it. In the first place he would state the error in Mr. Palmer's report as to reasoning on certain causes and effects, which might be applicable on that part of the estuary between Liverpool and Runcorn ; and, applying that reasoning to the mouth of the river, he thought what Mr. Palmer suggested would injure the mouth of the river. Mr. Palmer stated, that the accumulations about Runcorn were greatest when the water from the uplands was least. This he believed to be correct, and it must naturally be the case. Mr. Palmer gave this as a reason, among a number of others, why he conceived it was the water of the uplands that kept open the port of Liverpool. But it was quite clear, that when there was a diminution of water from the uplands, that circumstance would also cause an accumulation about the mouth of the river. There was another remark of Mr. Palmer's, which, he thought, would go to prove this. He said, suppose the port of Liverpool was a bay only, it would silt up, which he (Mr. Hawkshaw) believed was quite true. If no water were to come down from the Irwell, then it would silt up. Then, if this was the case, they would accelerate the silting up by taking away the back water. If they took away the back water, it would give it the character of a bay, and that bay would no doubt silt up. His reasons for supposing this were taken from the Ribble and the Dee ; and the port of Liverpool was kept open, in his opinion, from the great press of the column of water, which, after having flowed up by the tide, had to make its egress, and pass out again. Some observations had been made as to the nature of the deposits ; but that did not appear to him of the slightest consequence. He thought the effect of this estuary was to widen it as well as to raise it. He quite agreed with Mr. Palmer, that, for the purpose of getting a sufficient depth of water all the way up the river, it would be desirable to contract the estuary, and give it some regularity of outlet ; but he thought that carrying such a measure into execution would lead to stopping up the mouth of the river, and he did not know how that could be obviated but by continuing the embankments out. That would leave things nearly the same as they now found them,—the quantity flowing in would be diminished, but the force going out would be the same. That would be an enormous work ; but he feared that contracting the upper part of the estuary in the way that Mr. Palmer proposed, would injure the mouth of the river, and, if that was really the case, it would be vain to attempt such a measure. It would follow, therefore, that some other measure would have to

be devised. It appeared to him that the requisites for keeping open the port of Liverpool were opposed to those for making the upper part of the river navigable: at the same time he was of opinion, that that which was requisite for the port of Liverpool, and which at present was the means of keeping it up, became injurious from its own action. He agreed with Mr. Palmer, that the influx of the tide, by washing away the shores of the wide part of the estuary, was gradually filling it up; and he knew that what had formerly been part of the estuary, was now covered with grass. Therefore, in course of time this would silt up, but it would require a protracted space of time, and Liverpool could then be kept open only by contracting the mouth of the river. If the neck of the bottle were made less, the mouth of the river must be contracted also. Mr. Palmer drew a comparison between the eastern and western coast, and thought the cases were not parallel; because, on the eastern coast, he said there were beaches of shingle, the component parts of which must find a resting place somewhere. He (Mr. Hawkshaw) however, could not see any difference, because the component parts were all alike. It was known that there was a tendency to sand banks on the western coast, and nothing, in his opinion, would keep the port open, but the efflux of large bodies of water; and it was his opinion, that the reason of the port of Liverpool having been kept open so long, was its having a large estuary at Runcorn.

Mr. J. F. Bateman said, reference had been made to the Clyde; but there was nothing similar in the two rivers, except the susceptibility of improvement. The depositions in the Clyde were mica slate, and the sand was quite different.

Mr. Lingard said, with regard to what Mr. Buck had said as to Mr. Telford's opinion, he believed, was not quite correct, as Mr. Telford's opinion was, that it was not desirable, as the expense of doing it would be more than the advantage. Now though Mr. Telford might be a clever engineer, yet Manchester gentlemen would be better able to come to a decision, if he would tell them the expense. Mr. Buck, in reference to the Victoria Channel, had said, it was much better to have one good channel than three bad ones. That was exactly in accordance with the views of Mr. Palmer. There were a good many channels in the estuary, and he wished to have but one, and force the water out. Then with regard to the water of the Ouse, Mr. Buck stated, that the cases were not at all analogous. Mr. Palmer did not state them as being analogous; but what Mr. Palmer said was, that the same opinions were held by the parties there, that were now entertained by the parties at Liverpool; but these operations had not been verified in carrying out the work. Mr. Gibb had asked a question as to the nature of the deposits; and Mr.

Fairbairn said, the accumulation at the mouth of the river
was generally, if not entirely, borne down by the floods.
That had not been answered, and he thought it desirable it
should be.   Mr. Hawkshaw* had stated, that the estuary was
considerably wider than it was formerly ; but from some charts
of it as it was a hundred years ago, he believed the width to
be nearly the same.

Mr. Hawkshaw said, he did not mean that the water space
was wider, but the banks had been thrown down.

Mr. Bateman said, he quite agreed with Mr. Palmer as to
the Ouse : but the analogy did not hold.   It was merely done
to drain some fen lands.   The parties at Lynn were afraid,
that the greater scouring power obtained would wash away
the banks ; but the result turned out different, for the river
left the quays.

Mr. Buck said, there was no analogy between the Ouse and
the Mersey below Runcorn ; but it was precisely what Mr.
Palmer proposed to do with the other parts of the river, as
he intended to cut across the tortuosities of the river above.
Mr. Lingard had misapplied his argument, when he said one
good channel was better than several bad ones.   He thought
it better that the channels in the estuary should be separated,
as, if they confined them to one, he had not the slightest
doubt it would silt up.   If the operation of silting had been
going on, no doubt there would have been seen a great al-
teration in the course of a man's life.

Mr. P. Clare said, he was very glad to hear Mr. Buck al-
lude to the mouth of the river.   He thought that was one
of the most important points to be considered.   In reference
to the deposition of silt from the water, he apprehended that,
during the time the water was falling, very little deposition
was going on ; and that it was when the water was at rest,
that the greatest deposition took place.   Now, if they con-
sider the mode in which the water fell into the estuary of the
Mersey, he apprehended that at the commencement of the
flood tide the flow of the water at the bottom of the river
would be nearly at the same velocity as at the surface ; and
at length the water would all flow upwards, then after that
a straightness would take place, and the whole water would
be at rest, and then it was that the deposition took place.
Now he apprehended that the mass of sand which was at the
mouth of the Mersey, was in a great degree occasioned by
the estuary of the Mersey ; and he thought it was very pro-
bable, if the Mersey was scoured in the way proposed, that,
instead of the large accumulation of sand which now existed,
there would be a much less accumulation.   The tide came in
from the west, at right angles to it.   He apprehended that the

water which was filling up above the sand banks towards the sea, was supplied by a lateral current, and that this supply of water at the surface would prevent the water at the lower part being in motion as soon as it otherwise would.

The discussion was then adjourned to Thursday Evening, February 18th.

ADJOURNED CONVERSAZIONE, FEBRUARY 18TH, 1841.

Mr. CONSTERDINE in the Chair.

The Chairman, in commencing the proceedings, said, gentlemen would be aware that this was a second adjournment of the conversazione, arising out of Mr. Lingard's reading of Mr. Palmer's survey of the rivers Mersey and Irwell, with a view to their improvement.* Many gentlemen were present w ose opinions they were desirous to hear ; and it would be desirable for each to be as brief as possible, and to confine himself to the subject of the improvement of the rivers Mersey and Irwell. He then called upon

Mr. Joseph Radford, who said, the additional depth to be obtained in the river, between Manchester and Weston, to attain ten feet water, had alarmed many persons as a very heavy undertaking. He wished to state, that the powers of steam dredging were very little known in this part. Mr. Lingard gave an example of a dredge removing 700 tons per day, and he (Mr. Radford) had much pleasure in bringing before them some valuable facts connected with the subject. An experiment was made in the king's moorings, off Woolwich Dockyard, by a Mr. Hughes, for 14 days. The quantity excavated and lifted by 30 horses' power, from an average depth of 30 feet, was at the rate of 2000 tons a day. He found another example on the Caledonian canal, of a boat of 10 horses' power, lifting 1500 tons a day ; and another example, in 1810, of a dredge boat lifting 2570 tons in ten hours, at the East India Dock-gates, in a contract with the Corporation of the Trinity House. He had here a model of a river boat, made by the son of his partner, Mr. Edward Radford. She was called the *Rochester*, and plied between New York and Albany. Her length was 209 feet 10 inches, 24 feet beam, and 8 feet 6 inches depth of hold ; and she drew four feet water, with an average load of passengers. She made the passage from Albany to New York, on the 14th June, 1837, in ten hours and one minute, the distance being 150 miles. Her average speed was upwards of 14 miles per hour ; and her greatest speed, 16 miles

and a half.   Such a vessel, with the present locks in the Mer-
sey and Irwell lengthened, could take passengers—a thousand
passengers at once—from Manchester to Liverpool ; and, in
Mr. Stephenson's work on engineering in America, amongst
vessels of this kind which were mentioned, was the *Giraffe*,
which was 175 feet long, 26 feet beam, and only 4 feet draft.
He thought these facts very encouraging, as to the passage of
large vessels, with a small draft of water, in our own river. It
appeared, from the proceedings of the last conversazione, that
a little misunderstanding existed as to what Mr. Palmer recom-
mended ; for, although he talked about an alteration of the
estuary from Weston Point to Liverpool, and 34 pages were
devoted to his reasoning, he did not find any recommendation,
or any particular description of the plan of the improvement,
until they opened page 36, where he said, " At present the ex-
tent of the improvements I have to recommend, reaches no fur-
ther down than Weston Point."   But they occupied the whole
of their time last evening in discussions upon the estuary. Mr.
Buck had stated that he approved of Mr. Palmer's plan, as far
a the improvement was proposed between Runcorn and Man-
chester ; but a difference of opinion was expressed concerning
the entire estuary.   Now, on an examination of Mr. Palmer's
plan, he thought they could show that the distance from Run-
corn to Weston was so inconsiderable, compared with the whole
estuary, as to leave very little to differ about.   He should wish
to know whether Mr. Buck objected even to this small altera-
tion, which was included in the distance between Runcorn and
Weston.

Mr. J. F. Bateman said, at the request of Mr. Fairbairn, he
had been induced to bring forward his report on the improve-
ment of the Mersey and Irwell navigation, to which that gentle-
man alluded in his letter.   It was prepared last spring, after
an investigation he had been induced to make on the improve-
ments which might be effected in the river at Runcorn ; but it
was withheld in consequence of the subject being then under
investigation by Mr. Palmer.   Mr. Bateman then read the re-
port, of which the following is a copy.

RUNCORN GAP.

To the Company of Proprietors of the Mersey and Irwell
Navigation.

" Gentlemen.—In my recent investigation at Runcorn, and the
best means of improving the navigation there, I was led to the con-
sideration of the general improvement of the river Mersey, and
particularly of that part which lies between Runcorn and Warring-
ton.   A mode of effecting this in a manner which appeared to me
likely to be beneficial to every party interested, suggested itself ;

and, in the belief that it is deserving your attention and considera-
tion, I take the liberty of laying it before you.

" The improvement of the river for navigable purposes is a sub-
ject of great importance to the proprietors of the navigation—to the
town of Warrington, and to all who can participate in the advan-
tages which may be expected to result. It is a subject which has
frequently excited the most serious attention, and it appears re-
cently to have been taken up with a spirit from which some practi-
cal and useful result may be confidently expected.

" The river possesses within itself the means of very great im-
provement; and I am convinced, that, if these resources were
sufficiently investigated and developed, no great length of time
would elapse before we should see vessels of three or four times the
present burden, unloading their cargoes at the quays of Manchester.

" It is becoming of daily increasing importance, when we con-
sider the vast impetus which must be given to the trade of Man-
chester and its neighbourhood, by the many important railroads
which are now constructing—the great increase in the carriage of
merchandise which may consequently be expected—the important
benefits which the Inland Bonding Bill, if suffered to pass into a
law, will confer upon the town, and the probable increase in the
carriage from that cause also—with the necessity of carrying the
facilities of inland navigation to the highest pitch of perfection, in
order to cope with the powerful rivalry of collateral railroads.

" The river, as far as the navigation extends, may be considered as
naturally divided into three parts; from Liverpool to Runcorn;
from Runcorn to Warrington; and from Warrington to Man-
chester.

" The first is a wide and open estuary or inlet from the sea, navi-
gable at high water of all tides, for vessels of considerable burden;
and being from its nature susceptible of little improvement beyond
the deepening and straightening of the channels. At high water,
it is for the most part from two to three miles in width; but, at low
water, the channel is generally not more than 200 or 300 yards.
Upon this portion of the river, steamers ply regularly at every tide,
between Liverpool and the various canals which enter the river near
the town of Runcorn, for the conveyance of goods and passengers,
and for tugging vessels; and it forms the utmost extent to which
the natural navigation of the river, assisted by the tides, can be
regularly and certainly made.

" The second division forms the upper end of the estuary, separ-
ated from the lower part by a narrow strait called Runcorn Gap,
where the opposite rocky shores approach to within about 400 yards
of each other, projecting considerably within the limits of high
water, both above and below. It is nearly a mile wide at the lower
end, and terminates upwards in the ordinary channel of the river,
which is probably about a hundred yards in width. It is only
navigable at high water of spring tides, for vessels of more than

40 or 50 tons burden, and has been found so beset with inconveniences and difficulties, that the navigation of it has been nearly abandoned, artificial canals having been constructed inland, for the purpose of carrying on the communication.

"The third portion lies above the reach and influence of the tides, and is strictly an artificial river navigation, having been rendered available for that purpose by locks and weirs, to the town of Manchester, and shortened and straightened in various parts by artificial cuts.   It is only now, however, capable of being used by vessels ordinarily about 40 or 50 tons burden, drawing about four feet of water.   The depths of the pools vary considerably, being in many cases 10 or 11 feet, and in others not more than four or five feet.

"The navigation of this part is capable of being greatly improved, and may be adapted at a reasonable expense to the conveyance of vessels of 150 tons burden, or probably more.

"Several bridges would prevent the passage of high-masted vessels; but all steamers, and such vessels as could sufficiently lower their masts, might make the entire navigation.   This is perhaps now of less importance than it would formerly have appeared, as, from the rapid progress steam navigation has recently made, we may reasonably expect a very large proportion of the trade will be carried on by that means; while, to a considerable extent also, vessels expressly adapted to the circumstances of the navigation, would no doubt be constructed.   A survey for the purpose of reporting the most effectual means of accomplishing the improvement of this part of the river is now in progress, and I have little doubt the report will be of a satisfactory nature.

"The main difficulty in the way of a general improvement to the town of Manchester, so as to take vessels of the size above mentioned, appears to exist in the inconvenient state of the navigation between Runcorn and Warrington; and it is to the improvement of that portion of the river that my attention has been particularly drawn, and to which I shall confine my observations.

"Whether any definite plan for the improvement of this part, or the removal of its natural difficulties, has ever been proposed, I am not aware   but from the opposition which all attempts to carry bridges over the estuary at, or above, Runcorn Gap have been met with, and from the jealousy with which any encroachment on the tideway has been watched, the general impression seems to have been that it was necessary to keep it in its present state!—that of an open unobstructed tidal river.

"I rather think there has been, generally, a kind of vague idea, that some important plan of improvement would sometime or other be projected, and an apprehension that any alteration in the river might tend to prevent the accomplishment of the anticipated scheme; and, therefore, all parties have been particularly anxious to keep it in its natural and original state.

" The examination I have made of the river with information obtained respecting it, and a careful consideration of all the circumstances connected with it, have led me, however, to the conclusion that so long as the river above Runcorn remains an open estuary, washed over by the tide, it will be impossible to effect (except at an enormous expense) any advantageous or permanent improvement.

" The main difficulties under which this part of the navigation labours, are want of sufficient depth of water to carry vessels of any size up to Warrington, except during high spring tides—the short period of time during which it can even then be done—the circuitous and ever changing channels—and the constant alterations of the sand-banks which are operated on and shifted both by tides and land floods.

" To remove these difficulties—to secure a constant and unchanging channel of sufficient depth to allow nearly all vessels to go up to Warrington at any state of the tide, that can reach Runcorn Gap —to give a longer period of time during which the navigation can be made—to do away with the danger and annoyance of being neaped on sand banks, as at present—and to do all at a reasonable and warrantable expense, and so as not to injure the navigation of the port of Liverpool, nor injuriously to affect any other interest, is the end to be desired, and the end which, I hope to be able to show, the plan I have to suggest will be sufficient to attain.

" I have mentioned, that the width of the river at Runcorn Gap is about 400 yards, and it is bounded at each side by precipitous rocks. The tides here, even when pressed by strong winds, never rise more than 20 or 21 feet; and at low water the greatest portion of the channel is dry, there being little more than a few feet of water in any part.

" The plan I have to propose is to throw an embankment across the river at this place, with proper and sufficient locks and flood gates to admit and discharge the tidal waters under certain regulations.

" Were the question merely confined to the best means of improving the navigation from Runcorn upwards, without reference to any effect to be produced below, a simple embankment or weir, with self-acting flood-gates to admit and impound the high tide water, with such locks as might be necessary for the navigation, would be all that would be required; for by that means you would have a pool constantly filled, deep enough to float vessels to and from Warrington, at every hour of the day, drawing 12 or 14 feet of water.

" But it becomes a question as to how far the obstruction to the flow of so much tidal water, with its scouring effect upon the channel during ebb tide, would affect the entrance to the port of Liverpool, or the navigation from Liverpool to Runcorn; and I am of opinion, that, unless measures were adopted to prevent it, an em-

bankment only, which would constantly keep up the water, would
have an injurious tendency.

" To prevent this, and for the purpose of always maintaining a
deep channel (and I believe in a more effectual manner than can
now be done), I would propose the construction of sufficiently ca-
pacious flood-gates to discharge at half ebb of spring tides, when
the most effectual scour is going on, the whole body of water
which is impounded, refilling the pool at the next tide.

" Having thus stated generally the nature of the plan, I will
proceed to explain it more in detail, to point out what I consider
its advantages, and to investigate the objections which, it appears
to me, may be urged against it.

" The average height of the tides at Liverpool over the old dock
silt, is about 15 feet—the highest being about 21 feet, and the
lowest 10 feet.   These measured from low water are respectively
about 33 feet and 23 feet.   "

" An 18-feet tide at Liverpool, being an average spring tide, and
about 30 feet in the river, will rise about 15 feet at Runcorn, and 8
feet at Bank Quay, near Warrington.

" Such a tide will allow vessels drawing 13 feet to reach Run-
corn, and such as draw 8 feet, about 100 tons burden, to go for-
wards to Bank Quay.   A neap tide will scarcely bring a vessel
drawing 8 feet to Runcorn, and it will carry nothing at all (but a
flat, perhaps) to Warrington.

" The average of vessels drawing the greatest depth of water
which reach Runcorn, may probably be taken at 10 feet, varying
from 100 to 200 tons burden; and this size includes nearly all
the Coasters, those engaged in the Irish provision trade, and
Steamers.

" At present, such vessels can only get forward to Warrington,
at the very highest spring tides, perhaps two or three times in the
course of the year; but, by the plan suggested, they will be able
to do so as often as they can reach Runcorn; and, when once at
Warrington, all steamers, and such vessels as can lower their masts,
may go on to Manchester, when the necessary improvements on
that portion of the river are effected.   •

" It seems that the difference in the depth of water between
Runcorn and Bank Quay at high tide is about 7 feet.   Of this
I am inclined to think 4 or 5 feet is attributable to the natural
declivity of the ground, and the remaining 2 or 3 feet to the fall
in the surface of the flood tide, which, I apprehend, never at-
tains the same relative height at Bank Quay as at Runcorn.   If
I am right in this conjecture, the effect of an embankment will
be as follows :—

" A tide rising 15 feet at Runcorn will (as I have shown before)
give as the river is at present, 8 feet of water at high tide at Bank
Quay ; but supposing this tide to be retained at Runcorn, and

prevented from flowing back, the water would gradually level it-
self, by rising at Bank Quay, and falling at Runcorn; and if
the width of the river were the same from one end to the other, and
the difference to begin with was 3 feet, it would rise 1 foot 6 in-
ches at Bank Quay, making the depth of water there 9 feet 6
inches, and fall the same amount, 1 foot 6 inches at Runcorn
reducing that depth to 13 feet 6 inches. As the river, however,
is much wider at the lower than the upper end, the fall at Run-
corn would be less than half the amount of the difference, and
the rise at Bank Quay more than half,—making the depth there
probably ten feet. Suppose further, that the land or river water
was allowed to flow into the pool, so as to raise the entire surface
to the level of the original tide, 15 feet at Runcorn, which would
occupy about a day and a half, there would be a depth of 11 feet
at Bank Quay; and, supposing the river is then allowed to flow
on through the pool as usual, we must add the fall or declivity
in the surface necessary to give it the requisite velocity;—this
would be about 2 or 3 inches in a mile, and the distance being,
say 7 miles, we should have an additional depth of from 1 foot 2
inches to 1 foot 9 inches to add, making the total depth at Bank
Quay from 12 to 13 feet, being a gain of from 4 to 5 feet depth
of water.

" As this depth is 2 or three feet more than is required to
float a vessel of 10 feet draught, it will be sufficient if we retain
a tide rising 12 or 13 feet at Runcorn, or 15 or 16 feet over the
old dock sill at Liverpool.   It is of importance to mark this,
as you will perceive by observations I shall have to make upon
the scouring power I propose to substitute.

" Laying aside for the present any consideration of the effect
which may be produced below Runcorn, I can see no objection
which can reasonably be urged against it, but the possibility of the
river gradually silting up, by the deposition of material brought
down by floods.   The mode I have to suggest of scouring out the
channel, will, I think, almost entirely remove the possibility of this
being the case, in the navigable channel; but, even without that,
I do not think it would have such an effect.   The river would
maintain its course and current along the deep, depositing whatever
it might bring down on the sand banks and shallows at each side,
where there would be little or no current, thereby gradually raising
and preparing for agricultural purposes, an unprofitable waste of
sands, washed over now by every high tide by which they are fre-
quently removed and carried into the deeps.

" I know many instances of rivers maintaining a distinct course
through large lakes; but two, which must be familiar to nearly
everybody, will be sufficient to mention.   The Rhone through the
Lake of Geneva, a distance of 37 miles, and the river Bann, for 18
miles through Lough Neagh, in Ireland ; each river maintaining
a deep and distinct channel through the entire length of lake. The
Rhone, however, and, I have no doubt, the Bann also, forms a delta
on first entering the lake.

" I think that, generally, the channel would be improved ; and if deposit was to take place in the upper part of the estuary, where the river would first enter into comparatively still water, it might easily be removed by dredging. *

" The benefits to the town of Warrington, in particular, must be too obvious to need any remark.   The Sankey Canal would obtain a much better entrance than it has now; and the Mersey and Irwell Company would have so much of their navigation permanently improved, and rendered available for a large class of vessels, which they may then take on to Manchester.

" We now come to consider the effect which may be produced upon the channel below Runcorn Gap, and upon the entrance to the port of Liverpool.

" It would be of little use to suggest plans for the improvement of the upper part of a river, if the mouth were to become so choked up that no vessels could enter; and, in the maintenance of a good entrance to the port of Liverpool, the Mersey and Irwell Canal Company is as vitally interested as any party can be.

" I hope to be able to show, that, so far from the suggested works being likely to do injury, they will assist in scouring out and deepening the channels all the way out to sea. *

" Much evidence was given, in the trial betwixt the Old Quay Co. and the corporation of Liverpool, in 1837, relative to the scour of the river ; and from that it appears, that the most effectual in cleansing and deepening the channels is that produced by the ebb tide, when about half down, and the land floods; the latter losing much of their power, however, in the lower part of the estuary.

" As this accords strictly with my own observation, and the information of those connected with the river and daily navigating it, I have no hesitation in taking it as the fact.

" It appears, then, that the early part of the ebb tide is of little service in improving the navigable channels of the river ; and indeed this must be obvious, when it is considered that the water is then running with pretty nearly equal velocity over the whole bed of the river, and removing probably more sand from the banks into the channels that it carries out of them.   •

" Now, if any considerable portion of the water that is thus wasted, as it were, could be retained until the tide was half down, and then set at liberty, it would have the effect of keeping up the river for some hours longer at the most effectual scouring point, and be thus enabled to work deeper into the channels, and carry the sand or silt removed further out to sea.

" I think I can make it clear, that this will be the result of the scheme proposed during spring tides ; and that, during neap tides, or whenever prevented from flowing beyond the gap, the water will rise higher at Runcorn than it can now, and consequently increase

the velocity of the ebb. In either case there will be a strong tendency to improve the channels both above and below Liverpool. The estuary, to begin with, will contain nearly, if not quite, as much tidal water as it does now, and under regulations which will render it of more effectual service, while eventually the improvement of the deeps will enlarge its capacity.

" The upper part of the estuary and river, from Runcorn Gap to Howley Weir, at Warrington, containing at high water of spring tide (including Halton Marsh) about 1,300 acres, is about 1-17th of the entire area of the estuary above Rock Perch. In spring tides, at high water, it contains from 1-30th to 1-25th, and in neap tides from 1-50th to 1-40th of the whole body of water.

" Mr. Giles, in is evidence for the corporation at Lancaster in the suit before referred to, calculates the contents of the river at ordinary spring tides, from Runcorn to Warrington Bridge, at 10 ⅓ million tons, or about 13,733,000 cubic yards. As a 15-feet tide at Runcorn falls 8 feet to half ebb, considerably more than half the quantity has flowed out before that time, so that the remainder, say six million cubic yards, is the only portion that is effectually employed in scouring the deep. As this is six hours in ebbing out, the velocity becomes so trifling towards the end as to be ineffectual.

" In neap tides the effect is proportionably less.

" The late Mr. Nimmo, in his evidence for the company in the same cause, gives from actual measurement the ordinary flow of the river above Warrington, and the depth of a very heavy flood over Woolston weir, from which I have been able to ascertain its volume.

" From Mr. Nimmo's observations, the fair average of the ordinary quantity may be taken at 40,000 cubic feet, or 1,480 cubic yards per minute.

" The flood appears to have been about 580,320 cubic feet, or 21,493 1-3 cubic yards per minute, or nearly one million and a half an hour.—probably nearly equal to the tide a half ebb. It was running at the rate of 113 yards in a minute, or nearly four miles an hour.

" It is half ebb at Runcorn rather earlier than at Liverpool; and from half ebb to the commencement of the flood tide at Liverpool, there is about three hours. It is during the period that I would propose to discharge the water which would be retained above our embankment.

" I have stated, that a 15-feet tide at Runcorn has fallen eight feet, or to half ebb. If flood-gates were constructed in the bank, 60 yards in length, 8 feet in depth, and opened at half ebb so as to obtain an average pressure of eight feet to the bottom of the discharge, the quantity discharged in the three hours would be nearly six million cubic yards, or about the whole quantity now

contained in the estuary with a similar tide at half ebb, and requiring six hours to flow out.

" If the discharge sluices occupied 100 yards in length, instead of 60, being then 1-4th of the width of the gap, the discharge in the three hours would be more than nine millions and a half cubic yards, being half as much again as all the water now left in the estuary at half ebb, and more than 2-3ds of the whole contents measured at high water of spring tides, and nearly equal to the whole quantity at half ebb added to three hours of such a flood as Mr. Nimmo mentioned.   The discharge would be at a velocity of 10 feet per second, or nearly seven miles an hour. and would, after mixing with the other water, maintain a velocity of three or four miles, which is much greater than the mean velocity after half ebb at present.

" There cannot be a doubt, I think, that, under such regulations, the scouring power would be greatly increased; and, while below the gap. the direct force of this power would be employed in deepening the channel and carrying out the sand and silt to sea, the velocity of the current above the gap would be so much increased and confined to a particular direction, that the channels there would also be deepened, and any casual deposit carried out: so that, independent of other improvements, the channels of the whole river would be improved from Warrington to the sea.

" After these discharges the pool might be refilled at the next tide, or whenever the tide rose more than 13 feet at Runcorn.   At the lowest spring tides, for three or four days together, and at the highest, for seven or eight days together; perhaps twice each day; but at any rate, every alternate tide, and even much less frequently than this would, I am satisfied, be found amply sufficient.

" The next point is, that, by the tides being prevented from flowing beyond Runcorn Gap, they would rise higher there, and, by thus attaining a greater head or elevation, which will be an additional advantage, would produce an increased velocity in the ebb.

" The tide flows past Runcorn at the rate of five miles an hour; and if stopped there by an embankment, and prevented from flowing up to Warrington, and filling that part of the estuary, the momentum, which impels it forward for an hour after it has turned at Liverpool, would cause it to impound in front of the embankment.   From calculations I have made, I am disposed to think that the additional rise would probably be about 1-20th of the total depth of water, or from four to nine inches, according to the height of the tide.   This amount, small as it appears, would be of service in neap tides.

" I have now, I think, gone over the main points which appear to me materially to bear upon the question; and I hope I have succeeded in explaining them in such a manner as to render them intelligible, and enable you to understand my views.

" If I am any thing nearly right in the data I have taken,

and the conclusions I have drawn from the calculations I have made, the advantages in every point of view must be considerable ; nor are these advantages confined to the navigation only ; the adjoining landowners may reclaim a large portion of the land above Runcorn, which is now covered at high tides ; a good road, with draw or swivel bridges over the locks, may be formed on the top of the embankment, and thus join the two counties of Lancaster and Chester in a very much superior and more convenient manner than is now afforded by the dangerous and inconvenient ferry.   Even a railway viaduct, if carried at a sufficient height, would then be no objection ; and many miles of railway travelling might be saved to the London and Liverpool traffic, by crossing here, and joining the Grand Junction at Prestonbrook.

    " It only remains to explain shortly the kind of works which would be required, which will be facilitated by reference to plates iv and v.

    " The width of the strait at Runcorn Gap is about 1,250 feet.   The bed of the river consists of about 35 feet of rock on the Cheshire side, dry at low water ; about 745 feet of sand and silt in the middle of the river, extending, I believe, to a considerable depth, partially dry at low water ; and about 470 feet of solid rock, all above low water, on the Lancashire side.   The rock extends inland on each side, rising considerably, particularly on the Cheshire side, above high water level.

    " I would propose to construct two sea locks *in* the rock on Cheshire side ; one 180 feet by 40 feet, and the other 120 feet by 30 feet, with hydraulic gates, so that they may be self-acting, and used for the purpose of scouring.   In the rock on the Lancashire side, I would recommend the construction of the self-acting flood-gates, and between the limits of high and low water there is ample space for *ten*, with 20 feet clear openings in each ; the gates to be revolving on an upright axle, placed a little on one side of the centre, so that one leaf of the gate should be rather larger than the other.   The gate, of course, must open only one way, the larger half turning *up* the river : when therefore, the flood tide rises higher than the surface of the water on the upper side of the gates, the pressure being greater upon the larger leaf than the smaller, the gate opens, and the water is freely admitted.   When the tide has reached its greatest height, and begins to fall, the pressure is then reversed, and the gate closes, retaining all the water that has flowed past the embankment.   To open the gate, and discharge the water, *en masse*, various methods might be adopted.   The simplest, perhaps, would be to draw up out of the larger leaf a paddle of sufficient size to make the smaller leaf expose a greater surface to the pressure of the water, when, of course, the gates would open by the down-stream pressure, as they would in the other case by the up-stream pressure.   The pad-

dles may be worked by self-acting balance weights, or by a
water wheel set in motion by the fall of the tide, so as to make
the whole self-acting.   The water, after its discharge, may be
directed by proper jetties into the channel required.

" Over the intermediate space of sand and silt, betwixt rock
and rock, I would propose an embankment composed of rock
and earth, in the manner shown in the drawing ; the centre of
the bank of puddled earth or clay ; and the other parts of rock
faced with heavy squared pitching, brought up from low water
in a curved manner, as shown in the drawing.   In order to se-
cure as far as possible, or necessary, the water tightness of the
bank, I would recommend a row of sheet piling, perhaps 25 or
30 feet deep on each side of the puddle wall in the centre of
the bank, and at the foot of each slope another row of shorter
piles, to prevent the pressure of the bank forcing out or blow-
ing up the sand foundation.

" A carriage road to be formed over the whole, passing over
the locks by draw or swivel bridges, and over the sluices by
stone or wooden arches.

" This plan, with 15 feet of water impounded, would afford
a sectional area of discharge of 5,970 square feet.   The calcu-
lations in my report are made upon an area 2,400 square feet
only, so that, if by that amount the scouring power was trebled,
it would, by using all the means which the locks and sluices of
the plan just detailed afford, be increased more than sevenfold.

" At a ten-feet tide at Runcorn, the sectional area of the
stream is now about 9,800 square feet.   The locks and sluices
would afford at the same height about 4,120 square feet.   Al-
though this is less than half the present sectional area, a differ-
ence in level of considerably under a foot would so increase the
velocity through the sluices as to pass the same quantity of
water."

He might perhaps mention, that the first effect of this scour-
ing might be injurious to the bar at Liverpool.   The embank-
ment would be so at this point—[See the section of the river
at Runcorn Gap, plate iv].   The tide would be allowed to fall
eight feet before it was discharged, and no doubt the sand would
be carried out to the deep part.   Such an immense body of water
was impounded above the estuary at Liverpool, that, in order to
accomplish a passage for itself, it had excavated the ground to a
great depth ; and, notwithstanding the enormous quantity of sand
brought down, the velocity of the current was sufficient to
carry every particle past Liverpool, leaving a bare hard rock
channel 89 feet deep.   The sand was all carried out, and was
not deposited again until eight miles below the Rock Light-
house.   Therefore whatever was excavated from the estuary
above Liverpool, would be carried down to the deep part at

Liverpool. The velocity at Liverpool was about seven miles; down at the bar, about two miles; and it seemed that, when the velocity was so small, the sand deposited itself at this bar, and it might require constant dredging and other precaution-ary measures on the part of the parties interested in the port at Liverpool, to keep it in its present state; but, eventually, the scouring from the upper part of the estuary would increase its capacity, and every tide that flowed would bring in a larger quantity of water; the same quantity having to flow out, the scouring power would be increased, and the bar removed fur-ther into the sea. The greater the momentum, the further was the sand carried out, and they must take the harbour of Liver-pool as they found it. If it was a new thing to make a chan-nel to the sea, then he had no doubt Mr. Palmer's plan would work as well as any other. But they found sandbanks carried out eight miles, and they must not decrease the velocity up to that point, as, if they did, the sand would gradually silt up towards Liverpool, and the whole of the harbour would even-tually become choked.

The chairman then introduced Mr. Ritson, who, he said, was a gentleman of considerable nautical experience.

Mr. Ritson said, he did not expect to be called upon, because his knowledge of nautical affairs did not apply exactly on the present occasion. The case before them was one connected with the navigation of the Mersey from the entrance of the river to Manchester. He considered this question to be of great im-portance, involving matters of a serious nature, both as regarded Liverpool as a port and as the handmaid to Manchester, and also of great importance to Manchester as a town. As far as his observation had gone, with reference to the tides, he would give it to them; and probably from it they might judge whether the navigation to Liverpool would be affected by the contem-plated project now before them. It had been stated in that room, and properly so too, that the tidal wave flowed from the western ocean in the first instance. It first struck on the French coast, Scilly Island, Land's End, Cape Clear, and Toree Island, all at one and the same time, making high water at these points somewhere about half-past four o'clock. It proceeded then re-gularly, with some interruptions as it regarded Chepstow, up to Milford. Between Milford and Tuscar there was an irregularity in consequence of the contraction of the channel. The chan-nel between Milford and Tuscar was 30 miles across, and the depth 40 fathoms. This was one portion of the south entrance to the Irish Sea. Between Mull and Kentare, on the Irish coast, the average depth was five fathoms, and the water, coming in between these points, occupied a space of 15,000 miles and three fathoms deep; and it was a curious fact, that, as the tide flowed up from the Western Ocean, in the manner he had

described, that the water on the Welsh shore, and on the Eng-
lish shore, rose to the height of four fathoms; while, on the
Irish coast, the vertical rise was only two fathoms.   Conse-
quently, they had all the advantage, on the English side, of a
greater vertical rise; as, when it was high water on this side,
in some places, it was high water at the same time on the other
side.   It also threw in an inexhaustible supply of water.   The
quantity of water thus thrown in, aided by the pressure of the
wind, which had a serious and important effect, for practice,
one ounce of which was better than a pound of theory, had
shown that the water thrown up into the Irish Sea by the south-
west winds, (and winds prevailed from the westward near nine
months out of twelve in this hemisphere, and they would fre-
quently observe that, during the south-west gales, the wind sud-
denly shifted round to the north-west) caught, by the wind from
the north-west, a great body of water at right angles, which
had no means of escape but by coming into the estuary of the
Mersey.   The Mersey was nearly at the angle of the coast.
The Lancashire coast, down at Lancaster, formed a line in that
direction, so that the entrance to the Mersey was at the corner
of the square, as it were.   The quantity of the water thus
thrown up into the Mersey would aid the very able project as
stated by Mr. Bateman, as they would, by that means, get a
large quantity of water up the estuary.   He did not think, in
reference to Mr. Palmer's plan, that it would be fair, in that
gentleman's absence, to make any observations upon it; but, in
an abstract view of the question, that gentleman would be per-
fectly right, for they would want water.   Water would be
wanted, and more water still.   They might draw it from the sea
by means of machinery, which would raise them water to any
extent.   Furthermore, there were the resources from the back
country, Bolton, Bury, and that neighbourhood, from which, at
a small cost, they might impound a large quantity during floods.
Liverpool, and he said it with honesty, was the handmaid to
Manchester; and he should be extremely sorry to see anything
carried out that was detrimental to her interests.   But every
one must do the best for themselves.   The millions of money
which had been laid out in Liverpool in docks and other accom-
modations, and the very spirited manner in which her business
was conducted, aided by Manchester, would make the two
towns stand, as, indeed they did now, the first not only in this
empire, but in the world.   Her merchants, aided by Manches-
ter, had been the pioneers of our commerce; and he should be
extremely sorry to see any thing done that might deteriorate
her.   The plan proposed, as far as the river was concerned
above Runcorn, was by confining it in parallel lines.   They
would then get a sufficient quantity of water, and would clear
away all the silt; for he perceived, in looking at the plan, that
the influx and reflux of the tide caused eddies.   These eddies,

at low water, became quiescent, and made deposits; but, by narrowing the estuary, they increased the scouring power. But he would call their attention to another point, which Mr. Radford had alluded to,—the case• of the dredging machine. He would travel, in this instance, as far as the Thames on the one hand, and the Clyde on the other. The Clyde was now rendered navigable by the dredging machine; and the Thames was navigable above and below bridge, to a great extent, by it. He did not need, in that room, to make an objection as to the Mersey being stopped up ; for, so long as rain dropped from the clouds, the hills remained where they are, and the tide flowed in the sea, they could never stop up that river. The river Mersey had been a river ever since the Romans came into this country, that was, for 2000 years ; and he had seen a plan of the river as it was at that time; but he would give 400 or 500 years, and say the river had been open for navigation 1500 years ; that was saying a great deal, but he believed it to be the fact ; and was it to be supposed, that what they could do now would affect it, when they looked at the Victoria Channel ? He could remember the time when they could not enter by that channel. By the dredging machine they could keep the navigation open, and there was no necessity to use any others means. •

Dr. Black said, there had been considerable difference of opinion as to the nature of the deposits. He liked practical matters ; and he wrote to a correspondent last Saturday to send him up a sample of the deposits both from Runcorn and the mouth of the estuary at Liverpool, which he now laid before the meeting. One was taken at low water from the deposits by the Rock Lighthouse, and the other from a mile below Runcorn, at low-water mark also. There was not much difference in their appearance. He had analysed that taken from the river at Liverpool. One half of it was siliceous sand, extremely fine, and the other half consisted of aluminous, carbonaceous, and calcareous matter. The other sample he had not had time to analyse. It seemed, however, to be nearly the same, but to contain more of the aluminous substance. If the sands came from the sea, they might expect to find in them, pure, some remains of marine productions. But, if they came down the river, they should find siliceous matter, and that which formed the basis of all clay bodies and calcareous matter ; they might come from towards Cheshire and down the Mersey, and this matter would be from what was used for agricultural and other purposes. He had dried part of the specimen, which might be examined with a microscope. He would leave gentlemen who had a little knowledge of geology, and of the currents of the river, to judge from whence these deposits came. It was easy to account for them coming from the interior. A very small velocity in a river would carry matter a very long way. He

should merely make another observation, with respect to what Mr. Bateman had so very judiciously and mathematically stated, as to impounding the water at Runcorn, that it would seem that nature had anticipated him. It appeared to him, from the arrangement of the rocks at Runcorn, that the water had once been impounded there. On the Cheshire side they were the same as on the Lancashire side ; and it appeared, that in process of time, the attrition of the water had worn away these rocks, and there appeared to have been an elevation there by some revolution of nature. There appeared to him every reason to suppose so from geological appearances ; and that Mr Bateman had been anticipated by nature some 4,000 or 5,000 years ago.

Mr. Thomas Hopkins said, he had partly read and partly heard the discussion which had taken place on that subject in that room, and had felt a strong degree of interest in it ; but he should not have said any thing, if he had not found, that, during the whole discussion, one point had been, not overlooked, but rather assumed. He alluded to the possibility of excavating the Mersey, as far as the tide proceeded at present, or somewhere thereabouts. It had been alluded to as though it would be impossible to do it. He presumed that the impossibility would be the great expense ; but if it were possible, looking at what had been done at other places, it would appear that that was the best course. They had been told, that dredging machines were capable of excavating to an enormous extent, at a comparatively moderate cost. Now, supposing it to be possible, if they were to excavate a new channel from the estuary a little above Runcorn up to Warrington, at a moderate expense, it would be beneficial in accordance with what had been done at the Clyde. It was stated, that the tide rose seven feet at Bank Quay, and fifteen feet at Runcorn. If the river were excavated there to a moderate depth, if this were practicable, it would remove the objection arising from the apprehension that the river beyond Rock Perch would be blocked up. It was generally admitted, that the quantity of water which fell into the Mersey and out again, constituted the 'scouring power. Now, if they excavated the bed, they would have an additional quantity coming in and afterwards carried out, and consequently the scouring power would be increased. In Mr. Bateman's plan, though provision was made that the water would flow out in sufficient abundance to scour the river below, yet he could not perceive that he had made provision for the flow in of the water through his locks. He apprehended there would be sufficient pressure above to drive the water out ; but when the tide was coming in, he would not have an equal pressure ; and therefore he apprehended, that the flow upwards would be impeded. He rose principally for the purpose of sug-

gesting the practicability of excavating the river a long way up, say as far as Warrington ; and some persons present, he knew, had been surprised that that point had not been more dwelt upon. •

Mr. Heelis said, he would have been sorry to have obtruded himself, if he had not felt capable of throwing some light upon the subject; not from his own knowledge, but from that of other parties. He would address himself for a few moments to what Mr. Hopkins had said. He believed that between Runcorn and Warrington it would be found that the meandering of the river was such as to render it undesirable to take the course he recommended. He then referred to opinions which had been given by Mr. Telford and Mr. Nimmo, the substance of which was, that the effect of back water varied in each particular case, and that there were few more difficult subjects for engineers to give an opinion upon. The Liverpool gentry were not always in the same way, and it was exceedingly difficult to catch what they would be at when their interest was concerned. He held in his hand a bill dated 1837, for better improving the port of Liverpool. It was proposed by that bill to invest the conservancy of the river in various commissioners, the chief portion of whom were from Liverpool ; and these commissioners intended to take power to do a variety of things. First of all there were to be mark stones, which were to designate high-water mark at 29 feet 3 inches ; and then it went on to say, that parties might build up to this 29 feet 3 inches. The 24th clause was to give them power to alter divers embankments, and do a variety of other things, for the purpose of scouring and improving the river. It seemed, therefore, to him, that they were then not exactly satisfied in their own minds, as to whether the river should remain in the same state. He perceived, by the papers, that there had been some difference of opinion as to the time occupied in the flowing and ebbing tide. He believed it would be found that it varied much at various places. At Runcorn, the flood was 2½ hours, and the ebb 10 hours. There was one circumstance he might mention. At the trial with the Old Quay Company, one of the most intelligent witnesses was a man of the name of Peacock, who, he regretted to hear, had met his death by being carried away by the velocity of the flood tide. He was knocked overboard ; and, though the water was not more than up to his middle, he was absolutely carried away. Now, he thought Captain Denham would have great difficulty in making them think, that the velocity of the ebb tide was such as to cause loss of life. Then, it had been stated, and pretty broadly, that the Grand Junction Railway was an obstacle which could not be got clear of. But he would suggest that it was possible to give the Grand Junction Railway Company what they had been for years wanting, and at the same time allowing them to make the navigation. They had tried several times to get

across the river at Marsh Gate, Cuerdley Marsh. Now Mr. Palmer, he found, intended to make his basin there ; and it would be an easy matter to allow them to go over the river at that point, not perhaps at an elevation which would enable a sea-going vessel to pass under the bridge, but they might go over it by a drawbridge. He thought also it would be well, before this discussion was brought to a close, that there should be a right apprehension as to what Mr. Palmer had recommended, in reference to getting over the difficulty at Barton. He was afraid there was a notion abroad that he considered it essential that they should go on a level with the Bridgewater Canal. Now he did not know, that he said any such thing. By the improvements which had been made in the construction of steamers, as sea-going vessels, they might reasonably hope that the aqueduct at Barton would not present any such great obstacle but such as a little good engineering might obviate.

Mr. J. C. Dyer said, he had not read Mr. Palmer's report carefully through ; but it appeared to him, all things considered, the best plan that could be devised for improving the navigation for sea-going vessels to Manchester, and that it could be devised without damaging the port of Liverpool, and the consequent interests of the people of Liverpool. So far as an honest and fair ground of apprehension existed on the part of the Liverpool people, the people of Manchester, as fair-minded men, not wishing to benefit themselves at the injury of their neighbours, ought to take the subject into consideration, and not embark in any thing unless they were convinced that what they were going to do would not endanger the port of Liverpool, as by damaging it they damaged themselves. It was his humble opinion, that, by narrowing the estuary, they should not damage the port of Liverpool, but rather benefit it. He would call to their mind the simple fact that the quantity of matter which the water could hold in suspension was according to its velocity. When the water fell in, in large estuaries like this, there was a comparative stillness, and a diminished velocity ; and consequently these particles were beginning to be deposited the moment the estuary widened, and the velocity, consequently, was diminished. This, like all other estuaries, was rapidly filling up ; and it was in vain to tell him, that soundlings had been taken, and that there was no perceptible change. Another circumstance should be borne in mind. If, by Mr. Palmer's plan, three-fourths of the water now brought into it also excluded three-fourths of the matter held in suspension, while they had the same scouring power from the uplands, then they should reclaim an immense tract of land, and make the navigation suitable for the wants of Manchester, without any way injuring the port of Liverpool. It had been stated, that Mr. Palmer's plan was opposed by all the engineers : but he had occasion lately to speak to some eminent engineers, who spoke very highly of it.

Mr. Heelis said, he might be allowed to state, in corroboration of what Mr. Dyer had said, as to the filling up of the estuary, that Mr. Whitty, in giving his evidence on the trial, deposed to the fact that, at Frodsham, where the tide had formerly covered, there were now cabbages standing.

Mr. Goodman said, it had already been stated, that the incoming tide was of considerably greater power than the outgoing tide, together with the inland stream, tended to keep the river at a given depth ; and it had also been stated, that the balance of the two powers was so equal, that, for a long time, the river had been kept at one depth ; and that, if there were no stream at all from the uplands, the river would speedily silt up ; and he believed it was the opinion of every one present that, if there were merely a water stream, it would not silt up. He would suggest, that, if there were a given force of tide coming in, and a given force going out, if they diminished the quantity coming in, they would give the preponderance to that going out. It was obvious, that, by diminishing the estuary, they diminished the current inwards ; and, the preponderance being thus given to the current outward, the scouring effect at the bar of the port would be increased.

Mr. Bateman said, Mr. Hopkins had asked a question with respect to dredging. Of course it was perfectly easy to dredge, and he had no doubt a channel might be made to Runcorn by dredging. But dredging was an expensive process, and a great deal might be done by a proper combination of the natural resources of the river towards making it excavate its own bed. Engineers had varied much in opinion as to the best means of keeping open navigable rivers. Generally, however, but not universally, they had come to this conclusion, and carried on their operations on this footing, to embank the freshes and the river waters, and, by narrowing their channels, to make them excavate their beds, but to keep as large as possible the capacity for tidal waters in the bay. He must also explain as to the means he had provided for damming the water. He made his calculations on a 15 feet tide ; but it must be recollected, that, after the water had attained an elevation of ten feet, not another drop of water could come in, because the tide had turned at Liverpool, which Mr. Buck had so well explained. He would therefore take it at ten feet. He was quite aware, that the same level would not be attained with a diminished waterway above the embankment that it would now. The sectional area of the river now, when the water was ten feet high, would be 9,800 square feet ; the locks and discharge sluices would be 4,120 square feet, not one-half ; but the velocity would be so increased through the contracted openings, that nearly the same amount of water would pass through. Mr. Palmer alluded to the depositions being brought in from the sea, and said, he was

convinced that a quantity of the matter in the estuary had been brought in from the sea, and not from the uplands. Now, he had no doubt, that Mr. Palmer had considered that point attentively ; but he thought he had overlooked what became of the matter from the uplands. They knew well that every river abraded its banks. The whole of Holland was an accumulation of mudbanks. The Ganges sent its debris 300 miles out to sea. Egypt was the gift of the Nile, nearly the whole of that country being deposited from the Nile; and, in the Adriatic, there was a vast accumulation of land. He then read several extracts from Lyell's Principles of Geology, in reference to this subject.

Mr. J. C. Dyer would mention a case of the formation of a delta by the river Somme, in France, which was, in many respects, analogous to our estuary. The outlet now was very much like that proposed by Mr. Palmer for the Mersey. A very large ship canal had been formed (such a one as we were to have from Warrington). At St. Baliere, where the open sands commenced, there was a bar. There the scouring power by the fresh water was very feeble indeed ; and the consequence was, that the navigable channel was ever shifting, and sometimes the mouth of the river was so blocked up that there was no getting in or out, without waiting for some favourable variation in the wind and tide. The government had adopted, in conjunction with this canal, the plan of building a long pier on one side, where the shore was rather precipitious, and forming sand banks by driving piles and letting the sand bank form itself, keeping a channel perhaps 100 yards wide. This was being carried on every year ; and, as the work advanced, the outlets to the sea were more and more open, and the navigation much more improved, and less and less liable to its former obstructions. He did not doubt, that the quantity of sand brought down by our small land-streams was inconsiderable, compared to the amount of debris every day brought in by the tide.

Mr. Gough, in corroboration of the opinion expressed as to the silting up of the estuary, said, his mother, who had been born and lived at Frodsham, had pointed out to him four fields which the tide formerly covered.

Mr. Fairbairn said, as they were all agreed as to the practicability of making the river navigable from Liverpool to Manchester, the only question which remained was, whether they should do it or not. He was quite satisfied with regard to the advantages that would result to Manchester from bringing vessels of from 400 to 500 tons burthen up to Hulme ; but he had great doubts whether that would ever be accomplished in our time. However, they should investigate the question, and give it the best consideration in their power; He would give them a description of two iron steamers which had lately been built. They were each 145 feet long, two

feet six inches beam, and drew six feet. They each carried 121
passengers, and 100 tons of merchandise, and had both sailed for
Sydney. Now, he apprehended that vessels of this description
might come under Barton Bridge, and carry on a lucrative trade
with all parts of Ireland and the British Islands; and provided
they used the screw propeller, it would lessen much the necessity
of widening the lock.

Mr. Heelis asked what effect the steam vessels would have upon
the banks.

Mr. Fairbairn said, he could answer that question with regard to
the Clyde. He believed, if the trustees at Glasgow had to com-
mence again, they would make it much wider than it is now. He
had no fear at all with regard to blocking up the mouth of the river,
as they could at any time keep it open by dredging, which was not
expensive, and the very agitation of steam vessels would have a
tendency to keep it open. The only difficulty would be the wash-
ing of the banks, which must be sheeted with stone.

Mr. Hopkins said, he did not know whether the interest in the
question was exhausted, but it seemed to him important to settle
one point,—whether the influx or reflux of the tide did the greatest
good or harm; and he would suggest that an evening should be set
apart for the discussion of that question only.

Mr. Buck said, he had not intended to speak on the subject this
evening, because he had given his opinion already as to the neces-
sity of a great power of back water for keeping open the port of
Liverpool; but he had been much gratified with what he had heard
that evening, because most of what had been stated accorded with
his views. It had been stated, that in the neighbourhood of the
upper part of the estuary, where it is now dry land, there was for-
merly water. He had no doubt of it, and it perfectly accorded
with what he said the other evening. It was evident that this estu-
ary received all that it could receive of tidal water; but he meant
to say that the estuary is *never full;* for when it was full at Run-
corn it had fallen at Liverpool, and at the same time it was proba-
bly half ebb at the Victoria Channel, and it was the great body
which flowed out of the estuary at that period of the tide which
kept open the port of Liverpool. The back water which flowed
out during the latter half the tide, never reached the bar at all that
tide. The operation of the scour went on by steps; and the plan
proposed by Mr. Bateman, of constructing a dam at Runcorn Gap,
would produce the same effect upon the upper part of the estuary,
which the latter does upon the bar. If Mr Bateman's plan were
carried out, the sand in the upper part of the estuary would be
driven within the power of the efflux and would be carried out to
sea by the next tide, and there would be no danger that the port of
Liverpool would be injured by such an operation.

Mr. J. C. Dyer then moved, that the discussion be adjourned,
saying, it would be hardly creditable if, after all, they did not come

to a decision.—Mr. Luck asked how they were to decide,—by a
show of hands?—(Laughter.)—The Chairman said, it was not in-
tended that any such decision should be come to. He fancied they
were there for the purpose of conversing together. They should
express their opinions, and then leave the subject. The motion
that the question be adjourned to that day week was then put, and
unanimously passed.

At the conclusion of the meeting, Mr. T. O. Lingard read an
article from the *Liverpool Mail*, on this subject, characterizing any
meddling with the river to be an act of felony. The reading of the
article caused many a hearty laugh.

ADJOURNED CONVERSAZIONE, FEB. 25, 1841.

Mr. George Feel in the Chair.

The Chairman in opening the proceedings, said, the discus-
cussion was adjourned at the suggestion of Mr. Hopkins, for
the purpose of obtaining the opinions of scientific men, as to
whether the influx or reflux of the tide was the greater. He
then called on Mr. Hopkins.

Mr. Thomas Hopkins said, he was certainly surprised at
being called upon, as he suggested the adjournment with a
desire rather to hear others, than to speak himself. Before he
addressed himself particularly to the subject, he would state, in
answer to the assertion at the end of the extract read last eve-
ning, by Mr. Lingard, to the effect that they were attempting
to make use of property which did not belong to them, that he
believed the fact was, that at the lower part of the Mersey, the
tideway belonged to the crown, and was under the care of the
Admiralty. He recollected being in London a year or two back,
when the Liverpool people were wanting to get a more com-
plete control over the river. They saw Mr. Poulett Thomson,
who told them it would be useless for them to take any further
steps in the matter, as the Admiralty objected to what the Liver-
pool people proposed. He therefore presumed, that it would
be with the Admiralty to support or reject the proposed improve-
ment. In what he should have to say, he should avoid all
allusion to the improvement from Manchester to Warrington,
as he conceived that length to be in the department of practical
engineers ; but, when it came to the tideway, it appeared to him
that the question was a more open one ; and he would, there-
fore, say a few words upon it. The main point seemed to be,
whether contracting the space for the tidal water above Liver-
pool would injure the port or not. If there were any doubt

upon that point, he thought parliament would not allow the alteration to be made. No risk would be run, and it therefore became necessary to prove that the contemplated improvement would not injure the port of Liverpool, and particularly the outer entrance. Now, it was proposed by the different gentlemen, engineers, and others, to make the first lock, which might be called the tidal lock, at Cuerdley Marsh, as Mr. Palmer said; and about Runcorn, as Mr. Bateman said; while Mr. Buck talked of making a wall at each side, so as to take the water as it came in. And the situation of the tidal lock would affect the question, as to whether the tidal water had a beneficial scouring power or not. If the tidal lock were made at Cuerdley Marsh, he presumed it would prevent the tide flowing further up, and would therefore throw an impediment to the influx of tidal water, to a certain extent. Mr. Palmer, in addition, proposed to contract the channel, by making rubble banks; and he spoke of gaining from the river, land which was now overflowed. There would, therefore, according to this plan, be less space for the tidal water to flow in, even below Cuerdley Marsh. Mr. Bateman's plan was to have a lock somewhere about Runcorn, and admit the tidal water, so as to fill up the pool above his lock; and, as that would clearly prevent the influx of a portion of the tidal water, it was open to the same objection as Mr. Palmer's plan; but Mr. Bateman proposed to counteract that, by letting out a portion of the water at a certain state of the tide; and no doubt, the letting it out in that mannner would have that effect. But the first effect would be to check the influx of the water. He understood Mr. Bateman to concur with Mr. Buck in the plan of erecting walls, to a certain height, that should take advantage of the highest point of the inflowing tide. Mr. Buck had stated, that the difference between the highest point of the tide at Liverpool and at Runcorn was three quarters of an hour, which it took to flow from Liverpool to Runcorn, and he proposed to force this to rise higher by contracting the space. And there was no doubt, that, by contracting the space, the momentum of the water would cause it to rise higher; but he thought it would check the flow of the water past Liverpool, because the walls he would construct would be an impediment to the water in all its stages. Therefore, all these plans seemed to him to have a bearing upon the question as to whether the influx of the tidal water had a scouring effect or not. Was this channel kept open by the mass of water that flowed in from the estuary, was the question. It was possible by scientific investigation to go into all the facts and circumstances; but one person naming one thing, and another suggesting another, would not do. It was a subject more for the closet, than for a popular assembly. A more satisfactory mode there was for those who had read as to the operations of nature in various parts, to point out what those operations were; and, if the cases were analogous so

far as the principles in operation were concerned, they might
consider whether the principles which determined others would
apply to this. The cases might not be strictly analogous; but
probably they might show what would take place in the Mersey,
by showing what took place in other parts of the world.   It
seemed to him, that they ought to inquire what took place in
seas where there was no tide, but great freshes, and in seas
where there were tides. Into the Mediterranean there were, say,
five large rivers discharged.   The Ebro was a river which dis-
charged a large quantity of water.   There they had a strong
fresh, but the mouth of the river was notoriously very shallow.
The Rhone was another river which flowed into the Mediter-
ranean; but the mouth of it was so shallow that none but
very small vessels could pass.   There was no port at the
mouth of the Rhone.   Go across the Mediterranean to the
Arno, a river which discharged as much water as the Rhone
did, but the channel there was also very shallow, and they
had been obliged to have the port at Leghorn.   The Tiber was
similarly circumstanced; it was shallow at the mouth, and the
port was at a considerable distance from it.   Then there was the
Danube in the Black Sea; but the Nile, perhaps, afforded the
most striking instance of the little effect of fresh water in scouring
out the channel.   The Nile, it was well known, was subject to
great freshes; but there even they found, in the absence of tidal
water, that the mouth of the river was so shallow that no
sea-going vessel of any considerable draft could get over it.   In
this case, also, the port was at a considerable distance, namely,
at Alexandria.   Now, in all these cases, they had a body of fresh
water going down; and, according to Mr. Palmer's theory,
they ought to have a deep channel; but they had the reverse.
Now, contrast that with the effects produced in tidal rivers.   If
we came outside the Gulf of Gibraltar, namely, to Lisbon, the
river Tagus did not discharge as much water as the other rivers
he had named; but there was a deep channel, and men of war
having heavy stores could come opposite Lisbon.   Now, as all
the first five had fresh water, but the Tagus had but little fresh
water, but it had a tide, he thought it but fair to attribute the
depth of the channel to the flow of the tidal water.   At Oporto
they had the same circumstances, and the river there would
admit sea-going vessels.   The tide came up the river at Bour-
deaux in France, 100 miles from the sea, and they had a deep
channel; and if they looked at the Seine, they had the same
thing.   In these deep channels they had deep channels excava-
ted, very deep channels compared to those in the Mediterranean;
and in all the other cases he knew of, there were similar results.
These facts, he thought, justified him in coming to the conclu-
sion, that the flowing in and out of tidal water made deep
channels.   In England there was the Severn, at Bristol; the
Thames, at London; and the Humber, at Hull: to all of these

places sea vessels could go ; these few tidal rivers, in our own
country, showing similar effects to those he had named, in
having a deep channel. But he believed it would be found in
all these rivers, that they flowed through land which was not
much above the level of the sea ; and, consequently, the tidal
waters flowed far inland ; and it was the reservoirs admitting
the tidal water to flow in, which, as he conceived, caused the
excavation of the deep channels at the mouths of these rivers.
Now, in the Mersey, they had not the same length of flat land
over which the river flowed, as there was in the rivers he had
named ; but they had a large estuary. He apprehended there
would not be a deep channel at the port, if this river was con-
tracted in its space, having so short a distance to move, as in
that case, a sufficiency of water would not flow in ; and it ap-
peared to him, that it was only the breadth of the estuary that
compensated for the want of length. It appeared to him, then,
that in the absence of tidal water, there was not sufficient power
exerted by the freshes to excavate the channels ; but in all the
tidal rivers (those having a long course, as well as those having
a great breadth), there were deep channels, which were, there-
fore, excavated by the tidal water. But it was not necessary
that they should make their comparisons between places so
distant from each other. It struck him that they had a com-
parison before them which would answer their purpose very
well. The Dee, he apprehended, sent down as much water, or
probably more than the Mersey and the Irwell. Whe-
ther the quantity were larger or nearly as large, the rea-
soning would apply. If fresh was capable of excavating deep
channels, why did it not excavate them there ? but at Parkgate
it was very shallow. Therefore, reasoning from analogy, if
they had nothing but fresh water, or but little tidal water, they
would have the same result at the mouth of the Mersey as at
the mouth of the Dee ; that was, they should have a very nar-
row channel. He would only make one more remark. The
water that came down the Irwell and the Mersey passed Latch-
ford. The tide came up to Woolston (a little lower down).
Now if the fresh water would exert that scouring power which
Mr. Palmer and the other gentlemen seemed to think, how hap-
pened it that it did not exert a sufficient scouring power to make
a deep channel there ? They had there the same quantity of
water that they should have at the port of Liverpool, were
it not for the tidal water : and why should they suppose it
would make a deep channel there, when it had such little
effect so high up the river ? He could see no reason to sup-
pose that it would ; and he therefore came to the conclusion,
that it was the influx and reflux of the tidal water which caused
the deep channel. It seemed to him to excavate the channel
in this way—there being a large receptacle for tidal water in
the wide part of the river, the water rose there as rapidly as it

could come in from Liverpool.— When the tide fell, this water,
which was stored up, required time to force itself through the
narrow part of the channel, which caused the level of the water
at Liverpool to be materially different to what it was out at sea.
Say at half tide there was a very considerable difference be-
tween the level of the water at the bar and what it was at
Liverpool.   Then the water flowed down an inclined plane, and
they knew that where water did so, it had a disposition to exca-
vate; and that appeared to him to constitute the scouring
power.   Now, whether there were a wide receptacle for the water,
as in the Mersey, or a long river, the effect was the same.   In
fact, nature did here what Mr. Bateman proposed to do by the
formation of his gates; and if they were without this, it ap-
peared to him that the channel would be filled up as in the
Dee and in the other rivers to which he had drawn their atten-
tion.

Mr. Lingard said he wished to call Mr. Hopkins's attention
to two points.   The mouth of the Dee had a much better open-
ing than the Mersey.   And as to the operation of the freshes,
it was proved at the trial which the company had at Lancaster,
that these land freshes were the only means of keeping the port
open.— Mr. Hopkins explained, that he did not speak of the sea-
ward channel, but of the inward channel.

Mr. Lingard said he believed the whole of the discussion had
been as to the effect at the mouth of the river ; and he had not
heard it said by any one gentleman, that the effects of deepen-
ing the channel above Liverpool would have any thing but a
beneficial tendency.   Now, having shown that the mouth of
the Dee, which was funnel shaped, was better than the mouth
of the Mersey, he was at a loss to understand why the same
thing should not have the same effect on the Mersey as on the
Dee.

Mr. Bateman said, Captain Denman proved that the Chester
bar was not better than the Liverpool bar.

Mr. J. C. Dyer said he thought it very essential in consider-
ing this matter, that the few remarks which he made the other
evening should be shown to be sound or unsound with regard
to the silting up of enclosed waters.   He contended, that our
small river was in no way analogous to those enormous rivers
in different parts of the world, which had been mentioned this
evening.   It was the magnitude of those rivers which placed
them outside the discussion.   They had formed deltas, which,
in some instances were equal to the largest English county.
What they meant by the deltas of rivers was where the *debris*
of continents was brought down and was met by the *debris* of
the ocean ; it was then carried down, and deposited where
there was comparative stillness.   Now he meant to contend,
that, if there was any delta at all in the Mersey, it was above

Warrington. The sands which were brought down were met
and deposited there. But a far more important consideration,
he believed, would depend on another circumstance. The
waves of the ocean were agitated by the stormy winds, and
were continually tearing down, and carrying to vast distances,
the materials forming the rugged shores, and exposing promon-
tories where the action was most violent. These charged the
waves continually with as much solid matter as the water could
hold in suspension, and the quantity was determined by the du-
plicate motion. Taking that into view, it was quite manifest,
that, in an enclosed bay like this, the moment the water entered
the channel, it became comparatively still; and, as it went out
and widened, and the current became less rapid, all the heavier
particles in the water would be deposited in this enclosed and
sheltered condition; and, by the return of the tide, when there
was still water there would be a still larger amount of deposit.
Hence, in a short time,—and 50 or 100 years was a very short
time, geologically speaking,—the whole of this estuary would
be silted up, and there would be barely a passage left, which
would be eternally left open, sufficient to carry out the waters
brought down by the freshes from the uplands. If, by con-
tracting the river, as proposed by Mr. Palmer, only one-fourth
of the water came in, he would maintain that they would only
have one-fourth of the mischief to overcome. He then referred
to the river Hudson, in America, the bar of which, he said, was
190 miles out at sea; and then there was another at Sandy
Hook, and one 20 miles above New York. Near Albany, they
were frequently obliged to have casks to lift them over; but
when the freshes came strong, the bar went a little further
down. He then referred to the improvements in the Somme,
in France, and in the Clyde, and observed, that he would once
for all say, that he was neither able nor disposed to controvert
the opinions of engineers upon any matter which was strictly
engineering practice; yet he held, that any man's opinion, on
geological theories or mathematical data, was just as good as an
engineer's.

Dr. Marshall read an extract from the statistical account of
Scotland, published in the *Glasgow Herald*, which stated, that
"seventy years since, the depth of the Clyde at the mouth of
the Kelvin, was, according to a survey made by the celebrated
James Watt, only three feet eight inches at high water, and one
foot six inches at low water. Twenty years afterwards, no
vessels of more than 40 tons burden could come up to Glasgow.
Twenty-two years ago, the river was navigable to the Broo-
mielaw for vessels of 170 or 180 tons, and drawing nine feet
six inches of water. Within fourteen years afterwards, vessels
drawing thirteen feet six inches could reach the city; and now,
vessels from all quarters of the globe, some of them upwards of

600 tons burthen, and drawing sixteen or seventeen feet of
water, are frequently to be seen, in triple rows, nearly the whole
length of the harbour.

Mr. Hopkins said, he intended to suggest, that, if it were
practicable, it appeared to him desirable, that, instead of having
a barrier erected at Runcorn, or Cuerdley Marsh Gate, the
river should be excavated and deepened down to Warrington.
Provided that were done, an additional space would be afforded
for the tide to flow into, and they would thus have a greater
scouring power. He apprehended, that something analagous to
this had been done in the Clyde, which had been deepened ma-
terially. He presumed it was a well known fact, that a much
larger portion of tidal water flowed in now than did before the
improvement took place. If that were the case with regard to
the Clyde, it was possible that it might be so also with regard
to the Somme. But there was a case so nearly analogous to
the Mersey, that it deserved to be mentioned, although it was
on a small scale. It was a place where there was a considerable
artificial excavation. He alluded to the small port of Boulogne,
where there was a small inland stream which made a passage
into the river; but the French from time to time, and particu-
larly Buonaparte, made artificial excavations so as to permit
tidal water to flow into basins above. What was the effect of
this? There was very little went in, but the mouth of the
river was kept perfectly clear.

Mr. J. C. Dyer said he thought his position unanswered.
With regard to Boulogne, what Bounaparte did was to build a
large lock. The harbour had been long neglected; there had
been no dredging; and the consequence was, that the whole
harbour was silted up, not with sand, but with a very fine mud;
and this he had to dig out. He would admit that the greater
the depth of the river, the better; but if it had a power of
spreading over a large surface, he contended that the tide de-
posited a great portion of what it brought in.

Mr. William Read said, as he understood Mr. Palmer, that
gentleman had no intention whatever to diminish the capacity of the
channel, though he proposed to make it contain less water than
it now contained, whilst spread over the wide estuary.

Mr. Bateman said he had made some calculations upon that point,
by which it appeared, that the quantity of water would be diminished
to one-third or one-fourth. Mr. Buck had also made similar cal-
culations, by which he supposed it would be diminished one-fifth.

Mr. Dyer wished to make one remark, which was, that nearly
the whole of what he had said was to show the practicability of
Mr. Palmer's plan, and his remarks would also apply to Mr.
Bateman's plan.

Mr. Bateman said there were some few facts which appeared

not to be comprehended—there seemed to be some misunder-
standing. It had been stated, that the maximum velocity of the
flood tide was greater than the maximum velocity of the ebb tide ;
and it had been stated to the contrary. Now there were some
statements in Captain Denham's book which went to prove certain
facts. Captain Denham was employed to investigate all the par-
ticulars and intricacies of the port of Liverpool, and to form a
chart of sandbanks ; and he had executed his task to an extent
that had scarcely ever before been carried out. He had found
channels, and made the Victoria Channel, and he had made experi-
ments as to the velocity of the flood and ebb tide. There could be
no reason whatever to suppose, that, in doing all this, Captain Den-
ham had any other view than to record the facts as he found them ;
and he believed it had been universally admitted by all who had
the opportunity of judging, that they were as he had stated them.
He said, that the greatest velocity of the flood tide, at three hour's
flow, was six miles and three quarters an hour ; and that the great-
est velocity of the ebb tide, at two hour's flow, was seven miles ; the
greatest velocity of the flood-tide being at three hours, and of ebb
at two hours, and the velocity of the ebb being greater than the
flow by a quarter of a mile. He said, that every cubic yard of
the inflowing tide contained 29 inches of matter held in suspension,
and that every cubic yard of ebb tide contained 33 inches ; and he
tested this by measuring the sand carried out, and forming accu-
mulations about the bar. There was also another misunderstanding
with regard to the material brought down from the river. He had
ascertained, that the greatest velocity of the greatest flood at War-
rington was four miles an hour. As it widened below, the velocity
decreased until it came down to Runcorn Gap, where it was $2\frac{1}{2}$
miles an hour. It gradually increased again, till at Liverpool it
was seven miles an hour. Mr. Buck had shown very clearly the
effect of Mr. Palmer's plan. The tide rises and falls in pretty
nearly the same time ; and do what we will at the estuary, it will
continue to do so. The water now runs out nearly as fast as the
tide falls, and it is quite clear, that if only one-fifth of the quantity
flowed in, as it cannot flow out faster than the tide falls, it must
flow at one-fifth of the velocity ; this would reduce the momentum
to one-twenty-fifth, and where then would be the force to keep
open these enormous channels ? The velocity would not be seven
miles an hour at Liverpool, after Mr. Palmer's plan was carried
into operation, but probably a mile and a half. The bar would be
brought nearer Liverpool, and the deep part at Liverpool would be
gradually silted up. Not to travel too far for instances, if we ex-
amine all the estuaries on the same coast, we shall find they are all
open mouthed like the Dee and Ribble, except Wyre Harbour and
the port of Liverpool.

These are the only ports with narrow necks at their junction
with the sea, and wide expanded bays above, and yet they are the
only ones that maintain their deeps, while all the others have been,
or are gradually becoming, choked up with sand so as to lose their
intercourse with the sea.

Mr. Lingard said, if Captain Denham's statement, that every cubic yard of flood tide contained 29 inches of matter in suspension, and every cubic yard of ebb tide contained 33, were correct, that must cause an excavation of 16 inches of the bed of the river every year.   Then, Mr. Palmer only proposed at present to make the improvements down to Weston Point, and therefore the whole of the estuary was left open.   He was sorry the discussion had occupied so much of their time ; but he was glad it had come to so satisfactory a conclusion, because he did not recollect that there was any difficulty, except from their contracting the river above Liverpool.   He understood Mr. Palmer had been invited to come down this evening by some gentlemen connected with the society, but that he could not do so. He, however, had had some correspondence with Mr. Palmer upon the subject of the discussion, and would read to them his views upon it.—He then read extracts from Mr. Palmer's letters.

Mr. Hopkins said, as he understood the first letter, Mr. Palmer had reasoned merely upon the river above Liverpool.   If so, he had reasoned upon matters of little consequence.   Were they to understand, that Mr. Palmer had not directed his attention to the outer portions of the river, but merely to the estuary ?   All his (Mr. Hopkins's) objections applied to the silting up of the Victoria Channel and of Formby Channel.

Mr. Lingard said, from Mr. Palmer's report and correspondence he conceived that Mr. Palmer believed, that, by excavating this channel (in the estuary), the could cause the tide to rise higher ; and, inasmuch as it would come up to Runcorn in much less time then it now does, it would also return in about the same time ; and, therefore, the effect of this power would be much greater out at the mouth than it now was.   From a number of statements which had been made, it appeared that the river now had sufficient depth to bring up steamers of 120 tons burthen.   He had no wish to promote Mr. Palmer's plan, or Mr. Bateman's plan, or Mr. Rhodes's plan, but to ascertain whether any plan was practicable, and then to see if the benefits were such to Manchester as would warrant its being carried into execution.   They could not come to any thing more satisfactory, and it would be well to have a meeting somewhere else to ascertain the benefits.

The chairman then announced the conclusion of the discussion on this subject, which he said had excited considerable interest, as was evidenced by the numerous attendance.

Mr. J. C. Dyer moved a vote of thanks to Mr. Lingard for bringing the subject forward, and said, as the discussion was at an end orally, he hoped that gentlemen who were interested would now take up the goose quill.

Mr. Hopkins, in seconding the motion, said, he hoped and trusted that great good would arise out of the exertions of the company with which Mr. Lingard was connected, and that they would have the cordial assistance of the town.

The chairman then put the motion, which passed by acclamation, and was suitably acknowledged by Mr. Lingard.—A vote of thanks was then given to the chairman, and the conversazione terminated.

Something on an Experiment of M. A. DE LA RIVE. By
P. O. C. VORLESSMAN, *of Heer.**

The electro-conducting powers of metals becomes deteriorated by an elevation of temperature, whilst, on the contrary, elevation of temperature enhances the conducting powers of liquids. Although this inference accords with M. Enschede's views of this important point, it is by no means a decisive conclusion, as appears to be the opinion of most philosophers. M. De la Rive made some experimental enquiries respecting the phenomena that would occur by the passage of an electric current from a metal to a liquid, and from the latter to the former, when both were heated. For this purpose he employed a small voltaic battery of four pairs. The poles were joined to two platinum plates, one to each, which were immersed in water in a glass vessel. A galvanometer was also in the circuit: and the deflection of the needle amounted to 12°. The strong flame of a spirit lamp was then brought to beneath the positive plate, (that at which oxygen is developed) until it appeared at a glowing heat; and had communicated to the water a sufficient degree of heat to cause it to boil. This treatment caused no change in the deflection. The negative plate was next heated in a similar manner. The deflection became increased to 30°. When the application of the lamp was discontinued the needle returned slowly to 12° deflection again. By another experiment, in which he employed a more diluted acid in the battery, the first steady deflection was 45°. Heating the positive plate produced no change in the deflection; whilst heating the negative one carried the needle on to 80°. These experiments were published in the *Bibliothèque Universelle,* for February, 1837. From the above results De la Rive concluded that, *heat has no influence on the passage of an electric current whilst going from a metal into a fluid; but that the influence of heat is very perceptible when the current is passing from a fluid to a metal.*

Concerning these experiments I have nothing to say, but the inference drawn from them, I believe, is not correct. What

* Poggendorff's Annalen der Phy. and Chemie, B xlix. S 109.

De la Rive ascribes to heat is only a consequence of the *motion* of the water whilst boiling, by which the plates are frequently brought into contact with fresh strata of that liquid. Independently of heat I can easily produce the same phenomena, by merely agitating the plates a little in the liquid; or by agitating the latter, with a glass rod, in the vicinity of either of the plates. The following are the results of some of my experiments.

I took a voltaic battery of five pairs, and charged it with pure water. Two platinum wires, terminating in a glass of, water, were united with the two poles of the battery; and a galvanometer was in the circuit. The deflection of the needle was 45°. In a short time the deflection began gradually to lessen. I now agitated the positive wire, but no change took place; but I had only the negative wire alone to touch to obtain a sensible increase of deflection, and by a continued motion of the liquid, a steady deflection of the needle could be obtained. The first deflection being 45°, it became lessened in the following manner.

After.	Standard deflection.	Deflection by shaking the negative wire.
15 min	34°	40°
30	16	38
60	4	32

(Shaking the positive wire produced no change.)

When the shaking of the wire was discontinued, the needle gradually retired to its former degree of deflection. The same thing takes place when a copper wire is used; as will appear by the following table of results from my experiments.

		Standard deflection.	Deflection by shaking the negative wire.
April 13th, at 9h.	0m	52°	——
At the end of	10	38	45°
Ditto	20	34	44
Ditto	30	32	40
At ....10h.	0	25	32
At 10	30	15	23

(By shaking the positive wire not the smallest effect was produced.)

The difference of deflection by the employment of copper wire is much less than when platinum is used, as, for well known reasons, might be expected.

The motion thus performed in the liquid, is, in principle, precisely the same as by employing an extended surface of the negative wire. In the latter case we use a third wire: and by placing this in the liquid in connexion with the positive wire no change in the deflection of the galvanometer

needle is produced : but if we place it in connexion with the nega-
tive wire the intensity of the current immediately increases. As
soon as the fluid shows no strange properties, and, consequently
no polarization of the wires takes place, none of the above named
phenomena will be observed : a motion of the liquid or of the wire
makes no change in the intensity of the current. When De la
Rive employed the platinum wire and diluted acid, or the copper
wire and a solution of copper, he ought not to have arrived at the
singular conclusion, that heat exercises that mysterious influence
over the electric condition of bodies which he describes.

The same thing takes place by employing a simple element of
zinc and copper. When I agitated the zinc plate, no change in
the current was produced : whilst by agitating the copper plate the
intensity of the current became perceptibly greater. By this mo-
tion, the particles of zinc which had been transported to the copper
plate, became removed ; and instead of a partially zinc-covered cop-
per, I had one with a clean metallic surface.

If, instead of a diluted acid, the zinc and copper element be
immersed in a concentrated solution of sulphate of potassium, the
difference in the intensity of the current is reversed ; and we must
agitate the zinc plate to increase that intensity. This circumstance,
explains the excellency of Wollaston's battery, in which the zinc
is surrounded with a double copper surface. By the modern battery
of Daniell, we may probably ascertain whether or not this
advantage be due to the masses: or whether the surface of the
copper ought to be greater than the zinc, in order to obtain a
maximum of action. At present, I am not prepared to ascertain
the justness of this idea.

Galvanic Experiments. By Mr. William Sturgeon.

The above described experiments of De la Rive, and of
Vorlessman, appear to be of the same class as some of those
which I published in the year 1830, in a pamphlet entitled,
" Recent Experimental Researches in Electro-Magnetism and
Galvanism." My experiments were made with a single pair of
plates of the same kind of metal, placed in an aqueous solution
of some of the acids ; and those which were employed in the
experiments about to be described were two flat pieces of iron,
which formed the voltaic pair. A galvanometer in the circuit
indicated the direction of the current. The following extract
commences at page 46 of the pamphlet in which the experi-
ments were originally published.

Iron and a Solution of Nitrous Acid.

When both pieces are placed in the same acid solution, the one a minute or two before the other, the latter, if bright, operates as copper in the S. B* very powerfully indeed ; and this singular phenomenon is exhibited to as great an advantage with these materials as with any that I have noticed : but these electrical relations very soon cease, and the pieces almost immediately display electricity in the opposite way, precisely the same as when nitric acid is employed.

There is another phenomenon exhibited in these experiments which I believe has never before been noticed, but which, by the regularity of its display, must necessarily involve some *theoretical principle,* and consequently becomes as interesting to the philosopher as any other ; I have observed it more or less in several other experiments, but as it is very decidedly exhibited with these materials, I will describe it in this place.

When two pieces of polished iron have been for a few minutes immersed in a weak solution of nitrous acid, and in connexion with the galvanometer ; if one piece be taken out, and very soon returned to its place, the needle will be deflected to a considerable angle, amounting in some instances to 90°, indicating the piece last plunged in, to be operating as copper in the S. B., and the needle will not return so quickly as if that piece had been bright before immersion, but will frequently retain that character for some time ; it takes place with either piece ; it is a matter of no consequence which is first plunged into the acid solution, the last will always display the phenomenon I have mentioned. I have obtained the same result for twenty successive times, by first taking out one piece, then the other, leaving them in the solution about half a minute between each time.

Iron and Muriatic Acid.—When two equal pieces of iron are immersed in a solution of muriatic acid, the piece last plunged in will display its electricity in the character of copper in the S. B. If now, the other piece be taken out, it will also operate in the same capacity, in precisely the same manner, as when iron and nitrous acid are employed ; and this species of action takes place when the pieces are immersed, the one in the acid solution, and the other in water, so that the first effect indicated by the needle, will depend on the order of immersion ; but if they be left unmolested for a minute or two, the piece in the acid solution will operate as copper, whilst that which is surrounded by water will operate as zinc in the S. B. ; and these electrical relations of the two pieces will be uniformly displayed while undisturbed in their respective chambers, but if either piece be in the *least moved* in the fluid, that piece will immediately operate as copper in the S. B.

* S. B. means a standard battery of copper and zinc.

This singular and curious phenomenon, which I believe has not before been noticed, I shall endeavour to describe with some degree of minuteness ; and likewise the process by which it appears to be the most decidedly exhibited.

Let two flat pieces of good iron, having each about two square inches of surface, exactly alike, and well polished, be connected with the galvanometer, and placed in a vessel containing muriatic acid diluted with two or three times its quantity of water. The needle will vibrate a little, but will soon come to rest ; but, as it is next to impossible to select two pieces of iron so nearly alike in their electrical characters as not to display some galvanic effect, it is likely that the needle will not repose in the magnetic meridian, but will make some small angle therewith. Let that piece only, which the needle indicates to be operating as zinc in the S. B. be gently moved in the interposed fluid ; the needle will immediately be deflected the contrary way, showing that the electrical relations of the two pieces of metal become changed by this process. When the needle has again come to rest, move the other piece, letting the first moved piece remain unmolested ; the needle will again change its direction, and will indicate the last moved piece to be operating as copper in the S. B.

If, whilst the connexions are complete, one of the pieces be moved rapidly to and fro in the acid solution, whilst the other remains at rest, the needle will be deflected to an angle of 40° or 50°, and may be kept steady at about 20° by continuing the motion, still indicating the moving piece to be operating as copper in the S. B. But the moment the motion has ceased, the needle returns to the meridian, and very frequently takes a position on the other side.

I have tried iron with solutions of other acids, but cannot discover that decided effect as with the muriatic. I have also tried if the same phenomenon could be exhibited by employing other metals, such as copper, zinc, brass, &c., in different acid solutions, but I have failed to obtain any thing like that precision of results as are afforded by iron and diluted muriatic acid. In some cases indeed, the same process appears to operate in the contrary way ; and particularly with tin in a solution of nitro-muriatic acid.

W. STURGEON

Royal Victoria Gallery of Practical Science,
Manchester.

Electricity of Steam.

The experiments which have been made in this town on the electricity of steam issuing from boilers, have, I believe, been

rather numerous, though none of them, that I have heard of, have been attended with any brilliant results. Through the kindness of Mr. Joseph Radford, and Mr. Richard Roberts, I have had opportunities of making a few experiments on the steam both from fixed and locomotive engine boilers : but in general I have met with complete failures. The first experiments were on the steam from a fixed boiler in Messrs. Radfords and Co.'s foundry. Expecting to meet with powerful electric action, I provided myself with a Leyden jar, discharging rod, electrical stool, &c., but for fear of being disappointed in the employment of those pieces of apparatus, I also provided a delicate gold leaf electrometer.

The steam was up to 36lb. on the square inch, and Mr. Radford had got a hole bored in the side of the safety valve chamber, for the purpose of obtaining a horizontal jet of steam to operate on. Mr. Radford's foreman was placed on the insulating stool, with a frying pan in his right hand and one end of a copper wire in his left ; the other end of the wire being fixed to a medical discharger with a glass handle. Thus prepared, the plug was withdrawn from the hole in the safety valve chamber, and the frying pan was held in the jet of steam.

I held the discharger by its glass handle, and presented its brass ball to the surface of the boiler, but no spark made its appearance. I presented the ball to the apex of several of the conical rivet heads, but with no better success.

In another trial I mounted the stool myself, holding the frying pan in the steam with one hand, and with the other I presented a brass ball to the rivets of the boiler ; but all to no purpose. Mr. Radford afterwards tried in the same manner, and with equally negative results.

A copper wire was next well insulated, having one end in connexion with the gold leaf electrometer in a dry warm room, and the other end held in the jet of steam ; but not the slightest indication of electric action was discoverable.

In a subsequent set of experiments at the same boiler, with the steam at 50lb. on the square inch, we were similarly disappointed for a long time ; till at length, Mr. Ransome, who was one of the party, held the cap of the electrometer in the fiercest part of the jet of steam ; the gold leaves diverged and struck the sides of the glass. This indication of electric action urged us on to further trials for obtaining the spark, which eventually was discovered between an insulated brass ball and the apex of a rivet head of the boiler. In this experiment a coil of copper wire was held in the steam by Mr. Ransome, whilst standing on the insulating stool ; and the ball of the discharger, which was attached to one end of that wire, I presented to the rivet head. The spark was first seen by one of

the workmen, and afterwards by the whole party ; but I think that it never passed through a plate of air of more than 1-20th of an inch in thickness.   It was just perceptible when looking attentively between the two métals.   I ought to mention, that in this latter set of experiments, the steam had to traverse a glass tube, which Mr. Radford had had attached to the hole in the boiler for better insulation.   The outer end of the tube was covered with a brass ferrule.

In a set of experiments which I made on the steam of a new locomotive, in the works of Messrs. Sharp and Roberts, I could get no electric indications whatever.   In one part of these experiments the engine was insulated on blocks of baked wood, but all to no purpose.   The steam was 50lb. on the square inch. I have to acknowledge the kindness of Mr. Fothergill, foreman of the works, who rendered me every assistance in his power whilst making these experiments on the locomotive's steam.

●   W. STURGEON.

Royal Victoria Gallery of Practical Science,
       Manchester.   ●

ELECTRO-CHEMISTRY.   *On the Chemical Force of electric currents, considered in reference to Chemical Affinities ; and of the measure of the latter.*   By M. BECQUEREL.*
       ●

It is now some time since I first entered on a series of researches with a view of determining with exactitude the relations which exist in the forces which unite together those heterogeneous atoms which form chemical compounds. From a want of precise means, all my earliest attempts were entirely fruitless.   But having since resumed the solution of this question, I have been fortunate enough to discover a method of experimenting which at present permits of better chances of success.

In a binary compound, the force which unites two different atoms to each other, varies in intensity accordingly with the temperature, pressure, and other causes : and although the nature of that force is not exactly known, we have every reason to suppose that it is the electric.   Moreover, whatever it may be, it is essential to know its intensity, or the degree of energy of its action in those determined circumstances where we can

compare it with another taken as unity, if we wish to establish
a relation of the forces which regulate the combination of one
body with two others : that is to say, between the affinities, by
virtue of which, those compounds which result, exist.

Suppose that, with exceedingly delicate pincers, we could
take hold of each of the atoms of a combination, and draw them
in the opposite direction to those of their reciprocal attractions ;
it is obvious that a force thus employed, which would just
counterbalance, or just vanquish, that force of attraction, would
be a precise measure of its intensity. For want of apparatus
of this kind, which it is impossible to realise ; we have in elec-
tric currents, properly employed, a power capable of exercising
the same functions. The principal condition is, to find a means
of dividing a current which traverses a solution of two com-
pounds into two parts in such a manner that each of them may
separate an equivalent of one of the elements of each compound.

Several principles are necessary to arrive at this determination.
The first was discovered by Dr. Faraday, and consists in
this ;—the equivalents of bodies are united, or rather associated
with one and the same quantity of electricity : to that the same
current which traverses two metallic solutions, effects their de-
composition in such a manner that equivalents of each metal
are deposited on the terminal metallic plates. Such is the case
when the two solutions are placed in two separate vessels : but
when they are mixed together the effects produced are of a
compound nature which permit of comparing the affinity of one
and the same acid for two different bases. The great number
of experiments that I have made on this subject, prevent me
from giving them in detail, at this time : I will therefore only
allude to the principal consequences to which they lead.

By submitting to the action of the same current, a mixture
of the solutions of nitrate of copper and nitrate of lead ; of
nitrate of copper and nitrate of silver : of nitrate of silver and
nitrate of lead ; in equal atomic proportions : we find that the
decomposition is effected in definite proportions ; and that in
the mixture of the solution of nitrate of silver and nitrate of
lead, as well as in that of the solutions of nitrate of copper and
nitrate of silver, the nitrate of silver is, alone, decomposed ;
whilst in the mixture of nitrate of lead and nitrate of copper,
the latter, alone, suffers decomposition. It is proved, therefore,
that the current prefers exercising its decomposing influence, on
that nitrate in which the affinities of the oxygen, and of nitric
acid, for the metal, are least.

If we augment, by successive atomic proportions, the nitrate
of copper, in a mixture of nitrate of silver and nitrate of copper,
we arrive at a limit at which the precipitated silver ceases to be
crystalline : it becomes by degrees flocky and tuberculous. At
this time it takes the form of a mushroom of great extent, the

particles of which are in such a state of division that the weighing of them is attended with great difficulty. It seems to result, from this moleculare state of the silver, that the mass of nitrate of copper in proportion as it is augmented in the solution, exercises an attractive action on the molecules of the silver at the instant they are deposited on the negative plate, and in such a manner as to prevent them from aggregating into a mass, or at least, to prevent the force of cohesion to be exercised.

Very rigorous experiments have shown that by operating on a decigramme of nitrate of silver, and somewhat more than 60 equivalents of nitrate of copper, the copper begins to appear in the precipitate. By continuing to augment the equivalents of nitrate of copper as far as 67, the metallic precipitate is then composed of one equivalent of silver and one equivalent of copper. This result could only take place, according to the hypothesis of Dr. Faraday, when the current is divided into two perfectly equal parts; since the equivalents of bodies being associated with equal quantities of electricity require two currents of equal intensity to accomplish their separation. We may thence infer that, the force which unites the oxygen and the nitric acid to the silver, in the nitrate of that metal, when there is in the solution one atomic part of this compound equal to 0·18·, is the same as that which unites the oxygen and the nitric acid when there is in that solution 67 atomic parts of nitrate of copper.

The experiments were made in such a manner that no secondary compounds were produced capable of changing the atomic proportions of the dissolved salts. The influence of masses on the decomposing action of the voltaic current being thus made evident, it became necessary to ascertain if the relation of the masses, in order to arrive at the division of the current into two equal parts, ought not to vary when we augment the absolute quantity of atomic parts of nitrate of silver. That is to say, by adding, successively from 0·18·, to 0.58·, and to 1·08·. The results of our experiments have shown, as ought to have been expected, that the reaction of the masses was no longer the same.

By diluting the solutions by water, the influence of the masses diminished, as might be expected.

I regret that I am unable, in this place, to give the numerical results obtained as well as the discussion accompanying them, which are given in my memoir, in order to afford better evidence of the influence of masses on the electro-chemical action of the current, so as to force it to separate, at the same time, two equivalents of two different bases. When the quantity of nitrate of copper is considerable in the solution, not only are the above described effects observed; but there may also be seen in the

solution, when it is illuminated by the direct light of the sun, a considerable quantity of small metallic particles in motion, which seem to indicate, in some measure, the mode in which the particles are transported by the voltaic action.

When the separation of two equivalents is accomplished, the force which unites the oxygen and nitric acid to an equivalent of silver, and that which unites the same two bodies to an equivalent of copper, being overcome by the same current, must be considered as equal ; because two equal forces acting simultaneously, for the same period of time, and producing similar effects, are supposed to overcome equal resistances. Hence, we must necessarily infer that those affinities were equal to each other.

From the influence of masses in these actual results, we are enabled to draw some conclusions respecting their general influence in all similar circumstance. In a mixture of two salts, of two metallic nitrates, for instance ; if we augment the number of equivalents of that whose parts are united by virtue of the strongest affinities, we enfeeble the action of the current on the other nitrate ; so that at a certain period the action of this current is sufficient to vanquish the affinities which unite the oxygen and the nitrate acid to an equivalent of each of the two metals. Why is a similar effect produced by an increase of the masses? We cannot conceive that by admitting that in proportion as nitrate of copper is added to the solution, the particles of that nitrate approach one another, and thus increase the action as much as that which the current exercises on them. Now, as the action of the current is definite, it follows, that that which it exercises on the particles of the other nitrate should diminish in the same ratio ; or at least, exercised on a smaller quantity of it.

From the preceding facts, it appears that the forces which unite oxygen and nitric acid to an equivalent of silver and of an equivalent of copper are in some proportion to the masses necessary to accomplish a separation of one equivalent of each metal. But is it in the simple ratio of the masses, or as that of their squares, or as some other function of those masses ? Of this we are perfectly ignorant ; and nothing remains to be done beyond that of determining the general law of the atomic proportions necessary to accomplish the division of the current into two equal parts, when one of the elements is uncertain. This determination, which can be arrived at only by many experiments, would assist in discovering the general law of the affinities of nitric, or of any other acid, for all oxides. It would then be seen if the numbers obtained by M. Ed. Becquerel, by means of another process, of the affinities of certain bodies, are the same as those which I may ultimately arrive at.

I may here repeat, that the facts detailed in my memoir, show that two metals may be separated from their solution in the same acid, by a very simple process. Whenever the masses in a given volume of the solution does not tend to accomplish their separation immediately, this may be effected by dilution with water to the required extent.

On Electric Atmospheres. By GIAMBATISTA BECCARIA.

442. I call *electric well* (Pl. V. fig. 1.) the vessel, or can A, made of tin, fifteen Italian inches high, and six and an half wide ; and, to prevent its losing its electricity, its edge has been rounded by the means of an iron ring fixed to it. I insulate this can, or *electric well*, upon a small table T, raised upon a support of glass V, and I commonly electrify it by touching it with the hook (or with the coated bottom) of a charged bottle, which I hold by the means of an insulated handle (Pl. V. fig. 2.) : I need not say, that when I touch the *well* with the hook of the charged bottle, I observe to touch with my finger, when I mean to charge the well, with the outside of the bottle. In this well I distinguish two parts ; the first is the *lower* part of it, that is, that part of its cavity which reaches from the bottom, to two-third parts of the total altitude of it ; the *upper* part is that which, from thence, extends up to the edge of the well.

443. I call *scrutator* an electroscope annexed to a long stick of sealing-wax (Pl. V. fig. 1.), the threads of it are exceedingly fine, and only an inch and a half long, and to them are fastened two bits of paper in order to render their motions within the cavity of the well sufficiently conspicuous ; and that I may perceive them the better, I cover the bottom of the well with a round plate of tin blackened over.

444. I. A man suspends the scrutator in the middle of the lower cavity of the well, in such a manner that it does not touch either the bottom or the sides ; I then touch the well, at one time with the hook, and at another with the outside of the bottle, and I find that the threads remain unmoved. II. The person who holds in his hand the threads of the scrutator now touches the bottom, and then the sides of the lower cavity, and the threads still remain unmoved. III. The same person suspends again the scrutator in the middle of the lower cavity, without touching either the sides or the bottom of the well ; I then insert into the well a small rod of brass, with a ball at its

end, and present it to the threads of the scrutator, taking care
not to touch either the edge or the sides of the well, and the
threads fly to the ball. I destroy the electricity, and the person
who holds the scrutator draws it out of the well; then the
threads manifest an electricity contrary to that which I had
communicated to the well; that is, if I touched the well with
the hook, the scrutator runs to the hook, and flies from the
outside of the bottle ; if I touched with the outside of the bot-
tle, the scrutator flies to the outside of the bottle, and runs from
the hook. IV. I annex a short and very fine thread laterally
to the lower cavity of the well ; the scrutator is again sus-
pended with it, and I, with the bottle (I always understand it
to be strongly charged) electrify the well. Seeing that both
the threads of the scrutator, and the annexed thread remain
unmoved, I put the brass rod into the well, and present it to
the threads of the scrutator, when the latter instantly run to
the rod, and the annexed thread diverges a little from the well ;
if both the annexed thread and those of the scrutator happen to
be near each other, they immediately join.

445. I shall relate another experiment on the same subject,
the consequences of which are still more obvious than those of
that just described. I fasten to three silk threads B, a cylinder
of tin (Pl. V. fig. 3.) two inches high, and also two inches
wide, the edges of its bases are rounded, that it may not attract
electricity, and I call such cylinder the electric bucket. I. A
man suspends this bucket within the lower cavity of the well,
taking care that it does not touch either the sides or bottom of
it ; I electrify the well, the man draws the bucket out, taking
care to keep it at an equal distance from the sides ; I then pre-
sent a thread to it, and it is not moved in the least. The man
puts again the bucket into the lower cavity of the well, of which
I revive the electricity ; he draws it off again ; I then present
a thread to it, and it still keeps unmoved. III. The man again
puts the bucket into the lower cavity of the well ; I electrify
the well, and then put into it the rod C, the ball of which I
present to the bucket ; then a spark is thrown out of it, which,
with respect to its explosion and light, is much superior to what
might be expected from the capacity of the bucket. The man
again draws the bucket out, I present the scrutator to it, and
the threads are rapidly drawn, then repelled by it. When I
have touched the well with the hook of the bottle, the threads
repelled by the bucket, are also repelled by the outside of the
bottle, but drawn by the hook ; when I touched the well with
the outside of the bottle, the threads repelled by the bucket, are
also repelled by the hook, but drawn by the outside of the
bottle. IV. The man suspends within the well two buckets of
an equal size D, *d* (Pl. V. fig. 4.), the bucket *d* hanging from
the bucket D, by silk threads two inches long : both are drawn
out, and neither of them can move the threads I present to

them. V. The man again puts the buckets into the well; the lower *d* touches, as it did before, the bottom of it, which D does not : I then touch D with the rod, and a strong spark is thrown out ; the man draws out the two buckets, when both draw the threads, but D does it most strongly, and *d* but very weakly. VI. The experiment is repeated, and the buckets are drawn out; I present to D the threads of the electroscope, after they have touched the part of the bottle contrary to that with which I electrified the well, and they are repelled ; I then with my finger touch the bucket D, and these same threads which are strongly repelled by D, are weakly drawn by *d*.   VII. I again repeat the experiment : the buckets being drawn out, I suddenly destroy the electricity in D, and the threads of the scrutator, which have touched that coating of the bottle with which I have electrified the well, are repelled from *d*.

446. Dr. Franklin, in page 325 of his works, proposes his experiment of a cork ball, which, hanging by a silk thread, and lowered into a silver can till it touches the bottom of it, draws no electricity from it. I was informed of this experiment by a short but ingenious disssertation of M. de Saussure, a philosopher of Geneva, when in February 1769, I published my little book *de Atmospherâ Electricâ*, in which I fully analysed this surprising and mysterious experiment. I have since received the Work of Dr. Priestley, published in the year 1767, in which I have seen that the ingenious author had made several attempts (page 731.) to analyse the same experiment, though he had nowise succeeded ; this I attribute only to the extreme delicacy of the subject, which requires both a most favourable weather, and a most careful management throughout the whole experiment. The weather being supposed favourable, here are a few things amongst others, that must be attended to : I. The bottle with which the well is to be electrified, must, instead of a hook at the end of the rod inserted into it, have a pretty big ball fixed to it, because the hook which is commonly used, being made of a thin rod terminating in a point, may easily turn the electricity in the threads of the scrutator into a contrary one.   II, When afterwards the electricity is to be excited in the threads of the scrutator, they must at the same time that they are presented to the outside of the bottle or to the hook, (I retain the same name though a ball is now used) be rapidly touched with a finger, that they may thereby, first, contract a contrary electricity, and then fly to those bodies and impregnate themselves with their electricity.   III. Before drawing either the scrutator or the buckets out of the well, the electricity of the latter must be destroyed, lest those bodies in their passing through the mouth of it, should receive an alteration in their electrical state, from the united atmospheres of the edge. IV. But the chief reason why, when the two buckets are drawn at once, it becomes necessary to suppress the electricity of the

well, is to prevent a mixture of the two different electricities :
that is, the strong one in D, and the weak one in *d* : it is need-
less to observe, that before suppressing the electricity of the
well, the bucket *d* must be previously raised somewhat above
the bottom, else the electricity would be destroyed also.   V.
Moreover, when the two buckets have been jointly drawn out,
it must be observed, ~~that~~ as long as D will retain the electricity
it has contracted within the well, the bucket *d*, which will be
surrounded by the atmosphere of D, will repel the threads of
the scrutator when they have been repelled by D ; therefore,
the electricity of D must be previously destroyed to inquire
after the contrary, and weaker one in *d*.

447. These cautions being once carefully attended to, the ex-
periments that I have described are always followed by the same
effects, and afford an analysis of the said fine Franklinian experi-
ment, as well as complete the demonstration of the property of
electric atmospheres, which I deduced in the precedent Chapter
from the simple experiment of a thread presented either to the Chain,
or to the Machine : the above experiments, besides, open a vast
field for interesting and important discoveries.

448. All these experiments concur in making it manifest, *that
the electricity introduced into the well, endeavours, it is true, to
excite a contrary one in the threads of the scrutator, or in the
bucket, or in any other body placed within its cavity* (in which
consists the first property of electric atmospheres.)   II. *But that
the homologous electricities that seek to communicate themselves to
the above bodies from opposite parts of the cavity, reciprocally
obstruct and annihilate each other* (which is the second property),
as long as those immersed bodies have no communication either
with the ground, or other external body, by the means of which
they may be enabled to contract an electricity contrary to that in
the well.

449. Secondly, from the same experiments, a few additional
truths are moreover discovered, concerning the above properties of
electric atmospheres ; for instance, they shew *that an excess of
fire endeavours to produce a deficiency equal to it, and vice versa.*
If this were not the case, how could it happen that the electricity
of the bucket *d*, which touches the bottom of the well, comes out of
the same with an electricity so inferior to that of the bucket D,
which remains suspended within the cavity of it ? surely this must
be imputed to the aforesaid equality.   From the inner surface of
the well, no quantity of fire can be exerted greater than that which
can be driven from the small surface of the bucket D ; or, if the
well be negatively electrified, from the inner surface of the well, no
quantity of natural fire can pass to the outer surface of the same,
greater than that of the excessive fire which can flow to the incon-
siderable surface of the bucket D ; therefore, as the whole capacity
of the inner surface of the well, is to the capacity of the bucket *d*,

so the intensity of the *excited* electricity (whether positive or negative, which all lies in D) is to the intensity of the electricity in the bucket *d*, into which only a proportional part of the *exciting* electricity is diffused.

450. Fourthly, from the same experiments we may again draw this consequence, viz. that *the electricity which arises in the bucket immersed within the cavity of the well, must be as much superior to the electricity that would take place on the same, if immersed in the outer atmosphere of the well, as the number of points in the cavity that concur to actuate a contrary electricity in the bucket, is greater than the number of points that endeavour to introduce a contrary electricity into the same, when suspended in the outer atmosphere.* In fact, the bucket outwardly suspended gives a spark much weaker than that which is thrown out by it, when it is placed within the cavity of the well, &c.

451. In the third place, it follows from the same experiments, *that, both the quantity of electricity that will be exerted from the cavity of the well and the atmosphere that will be excited by it, as well as the contrary electricity that will rise in that part of a body which is only partly immersed in the well, will all of them be in proportion to the capacity of that part of the same body which is not immersed.* With three silk strings joined together in B, (fig. 4.) I suspended within the well A, the cylinder C, the edge of which is rounded at its basis, and I keep it at an equal distance, that is two inches, both from the sides and the bottom. I electrify the well, and that part of the cylinder C, which is not immersed, repels the same electroscope that is repelled by the well; it possesses, therefore, the same kind of electricity. This may be easily explained : the electricity of the well, by raising an electricity contrary to its own in the immersed part of the cylinder, necessarily introduces a contrary one (consequently one homologous to its own) into that part which is not immersed; or, in other words, the natural fire driven from the former part of the cylinder goes into the latter; or, if the well be negatively electrified, the natural fire runs from the part which is not immersed, and accumulates itself on that part which is immersed. Therefore, according as the capacity of that part of the cylinder C which remains out of the well in the open air (it must be observed, that the air near the mouth of the well is somewhat affected by the atmosphere, both of the edges, and of the upper part of the cavity, which obliquely exerts itself) will be greater or less, both the excited and the exciting electricities, and the atmosphere through which this excitation is effected, will also be proportionably stronger or weaker.

452. From the same experiments it results also, *that electric atmospheres exert themselves and operate obliquely ;* this I have just now taken for granted, I will now try to demonstrate it. Let us for an instant suppose that electric atmospheres only operate directly ; if so, the electricity from the bottom of the well will suffer

no kind of counteraction, while the sides of it will counteract each
other; an electricity will, therefore, be able to spring directly
from the bottom of the well, and none will be thrown from the
inner edges, since every point of them has some other point di-
rectly opposite to it; but now, this is not by any means the case ,
we must, therefore, acknowledge, that the electricity that is thrown
from the bottom is counteracted by that which obliquely springs
from the sides of the well, directing itself towards that same
bottom ; and also, that a certain quantity of electricity really
exerts itself in an oblique direction, from the inward edge or
mouth of the well. All this may be very easily ascertained by
the following experiment. Let a number of short threads, about an
inch and half long, be, both outwardly and inwardly, annexed to
the sides of the well; let them be disposed in a vertical line, so as
to form as it were a single continued thread. When the well is
electrified, those threads which stand within the lower cavity of the
well, keep their former situation ; those which are adapted to the
outside of it, are all raised into lines perpendicular to the sides of
the well, though with a small deviation from each other, by which
the one are inclining to the right, the other to the left side alter-
nately : those which are annexed to the upper part of the inside of
the well, diverge more or less from it, according as they stand
nearer to the mouth ; and if any threads have been annexed to the
bottom of the well, they remain entirely unmoved.

(To be continued.)

ELEMENTARY LECTURES ON ELECTRICITY. &c.

LECTURE VII.

Although we are very far from having exhausted the electro-
scopic class of phenomena, it may be prudent, at this period of
our illustrations, to direct your attention to the structure, ma-
nagement, and uses of other pieces of apparatus of a somewhat
more formidable character, and by means of which we shall be
enabled to pass through a great number of illustrative experi-
ments, and show some of the principles of electric action to a
much greater advantage than by the employment of those deli-
cate instruments which we have hitherto operated with.

I will first describe to you the electric machine, an instrument
of general use amongst electricians whilst performing many of
their favorite experiments. There are, however, at this time,
two distinct forms of electrical machines : both of which are in
general use. One of these is called the *cylindrical* machine,
the other the *plate* machine. Some persons prefer one of these
machines and some the other : but as the cylinder machine is

more commonly used than the plate one, I will describe it, only,
in this place.
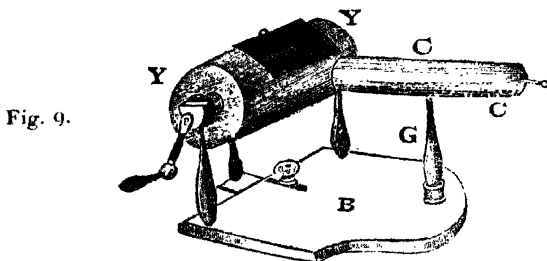
Fig. 9.

The principal operating parts of this machine consist of a
cylinder of glass and a flexible cushion; which, by rubbing
against each other, excite the electric fluid in precisely the same
manner as it is excited on the surface of a glass tube, or on
sealing-wax. Fig. 9 is a representation of a cylinder machine ;
in which B is a stout mahogany board, and, in some cases, well
varnished also. Both are useful, inasmuch as, if the copper
surface of this board were not so finished, the fibres of the
wood, would be apt to draw off some of the fluid which had
been excited ; and would, consequently, lessen the disposeable
part which was intended for experiment. This board forms the
bases of the instrument. Y, Y, is a cylinder of glass, supported
on two pillars whose lower extremities are well fastened into the
board B. The glass cylinder has an open neck at each end ;
and on each of these necks a cap of wood is firmly cemented.
Each wooden cap has a projecting cylindrical pin, formed of
the same piece of wood, in a lathe. When the caps are cemented
to their respective necks of the glass, these pins ought to be
situated in the axis of the cylinder continued ; because they form
the pivots on which the cylinder rotates when the machine is in
action. Proper pivot holes in the upper ends of the two sup-
porting pillars are provided for the reception of these pivots.

To one end of this axle is attached a handle, or winch, as
seen in the figure, for the purpose of turning the cylinder. On
the further side of the cylinder is a cushion, mounted on a glass
stem, the lower end of which is fixed in a piece of wood that
slides in a dovetailed groove in the base-board. There is also a
a set-screw attached to this piece of mechanism, all of which
will be understood by looking at the figure. • This sliding piece
is for the purpose of pressing the cushion more or less against
the surface of the cylinder, and the screw is to hold it fast at its
place when the required pressure is obtained. The cushion is
frequently called the *rubber*. It is furnished with a silken flap,
one end of which is sewed to the upper side of the rubber and

the other part lies on the upper surface of the cylinder, in the manner shown in the figure. This piece of silk is intended to prevent the excited fluid from flying off into the atmosphere, until it arrives at the extreme edge, where it is attracted and taken from the cylinder by a row of metallic points, which are attached to one end of the prime conductor C, C. The prime conductor being supported on a pillar of glass, G, is insulated, and from it the fluid can be transmitted to other pieces of apparatus for experiment. The cushion is also insulated ; and some experimenters think it necessary that the glass cylinder should be insulated : for which purpose they have its supports of glass. For my own part, I do not see the necessity of having the cylinder insulated.

There are many particulars to be attended to in order to make a machine work well. Every part of it ought to be quite free from dust and moisture ; and the glass parts to be somewhat warm. The face of the cushion which rubs against the glass cylinder must be covered with an amalgam of zinc, of about the consistency of butter, and mixed up with a little tallow from a candle. It must also be connected with the ground by means of a copper wire of about the diameter of bell-wire. When these precautions are properly attended to, the machine usually works well, especially in a warm and dry room, which is better adapted for electrical experiments with the machine than in any other situation.

The machine being now in good order, we will take away the prime conductor, and put the cylinder into motion. You will now observe an immense quantity of the electric fluid darting from the edge of the silken flap into the air ; and several bright sparks passing round the surface of the revolving cylinders. These appearances are exceedingly beautiful when the room is darkened ; and they may be produced for a long time together by continuing the motion of the machine. If you hold the ends of your fingers towards the silken flap, the nails will be curiously tipped with luminous matter, and the whole of the fluid excited by the machine will lean towards them, and scarcely any sparks will be seen travelling round the glass cylinder. In this case your fingers attract the fluid, and draw it from the surface of the cylinder, and it is carried away to the ground by yourself, the floor, and the other materials of the building ; all of which are sufficiently good conductors for this purpose.

Let us now insulate the cushion by removing from it the copper wire. You will now find that the machine produces but a very small quantity of fluid, the reason of which is, that when it has parted with that which naturally belonged to the cushion, it can yield no more, excepting some small portion which it receives from the atmosphere. But when the cushion is in metallic connexion with the ground by means of the copper wire, or

when the hand is placed on it, it gets an abundant supply from that source. Hence whatever quantity of fluid is drawn off by your fingers when presented to the edge of the silken flap, a similar portion is immediately supplied to the cushion from the ground. In many experiments, as you will see in a future lecture, we dispense with the copper wire, and have the cushion purposely insulated.

Let us now turn the points of the prime conductor towards the cylinder, taking care that they do not touch it. Now turn the machine, and you will observe that these metallic points are all tipped with small luminous stars. They are receiving the electric fluid, and conveying it to the prime conductor.

You will see that there is a metallic ball and stem attached to that end of the prime conductor which is furthest from the cylinder. This ball is screwed to the stem, and may be removed from it at pleasure, and would then expose the sharp pointed termination of the stem.

When this sharp point is uncovered, and the machine in good action, you will observe a beautiful brush of electric light proceeding from it; and if you present the back of your hand to this brush, a singular and rather pleasant sensation, something like a gentle stream of wind, is experienced. When a smooth metallic ball is presented to the point, a series of exceedingly minute sparks is observed; but when another fine metallic point is presented to it, this latter is tipped with a spot of light. Now as the point which is attached to the prime conductor is delivering the fluid and the other receiving it, we have these two distinct kinds of phenomena produced. The delivering point exhibits a brush, and the receiving one a star.

We will now screw on the ball, and thus cover the metallic point; you will now find that a series of beautiful sparks pass from the ball of the prime conductor to another ball presented to it. The snapping noise which these sparks make is occasioned by a sudden collapsion of the air which becomes displaced by the electric fluid whilst jumping from one ball to the other. It is from a snilar circumstance, only on a larger scale, that thunder is produced, by the lightning which darts from the clouds. In miniature, lightning is very beautifully imitated by the sparks which traverse the air from the prime conductor to the other ball; especially when the ball, from which the sparks proceed, is rather small. You will now perceive that the sparks which pass from the small ball in the end of the conductor, are ten or twelve inches in length, and that they travel through the air in zig-zag paths, in precisely the same manner as you may have observed lightning traversing the air. The crooked paths of the sparks, and the same remark is applicable to lightning, are occasioned by the resistance which the

electric fluid meets with in the air. When the fluid first sets
out, it drives a portion of air before it, and thus suddenly con-
densing it in the direction of its path, causes a greater resist-
ance in that direction than in any other. This being accom-
plished, the electric fluid finding an easier path becomes de-
flected from the original one : and as it performs the same ope-
ration on the air in the new path as in the old one, it is again
suddenly deflected: and thus by a rapid series of deflections
arising from the same cause, the fluid is compelled to move in
a crooked path through the air, till it reaches its destination.
There is also another cause which assists the resistance of the
compressed air in producing these deflections of the electric
fluid. The compressed air, in front of the spark, becomes
highly charged with electric fluid, which thus operates repul-
sively on the approaching spark, and tends to drive it out of its
then direction.

Approach the ball of the prime conductor with the back part
of your hand, and you will receive a series of sparks which
produce a sharp burning sensation. You may have on your
glove, and still the sparks will arrive at your hand : and by
presenting your arm to the ball, you will find that they pass
through your coat sleeve. As we proceed I shall have to show
you that the electric fluid is capable of perforating, and even
tearing to pieces, those bodies which are not good conductors.

If, instead of a ball you present a sharp pointed wire to the
prime conductor, you no longer get any spark, but you will
observe a pretty little luminous star on the point, which shows
that it is receiving the fluid in a silent and almost imperceptible
manner. Hold the point with one hand towards the prime
conductor, and with the other hand present a ball to it. You
get no sparks under these circumstances, although the ma-
chine is in good order ; but if you take away the pointed
wire, you immediately get a series of sparks. Now these are
exceedingly interesting facts, by teaching us that sharp me-
tallic points have great influence in drawing off the electric
fluid from those bodies which are charged with it. You may
place the pointed wire at the distance of several inches from
the prime conductor, and still it has the power of attracting the
fluid from that apparatus to a great extent. You may try vege-
table points, such as thorns, the points of green leaves, &c.,
and you will find that all of them have the same property as
the metallic point in drawing off the electric fluid. A bunch of
grass held at a few inches distant from the prime conductor,
robs it of all the fluid that is communicated to it by the ma-
chine. You will now see the necessity of keeping all kinds of
sharp-pointed articles entirely away from the prime conductor,
otherwise much of the fluid, intended for experiment, would be
lost by means of them.

MERSEY and IRWELL IMPROVEMENT, PROPOSED BY J. F. BATEMAN, C. E., M. Inst. C. E.

Section of Embankment.
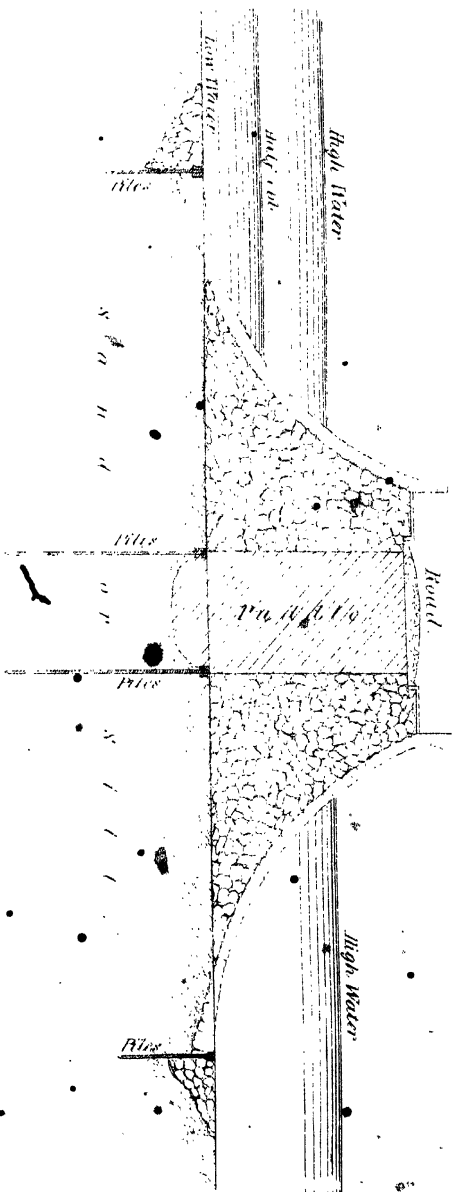
High Water

Low Water

Half ride

Piles

Vaulting

Road

High Water

Piles

Piles

Scale of Feet.

THE ANNALS

OF

ELECTRICITY, MAGNETISM,

AND CHEMISTRY;

AND

Guardian of Experimental Science.

JUNE, 1841?

On *Electric Atmospheres.* By GIAMBATISTA BECCARIA.

(Concluded from page 420.)

453. This oblique action of electric atmospheres explains
how it comes to pass, *that the atmosphere of the one surface of an
insulating plate, extends itself so far as to reach the other surface
of it ;* and from thence comes to be completely understood, one
of the most perplexing experiments that occur in the science of
electricity.   The experiment has been first made by Mr. Rich-
man : M. Epino, as I understand by Mr. Priestly's Work, has
related it, but as I have not been able as yet to procure his book,
I shall describe the above experiment in the manner that it has
constantly succeeded with me, and conformably to the analysis
I have made of it.   Let a b (Pl. ♥. fig. 6.) be a glass plate
nine inches long, and seven inches wide; let m n and o p
represent its coatings ; c d, e f, are two very thin threads, an
inch and a half long, with stripes of paper hanging from them,
and they are annexed to the middle points of both coatings ; I
then insulate the plate on a stick of sealing-wax, and make the
coating M N communicate with the chain, though in such a
situation that the electricity from the chain does not counteract
the electricity which is to rise in the thread c D.   From the
ground I touch the other coating P O, taking care to keep my
finger at such a distance o from the thread E F, that it may

not influence its divergence ; and then I observe, I. That in
proportion as the charge is farther promoted, both threads c d,
e f, gradually acquire a greater divergency. II. That if, in
this state of things, a person insulated and communicating with
the chain, presents the palm of his hand parallel to the plate,
near the thread c f, the latter falls ; and if from the ground I
likewise present my hand to the thread e f (still touching the
coating in O), the thread e falls likewise. If both the hands
that are presented to the threads, or either of them, are removed,
both the threads, or either of them, begin again to diverge.
III. If, when the charge is completed, I present my hand to
the thread e f, it falls down, though I entirely leave off touch-
ing the coating with my other hand ; but, on the contrary, if
before the charge is completed, I present my finger to the thread,
and leave off touching the coating, it flies to my finger.

454. All the difficulty arising from this experiment is re-
solved by supposing (which is the truth), that the electric
atmosphere, which (while the charge is advancing) is excited
from the coating m n, bends its course, and actually extends
itself to the air contiguous to the other surface of the plate ;
hence both the coating p o,. and the thread e f, being im-
mersed in air actuated by the electric atmosphere from the
opposite surface which is positively electrified, become electric·
by deficiency ; therefore, the thread e f diverges from the
coating. But when afterwards I oppose my hand to this thread
e f, my hand also becomes electric by deficiency ; hence the
homologous electricities of the coating p o, and of my hand,
opposing the action of each other, the thread between them falls
When I present my hand to the thread e f, and cease at the
same time to touch the coating, then the thread e f runs to my
hand ; the reason of this is, that the natural fire which is con-
tinually driving from the coating to which the thread is
annexed, runs, the other communication being now removed,
to my hand through that same thread. But when the charge
comes afterwards to be completed, as no fire is any longer
driven from p o, then, even though I cease to touch the coat-
ing p o, the thread e f, the coating itself, and my hand, all
remain electric by deficiency, though the continued action of
the electric atmosphere of the other surface m n, which, as has
been observed above, continues to bend its course, and to flow
to this surface p o.

455. I demonstrate the truth of the above explanation by
experimenting with the large plate A B C D (Pl. V. fig. 7.) I
annex two stripes of gold leaf to the margin of its inferior coat-
ing, and place them parallel, and near to each other ; I annex
two other stripes to the same coating, and place them somewhat
more advanced within the compass of it ; and two others be
sides, at a still greater distance from the margin ; this inferior

coating is then made to communicate with the ground, and the upper coating with the chain; when I observe, I. That in proportion as the charge advances, the electroscopes, or stripes, annexed to the inferior coating, gradually acquire some divergence; but with this difference between them, that those which stand nearest to the margin of the coating, manifest first their divergence; then those which follow them; and last of all, the third stripes begin to manifest what small degree of divergence they may have acquired. II. The charge being completed, the greatest divergence still continues to take place in those stripes which stand nearest to the margin; a less divergence obtains in those stripes which have been placed in the middle; and the least divergence is manifested by the remotest stripes. III. All those stripes fly from my finger, and those which diverge least, increase their divergence when I place my little finger between them. IV. All fly from the outside of a bottle inwardly charged by excess. From these facts I therefore conclude, that all the stripes are electric by deficiency, as also is my finger (though no doubt in a less degree than the above-mentioned bottle.) The reason is, both they and my finger are immersed in an atmosphere actuated from an excessive electricity, which, bending its course, flows to the inferior coating to which those stripes are annexed, and introduces a greater or less degree of electricity into them, according as they are situated nearer to, or farther from, the edge of the plate. That a body immersed in a given atmosphere acquires an electricity contrary to that by which this atmosphere is actuated, is what I think I have demonstrated before, and what every body may easily ascertain. Present a very fine thread to the chain, and when it begins to move towards it, present your finger laterally to this thread, it will fly from your finger.

455. Fifthly, We may conclude from the above experiments, that *from hollow surfaces imperfectly closed, an electricity will be manifested, which will be less in proportion as those surfaces are nearer being completely closed; and in proportion as they change either to plane, or to convex, or to more concave, surfaces, an electricity will be manifested around them in greater plenty and augmented force;* this is, because in proportion as the surface will be less hollow and closed, and will approach nearer to being plane, the reciprocal counteraction of the atmosphere actuated from different points of the surface, will lessen. But as atmospheres, as we have just now observed, act also very obliquely, and even so much so as to bend their course from the one side of a plate to the other, always preserving their property of exciting an electricity contrary to that by which they were actuated in the beginning, it hence follows that electrical atmospheres from plane surfaces, counteract each other in a less degree than those which spring from concave surfaces, though in a greater degree than those which spring from convex

surfaces ; so that an electricity that rushes from an infinitely convex surface, that is, from a very sharp point, placed at a great distance from any other surface that may be animated with an electricity homologous to its own, will be counteracted the least possible, will be thrown out with the greatest force, and will exhibit the brush or the little star. But of these important conclusions, I only mean in this place to drop a seed, or hint ; let us for the present proceed to reap other fruits from our analysis of the electric well.*

456. Of such fruits or discoveries, the following is certainly a very valuable one, viz. *that we have at length ascertained that, in* pressing *electricity, any excess of fire, any deficiency of the same, any kind of electricity, in short, is confined to the free open surface of bodies, without in the least diffusing itself into their substance.* I say in *pressing* electricity, because with respect to the *vivid* electricity of a spark, there is no doubt but the latter condensates itself for an instant within the pores of bodies, and endeavours to break the cohesion of their solid parts. With regard to common, *pressing*, electricity, if no electricity can by it be made to adhere to the wide cavity of an electric *well*, how can we imagine that the electricity will better be brought to adhere and be accumulated within the narrow cavities of the inward pores of bodies ? We cannot, it is true, suppose that within the substance of bodies there is any such medium as air, by means of which opposite atmospheres may be actuated ; but yet, do not the solid particles that constitute the partitions between the pores of such compact bodies, for instance gold, stand much closer to each other than do the solid particles of air ? Must not, therefore, the same counteraction and opposition take place between the portions of electric fire diffused within the pores of a piece of gold, as obtains, by means of the fire inherent in the substance of the air, between the homologous electricities that endeavour to accumulate themselves within the cavity of the well ?   •

457. On this occasion I shall remind the reader of the observations formerly related in page 76 of *Artificial Electricity.*   A cube made only of gilt paper both attracts and gives sparks with the same degree of force as a similar cube of solid iron Here too comes in its proper place the experiment of Dr. Franklin, who saw an electricity exerted on the surface of a metalic chain, grow more vivid in proportion as a greater portion of the said chain was gradually made to gather within a

* If the reader entertains still any doubt about the reality of the *oblique* notion of electric atmospheres, the following experimet will remove them. Let him place one of his hand open under the great conductor, with a thread hanging from the middle of it, he will find that that this thread becomes electrified by deficiency, though the *oblique* action of the atmosphere of the conductor, as well as a finger of his other hand, exposed to the direct action of this atmosphere ; and, in fact, if he presents that finger to the thread, the latter will by from it.

tumbler. I have imitated the same experiment after another manner. I adapt fine and short threads to the body of a man, to his forehead, to his back, to his legs, to his arms, &c. I bid him stand, with his arms and legs stretched as much as he can, on two insulating stools, and then give him a spark from a bottle strongly charged ; when I find that the threads immediately diverge ; though most of all, those which are least counteracted by the electricity of other adjacent parts. The man afterwards joins together both his legs on one stool, drops his arms, and at last gathers and folds his body in the same manner as tailors use to do ; then the threads placed in those parts which are become close to each other, fall down ; and those, on the contrary, which remain exposed to the free open air, increase their divergence. I repeat the experiment in a contrary manner, and give a spark to the man when he is thus gathered ; and as soon as he begins to unfold and stretch himself, the divergence of the threads in the open air lessens, and those situated between joined parts of his body, acquire a divergence according as they gradually become separated from each other.

458. If to these observations we add, that any spark, even the smallest, given to, or drawn from, a large conductor, always is seen to cause a sensible excess or deficiency in the threads, though ever so thin and comparatively small, that may be annexed to it ; that any electricity excited in one part of an extensive conductor by any kind of electric atmosphere, becomes manifested on the whole part of that conductor which is immersed in that atmosphere, and excites an equal, but contrary one, on that part of the same which is not immersed, then, we shall be convinced, that electricity does not diffuse itself into the substance of bodies ; or, in other words, that the electrical fire is not condensated or dilated within the inward substance of electrified bodies.

459. But, lastly, if electricity did really penetrate into the inward substance of electrified bodies, surely when I lower the bucket into the electrified well, and make it touch the bottom, a part of the electricity which, in the case we suppose, is diffused within the cavity of it, would also diffuse itself into the bucket, and be manifested on it when I extract it from the well ; now, when the operation is carefully made, the bucket constantly comes out of the well without the least electricity.

460. Lastly, The same experiments explain the following paradox, viz. *that the natural quantity of fire contained within insulating bodies, is incredibly greater than the quantity contained in different bodies.* This principle becomes farther confirmed, when we consider, that an hundred insulated men can scarcely supply the surface of a bottle with the necessary fire to charge it ; neither could they receive the whole quantity that escapes from the outside of the same bottle. The same thing obtains

also with regard to the discharging of the bottle; an insulated
man touches an hundred different times the coating of a charged
bottle, the other coating of which communicates with the ground,
and, though after every touch he takes care to dissipate the fire
he has received (or to recover that which he has given,) he
never has done exciting fresh sparks from the bottle; whence I
was induced to conclude that the bottle could give or receive
more fire than an hundred men could receive from, or give to,
it; and, consequently, that the natural quantity of fire in an
ordinary glass bottle exceeded that contained in an hundred
men.

461. But though we have demonstrated this truth, that the
natural quantity of electric fire remains *unaltered* within the
substance of *deferent* bodies, yet we have nowise demonstrated
this, viz., that glass actually contains more fire than deferent
bodies do. A man, for aught we know, may contain a very
great quantity of electric fire; this only is ascertained, that he
can give to the bottle no other fire than what he can himself re-
ceive from other bodies, or receive from the same no more fire
than he can transmit to other bodies. With respect to the air
around him, as the *charge* of that element can be but very
small, a man needs only receive a very small quantity of elec-
tricity from it, to exhaust it; therefore, in this case likewise, he
may actually contain a very great quantity of fire, though he
only gives to the air around him, or draws from the same, an
exceeding small quantity. In order to be able to draw a great
quantity of fire from the air, or give it to the same, a man must
communicate with an extensive system, from which he may
draw, or to which he may transmit, such fire.

462. The above is the essential difference between *insulating*
and *deferent* bodies. With respect to the former, such is the
consequence of their electrical *impenetrability,* that great altera-
tions in their electric fire may be introduced in their opposite
surfaces; and such alterations are both equal to each other on
the respective surfaces on which they take place, and contrary;
but in *deferent* bodies, no such alteration can be effected, on
account of their *penetrability*; though, for aught we know, the
density of the fire in deferent bodies may very well be equal to
the natural density of it in insulating bodies.

463. I attempted to ascertain this by an immediate experi-
ment; I insulated a wax candle a foot high, ten lines thick, on
a glass candlestick; I insulated a tube of glass above the candle,
so that the flame entered it; above the said tube I again insu-
lated, with its head downwards, an alembic of brass, the neck
of which was bent backwards and forwards several times; yet
out of several very fine threads which I annexed to that neck,
none manifested the least electricity, either when the glass tube
became deferent by its being heated till it grew red hot, or

when the same afterwards cooled, and thereby recovered its in-
sulating nature.

464. But, of all others, the *following* observation convinces
me most of the above truth. A strong spark from an electric
battery will vitrify metalic particles, and thus in an instant from
deferent render them *insulating;* yet no part of the spark is
found afterwards to have remained affixed to these bodies ; for
the deficiency that may have remained on the one side of the
battery, after a discharge of the kind we mention, is found to
be precisely equal to the remnant of excess left on the opposite
side.

Description of a New Electro Magnet. By J. P. Joule Esq.,
In a Letter to the Editor.

Broom Hill, near Manchester, April 30th, 1841.

My Dear Sir,

In that part of my researches on " Electro-Magnetic Forces"
which was published in the "Annals" for September last, I proved
that the maximum lifting power of the electro-magnet is pro-
portional to the least transverse sectional area of the magnetic
circuit ; and at the same time I pointed out the method whereby
a very great magnetic attraction could be produced between
masses of iron of inconsiderable size.

To illustrate my views I made several electro-magnets, the
relative and absolute powers of which were surprising and un-
precedented.   Two of them have been for a long time on
exhibition at the " Victoria Gallery."

Stimulated by my success, some gentlemen of Manchester
have constructed electro-magnets of a variety of forms, embody-
ing the principle of a large sectional area.   The great weight
sustained by them is another proof of the correctness of my
views.—I cannot in this place enter into any lengthened dis-
cussion of them ; I will only observe that Mr. Radford has the
merit of producing an arrangement which involves a curious
principle of magnetic action, and that Mr. Roberts has made a
very pretty electro-magnet by planing grooves into a flat plate
of iron :—his instrument consists virtually of a number of mine
placed side by side.

In the early part of January 1 devised an electro-magnet on
an entirely new principle. In order to give a clear description.
of it, I will with your permission avail myself rather extensively
of the lithography which is so excellent a feature of your work

Fig. 1, plate 6, is a bird's eye view of this magnetic with its appendages, b, b, are two rings of brass, each 12 inches in exterior diameter, two inches in breadth, and one inch in thickness; to each of these, pieces of iron are affixed by means of the bolt-headed screws, s, s, &c.; 24 of these, m, m, &c., figs. 1 and 2, are grooved and fastened to the upper ring; 24, a, a, &c., figs. 1 and 3, are plain and affixed to the lower ring.

A bundle,* (w, w, fig. 1,) consisting of sixteen copper wires i(each of which was sixteen feet long and one-twentieth of an nch thick,) covered with a double fold of thick cotton tape, was bent in a zig zag direction about the grooved pieces, m, m, &c., as seen in the figure.

Fig. 4 represents the method which I adopted for the purpose of giving the *electro-magnetic ring* a firm and equable suspension. a, a, are hoops of wrought iron, to each of which four bars of the same metal are rivetted, and welded together at the other end into a very strong hook. The hoops are bound down to the brass rings by means of copper wires. c, c, &c.

I arranged sixteen of your cast iron pairs† (so valuable where a convenient and powerful battery is desired) into a series of four. The poles of the battery were connected by means of large strips of copper, with the magnetizing wires; and a weight of 2710lbs. was suspended from the armature without separating it from the electro-magnet. Theoretically, the maximum power is 0·635 (the least sectional area) $\times$ 24 $\times$ 280 = 4267lbs.; and I have no doubt that by the use of some precautions which have occurred to me since making the above experiment, the actual power will be very considerably augmented.

The weight of the pieces of grooved iron is 7·025lbs., and that of the plain pieces 4·55lbs. The power per lb. of magnetized metal was therefore $\frac{2710}{7·025 \times 4·55} = 234$lbs.

When the apparatus is in the position which is represented by figs. 1 and 4, it is evident that the zig-zag ring of iron is magnetized by the conducting wire in precisely the same manner as the plain ring r, fig. 5, would be by the passage of electricity along the wire x, x, which is coiled upon it;—wherever such a ring is cut, the display of maximum lifting power is proportional to the least transverse sectional area of the entire magnetic circuit.

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* My plan of covering a bundle of wires with tape has been copied with success by Mr. Roberts and Mr. Radford.

† I described these in Vol. 5, p. 198. They consist of boxes one foot square, and one inch and a half in interior width. With the arrangement of them described in the text, I have succeeded in producing a continued flame of electricity one-eighth of an inch long; a striking distance, which is, I believe, altogether unprecedented when four pairs only are in series.

In the above position it is impossible to consider the instrument other than a single electro-magnet; but when the armature is turned so as to cause the plain pieces which are affixed to it to cover the grooves of the other pieces, it is converted into a compound electro-magnet, the lifting power of which I have not yet ascertained.

I remain, dear Sir, yours respectfully,

J. P. JOULE,

Wm. Sturgeon, Esq., Royal Victoria Gallery,
Manchester.

On the Anhydrous Sulphate of Ammonia, (Sulphat Ammon.)
By HEINRICH ROSE.

(From Poggendorf's Annalen der Physik and Chemie, No. 1, 1840.)

Whilst I was endeavouring to separate the excess of sulphuric acid from a solution of its combination with anhydrous sulphate of ammonia, (super sulphate of ammonia) by carbonate of barytes, I happened to obtain large crystals from the fluid separated from the sulphate and carbonate of barytes, I supposed these to be crystallized anhydrous sulphate of ammonia.

As I had obtained only a small quantity of these crystals, I determined not to analyse them; instead of which, I operated on the imperfectly crystallized mass, which was formed by evaporation over sulphuric acid; and obtained, at the same time, with those crystals. I found, in this mass, only 67·47 per cent. of sulphuric acid, instead of 70·03, which, according to calculation, are contained in anhydrous sulphate of ammonia.*

Since that time, I have separated greater quantities of the free acid from the anhydrous super-sulphate of ammonia, and have obtained a greater abundance of the above-mentioned crystals: at the same time I enquired more accurately into the operation of water on the anhydrous sulphate of ammonia, which had been prepared with particular care, and was completely neutral. I also convinced myself, that, when from a solution of the acid compound, the excess of acid is separated by means of carbonate of barytes, the remaining neutral solution contains neutral salts, as decidedly as they are contained by a solution immediately formed by anhydrous ammonia and pure sulphuric acid.

* Poggendorff's Annalen, Bd. XLVII, S. 474.

I have, also, by a new examination of the properties and com-
position of neutral anhydrous sulphate of ammonia, discovered
some facts, which will serve to complete those investigations
which I have already made known in previous essays on this
salt.*

The anhydrous sulphate of ammonia, I propose to name
sulphat ammon, and one of the two salts which I have obtained
from its aqueous solution, I shall, for the present, call *parasulphat
ammon,* and the other *deliquisent salt;* but it is only provision-
ally that I employ the terms *sulphat* and *parasulphat-ammon* to
those salts.   I shall willingly withdraw these terms, if the
sagacious views of Dr. Kane, on the nature of ammonia, become
more generally received ; according to which, ammonia is con-
sidered as an *amide* of hydrogen.   By this hypothesis, indeed,
those phenomena which the compounds of anhydrous sulphuric
acid with ammonia exhibit by reagents, can be more directly
and satisfactorily explained than by any other.   Notwithstand-
ing, however, the numerous compounds of ammonia with oxy-
acids and with water, may probably be better explained by the
theoretical views of Berzelius, according to which, these com-
pounds contain the oxide of ammonium.   This simple theory is
plausible, and extensive in its application, especially as these
salts, in point of composition, are completely analogous with
salts containing other basis.

I.—*Neutral Anhydrous Sulphate of Ammonia,* (*Sulphat-Ammon.*)

The principal properties of this compound I have already
given in a former essay, in which I mentioned its behaviour
with solutions of salts of barytes, with oxide of lead, strontia
and lime, and also with chloride of platinum.   Other reagents,
which immediately indicate the presence of ammonia in a solu-
tion of the oxide of ammonium, develope that substance but very
imperfectly in a solution of *sulphat-ammon.*   For the purpose
of ascertaining this point with the greatest degree of accuracy,
equal parts, by weight, of very pure *sulphat-ammon,* and of
sulphate of oxide of ammonium, were each dissolved in nine
times its weight of water, and both solutions tested with the
same reagent.   In a smaller proportion of water than here men-
tioned, the sulphat-ammon does not dissolve completely.

On applying a solution of sulphate of alumina to the solu-
tion of oxide of ammonium, a good crop of crystals of alum was
immediately produced ; whilst a similar application to the
solution of sulphat-ammon,for a while appeared to have no such
action ; though in time a small insignificant quantity of alum,
crystals became formed.   A concentrated solution of racemic

acid, which, for ammonia is a more delicate reagent than the tartaric acid, produced similar effects. But the precipitation, by this reagent, was much more considerable in the solution of sulphate of oxide of ammonia than in the solution of sulphat-ammon.

A solution of carbazotic acid (Kohlenstickstoffsaüre) acted in a similar manner. It immediately occasioned copious precipitation in the solution of the sulphate of oxide of ammonium; whereas in the solution of sulphat-ammon a longer time is required to obtain even a very inferior degree of precipitation. I have ascertained, moreover, that all reagents which separate ammonia from the solution of sulphate of oxide of ammonium, separate it but very sparingly from solutions of sulphat-ammon.

The sulphat-ammon appears to be a homogenous uncrystalized powder. Even under the microscope no trace of crystallization is perceptible amongst its minute particles. They are uniformly of the same kind, and perfectly homogenous.

Like other pulverable bodies, the sulphat-ammon attracts humidity from the atmospheric air, which consequently adds to its weight: but this moisture may be completely evaporated by means of a water bath without any change being produced in the properties of the salt; which, by subsequent exposure to the atmosphere again absorbs as much water as before.

I have, it is true, already made known the analysis of sulphat-ammon: but I found it necessary to repeat that analysis by a series of more instructive experiments, which I have had an opportunity of carrying on by operating on a considerable quantity of this salt in a state of great purity. But, as I have in another place more particularly stated, it is very difficult to obtain the sulphat-ammon perfectly pure, in any considerable quantities. The best proof of its purity is that it not only reddens litmus paper very feebly, but even, afterwards, turns it somewhat blue. And although these effects are exceedingly feeble, it is necessary to keep the salt in a well stopped bottle, which is well charged with ammoniacal gas. If litmus paper, which has been moistened in an aqueous solution of sulphat-ammon, be dried in the air, it reddens; which is a property in common with sulphate of oxide of ammonium, and almost all the ammoniacal salts.

One hundred parts of sulphat-ammon were treated with a solution of chloride of barium; the whole evaporated to dryness, and the dry mass brought to a glowing red heat. This was afterwards covered with a mixture of hydrochloric acid and water, which produced 203·7873 parts of sulphate of barytes.*

* The author operates upon 1,399 grm. of sulphat-ammon, and obtains 2,851 grm. of sulphate of barytes.:. 1,399 : 2,851 :: 100 : 203·7878 nearly.—TRANS.

It is only by these means that we can possibly convert the whole
of the sulphuric acid into sulphate of barytes. Besides, it is
obtained only by frequent filtering, which is a tedious process.
The sulphate of barytes thus procured indicates 70·04 per cent.
of sulphuric acid in the sulphat-ammon, which agrees as nearly
as possible with the formula $\bar{S}$ + N H³: by which we calculate
the contained sulphuric acid to be $\overline{70}$·03 per cent. This result
is also confirmed by several other analysis of sulphat-ammon.

II.—*Parasulphat-Ammon.*

I have, provisionally, given this name to a remarkable salt,
which, by concentration of an aqueous solution of sulphat-am-
mon, shoots into large well-formed crystals : and may likewise
be obtained from a combination of sulphat-ammon with anhy-
drous acid, by a method already mentioned. These are the
crystals of this composition which my brother has described at
page 476 of the 47th vol. of these Annals.—(Poggendorff's.)

These crystals are obtained by concentrating the solution by
evaporation ; but this solution, like that of the sulphate of oxide
of ammonium, becomes slightly acid by long heating ; and then
contains a small portion of sulphuric acid, by which the pro-
perties of this compound are prevented from being distinctly
ascertained. It is therefore judicious to evaporate the solution
over concentrated sulphuric acid in a vacuum. When fine well-
formed crystals are wanted, the evaporation must proceed very
slowly.

By further evaporation of the mother liquor another salt is
obtained, whose properties essentially differ from those of the
former large crystals : but it is difficult to separate the one from
the other, especially if a considerable portion of sulphat-ammon
be not operated on. This salt attracts moisture from the atmos-
phere, which is not the case with crystals of parasulphat-ammon,
which, when perfectly dry, suffers no change by exposure to
the air.

The parasulphat-ammon is somewhat more soluble than sul-
phat-ammon. Its solution is neutral to litmuspaper. It also
remains neutral for a long time, when care is taken to prevent
its evaporation and crystallization. If, however, the salt be
moistened with water, it shortly acquires the property of red-
dening litmus paper. The solution has then also other proper-
ties, and behaves to other reagents in a very different manner
to the action shown by the unmoistened salt.

The acid reaction which this salt acquires by moisture may
probably originate from the water having expelled some of the
ammonia which would volatalize. It also appears that the car-
bonic acid of the atmosphere has some influence in this busi-
ness ; for when a solution of parasulphat-ammon is slowly eva-

porated in the cold over sulphuric acid, with excess of air, it, like the mother liquor, often becomes considerably acid, which is not the case when evaporated in a vacuum.

When the crystals of this salt are obtained, no attempt must be made to cleanse them from the mother liquor by washing them with water. They are only to be dried with blotting paper.

That which partially characterises parasulphat-ammon, and distinguishes it from sulphat-ammon is, that the solution of its dry salt is not rendered turbid by the salts of barytes and of lead; even when they remain together for a long time. This property is sometimes with difficulty observable, partly because the crystals may contain a portion of the mother liquor, from which they have been taken, and therefore are impure by containing a deliquescent salt; and partly because they have got moistened by exposure to the atmosphere for some time, which also communicates an impurity, and causes their solution to redden litmus paper. In both cases the solutions of the salts immediately produce precipitations of barytes and of lead.

If to the solution of parasulphat-ammon we put hydrochloric acid and a solution of the chloride of barium, the liquid continues perfectly clear for some considerable time. In about twelve hours, however, there forms in the solution a precipitate of sulphate of barytes : but this does not occur in the absence of the hydrochloric acid.

In the property of not precipitating the solution of barytes in the cold, the parasulphat-ammon is similar to the compound which Regnault obtained by saturating one of his discovered sulphates of chloride of sulphur, $\overline{SCl^3} + 2\overline{S}\ (\overline{S}\ \underline{Cl})$ with anhydrous ammonia :[*] and which he regarded as a mixture of sal-ammoniac with a sulphamide ($\overline{S}\ \underline{N\,H}$). The solution of this mixture, according to Regnault, gives no cloudiness by adding solutions of barytic salts, even when permitted to remain long in contact. Regnault was unable, by crystallization, to separate the sulphamide from the sal-ammoniac. He remarks, moreover, that the compound which he obtained very soon attracts moisture from the atmospheric air, which, as has already been remarked, is not the case with crystals of parasulphat-ammon. Even sulphat-ammon does not melt in the air.

In this case also, the results of the analysis show, that the crystals cannot be regarded as an anhydrous sulfamide. 100 parts of this salt were dissolved in water, and mixed with a solution of chloride of barium, and boiled. By boiling for some time there appeared a precipitate of the sulphate of barytes; though much slower and less significantly than that which

occurs under similar circumstances, with a solution of sulphat-ammon. The whole was evaporated to dryness: the residue heated to an incipient redness, and treated with hydrochloric acid and water, left 204·044 parts* of sulphate of barytes. This is equivalent to 70 per cent. of sulphuric acid in the compound.

This result shows as nearly as possible that these crystals possess the same per centage composition as the anhydrous sulphate of ammonia, or sulphat-ammon. If, in an anhydrous sulfamide, $\ddot{S} \underline{N} H^2$ the sulphur were completely converted into sulphuric acid ; we should obtain 83·08 per cent. of sulphuric acid from the sulfamide employed.

One hundred parts of crystal of parsulphat-ammon, which had been prepared at another time, gave, by a similar treatment, 204·495† parts of sulphate of barytes. This result answers to 70.29 per cent. of sulphuric acid in the compound.

If we regard my prepared crystals, on account of their similarity to Regnault's compound, as a sulfamide, it must be looked upon as an hydrated sulfamide $\ddot{S} \underline{N} H^2 + H$. Since, however, the existence of hydrous amides has not yet been proved ; and in many respects appear even improbable, I have named these crystals *parasulphat-ammon,* or *parsulphammon ;* because of their similar per centage composition to that of sulphat-ammon.

In the solution of parasulphat-ammon, the ammonia is still more imperfectly separated by reagents than in the solution of sulphat-ammon. In equally concentrated solutions (one part of the salt in nine parts of water,) a very concentrated solution of tartaric acid, even after being many days in the solution of parasulphat-ammon, shows no formation of supertartrate of ammonia ; whilst in that of the sulphat-ammon, a precipitation, though not copiously, is brought on. A concentrated solution of racemic acid produces, although not immediately, but after some time, a very feeble precipitation of crystal in the solution of parasulphat-ammon : which are ever much more insignificant than those which, under similar circumstances, are obtained in the solution of sulphat-ammon. Solutions of chloride of platidum, carbozetic acid, and sulphate of alumina, behave towards or operate upon, the solution of parasulphat-ammon, as towards that of sulphat-ammon.

Little as is the indication of the presence of sulphuric acid in a solution of parasulphat-ammon, by treatment with solutions of the salts of barytes and of lead ; it is quite as little as might be expected, by solutions of the salts of strontia and of lime.

* In the original the numbers are 1,014 grm. and 2,065 grm. Then 1,014 : 2,065 : : 100 : 204·043 nearly.

† In the original the numbers are 1,001 and 2,047. : . 1,001:2 047 : : 100 : : 204·495 nearly.

I have long doubted that crystals of parasulphat-ammon essentially differ from sulphat-ammon; any further than that of their crystalline forms. It is well known how difficult it is to obtain perfectly anhydrous sulphuric acid; and if it contain only a trace of hydrated sulphuric acid, a corresponding trace of sulphate of ammonia is formed, by saturation with dry ammoniacal gas: and then the solution of barytes being so extremely sensible a reagent for sulphuric acid, it might easily occur that the solution of sulphat-ammon wouldbecome slightly precipitated, in the cold, by the solution of barytes; because of its being impure by the presence of the sulphate of oxide of ammonium.

It is true, however, that the solution of parasulphat-ammon, acts somewhat differently to that of sulphat-ammon, towards solutions of barytes and lead, and other reagents, such as the tartaric and rasemic acids. The sulphat-ammon is, also, somewhat more difficult of dissolution than the parasulphat-ammon, and does not so easily acidify when in a moist condition. But all these circumstances are of too little importance to enable us to decide with certainty, that the parasulphat-ammon differs from sulphat-ammon, only as an isometeric substance.

I have been induced to form this opinion from the following facts. If to a cold solution of pure sulphat-ammon a neutral solution of chloride of barium be added; where, of course, no free acid is supposed to exist, and sulphate of barytes be permitted to be completely precipitated: and in half an hour or an hour afterwards we filter the liquor, in a few hours more the clear liquid again becomes troubled, and sulphate of barytes begins to fall down: and this takes place even after other filtrations of the troubled liquid. This is not the case with a solution of parasulphat-ammon, which will remain clear in the cold for many months, if no free acid be added. In these experiments equal weights of both isometrical substances were dissolved in similar quantities of water, and treated with the same quantity of the solution of chloride of barium.

I consider these different attributes to indicate an essential difference between these substances: and the following series of experiments are also perfectly decisive on this point.

One hundred parts of very pure sulphat-ammon weighed 97·9* parts after drying in a water bath. This salt was then dissolved in cold water, without any acid, and mixed with a cold solution of chloride of barium. An hour after mixing, the sulphate of barytes was separated by the filter and washed with cold water, but towards the end, with warm water. It amounted to 51·71 parts :† answering to 18·16 per cent of sulphuric acid. To the filtered solu-

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* In the original the numbers are 1,516 grm. and 1,484 grm., therefore 1·516 : 1·484 : : 100 : 97·9 nearly.

† Original is 0·784 grm.

tion, hydrochloric acid was added, and then evaporated to dryness. The dry residue slightly heated, treated with water and hydrochloric acid, gave 13·78* of sulphate of barytes; which answers to 51·16 per cent. of sulphuric acid.• The quantity of sulphuric acid contained in this substance amounts to 69·32 per cent, which, by calculation, is very near the quantity contained in sulphat-ammon.

Suppose, now, that the 18·16 per cent. of sulphuric acid is precipitated in the cold, to be derived from an admixture of sulphate of ammonia with sulphat-ammon, they would answer, or be equivalent to 30 01 per cent. of sulphate of ammonia. The 51·16 per cent. of sulphuric acid obtained by evaporation, indicates 73·05 per cent. of sulphat-ammon; which gives an excess of 3·06 per cent. which the analysis does not admit of.

There are two additional experimental enquiries which have shown the decided difference between sulphat-ammon and parasulphat ammon. 100 parts† of the same kind of sulphat-ammon as that before operated on, and corresponding to 97·93 parts‡ of dried sulphat-ammon, were dissolved cold with a solution of chloride of barium, gave 63·84§ of sulphate of barytes; which, in half an hour after precipitation, was separated by the filter; and answers to 22·41 per cent. of sulphuric acid in the silt. In another experiment the sulphate of barytes was separated an hour after the precipitation, and gave 23·49 per cent of sulphuric acid. In neither of the above experiments was the quantity of sulphate of barytes ascertained.

It is obvious, however, that the quantities of sulphuric acid, when precipitated in the cold, may differ very considerably. The quantity of sulphate of barytes obtained in the cold by a solution of the chloride of barium, depends upon the quantity of water in which the sulphat-ammon is dissolved, the concentration of the solution of chloride of barium, and also upon the time of standing before filtration.

Were we to suppose, that in the last described experiment, the sulphuric acid precipitated in the cold is derived from the sulphate of oxide of ammonium, there would arise greater contradictions than would attend the results of the first-mentioned analysis; for 22·41 parts of sulphuric acid would correspond to 37·03 parts of sulphate of oxide of ammonium. The different analyses of the sulphat-ammon having constantly given 70·03 per cent., or very nearly of sulphuric acid, there would be obtained by further treatment 47·62 per cent. of the same acid, which corresponds to 68 parts of the sulphat of ammon,

* Original is 2·209 grm.
† Original is 1·665 grm.
‡ Original is 1·630 grm.
‖ Original is 1·063 grm.

In this case, however, the quantities of sulphate of oxide of ammonium, and of the sulphat-ammon would amount to 105·03 per cent., and thus the analysis would indicate 5·03 per cent. in excess.

By the preceding examination of the sulphat-ammon, in the cold, 23·49 per cent. of sulphuric acid were obtained. If these yield 38·81 parts of sulphate of oxide of ammonium ; and if, 46·54 parts of sulphuric acid, obtained by evaporation, answer to 66·46 parts of sulphat-ammon, the analysis would have given 5·27 per cent. of excess.

III.—The Deliquescent Salt.

This salt, as has already been remarked, is contained in the mother-liquor from which the parasulphat-ammon has been obtained by crystallization. If we evaporate this to dryness over sulphuric acid in a vacuum, we obtain imperfect crystals, or only papillary crystallized crusts, which, in time, attract moisture from the air, and eventually deliquesce.

It is very difficult to obtain this salt perfectly free from parasulphat-ammon. It is, indeed, more soluble, although the parasulphat is not very difficultly soluble, which makes it impossible to separate them when small quantities are operated on. By operating on larger quantities I have accomplished their separation. In this case I permitted the mass, which was obtained from the mother-liquor when evaporated to dryness over concentrated sulphuric acid in vacuo, to partly deliquesce in the atmosphere, or by adding a few drops of water to it. I then evaporated the small portion of dissolved salt to dryness as before, and afterwards analysed it.

When this salt contains parasulphat-ammon, and is slowly evaporated over sulphuric acid in the open air, the moist crystals very readily become acid. We must therefore pick out the crystals of parasulphat-ammon as much as possible from the dried mass : the deliquescent salt must then be dissolved in water, and add carbonate of barytes to saturate the free acid, and again evaporate over sulphuric acid in vacuo.

This salt crystallizes so very imperfectly, that one cannot ascertain the crystalline forms—they are usually mere crusts ; and any crystals which one observes with bright faces, are parasulphat-ammon.

The solution of this salt precipitates solutions of barytes immediately ; but, as also happens with the solution of sulphat-

ammon, the whole of the sulphuric acid is not precipitated in the
state of sulphate of barytes. When hydrochloric acid is added
to the solution, more of the sulphate of barytes is precipitated
in the cold than without the ácid. A solution of chloride of
strontium produces no immediate precipitation in solutions of
this salt unless very concentrated . in which case, a very prompt
precipitation appears. This fact distinguishes it from sulphat-
ammon. If equal quantities of both salts be dissolved in equal
quantities of water, neither of the solutions are precipitated by a
dilute solution of the salts of strontia : but after standing for
some time, that re-agent, if not too dilute, causes precipitation
in the solution of the deliquescent salt, whilst that of sulphat-
ammon remains clear.

   A solution of acetate of lead precipitates the solution of the
deliquescent salt in the same manner as that of sulphat-ammon.
A solution of chloride of calcium does not affect either of these
solutions.

   Solutions of chloride of platinum, sulphate of alumina, tartaric
acid, racemic acid, and carbazotic acid behave towards a solu-
tion of deliquescent salt as towards one of sulphat-ammon.

   It is difficult to prevent the solution of this salt from reacting
as a feeble acid on litmus paper : but this reaction is very insig-
nificant when the salt is well prepared.

   The salt which was prepared in vacuo was afterwards dried
on a water bath until it ceased to lose any more weight. One
hundred parts of this dried salt dissolved in water, mixed with
a solution of the chloride of barium, and permitted to stand
for twenty-four hours in the cold, gave 20·42 parts of sulphate
of barytes. Hydrochloric acid being added to the filtered
solution, it was afterwards evaporated to dryness, and the residue
heated with hydrochloric acid. The quantity of sulphate of
bartyes now precipitated was 166·18 parts. The quantity of
sulphate of bartyes, precipitated in the cold amounts to scarcely
one-eighth of that which was obtained by the former operation.
Both quantities gave 64·14 per cent. of sulphuric acid in this
salt. This answers nearly to a compound of anhydrous sulphate
of oxide of ammonium, with ½ an atom of water combined,
which by the formula's $\overline{N H^3} \times \frac{1}{2} \dot{H}$ contains in a hundred parts.

$$
\begin{array}{lr}
\text{Sulphuric acid} & 64·93. \\
\text{Ammonia} & 27·79. \\
\text{Water} & 7·28. \\
\hline
& 100·00. \\
\hline
\end{array}
$$

   By a repetition of this experiment with a small portion of
this salt, obtained at another time, from a solution of pure

sulphat-ammon, I got in the cold from 100 parts, by adding hydrochloric acid and chloride of barium, 106·06 parts of sulphate of barytes: and the, residue, after evaporating to dryness and again treating it with hydrochloric acid, yielded 84·62 parts more of the sulphate of barytes. We see, from these results, that more sulphuric acid is precipitated from this salt in the cold when mixed with hydrochloric acid than when this acid is not the used.

Both quantities of sulphate of barytes indicate 65·54 per cent. of sulphuric acid in the salt. The small excess is obviously derived from some small portion of parasulphat-ammon contained in the salt, in consequence of its being obtained from only a small quantity of sulphat-ammon.

When I prepared the crystals of parasulphat-ammon the first time, having obtained but a small portion, I resolved not to analyse them, but to examine the incomplete crystalline mass obtained by evaporating to dryness, which must consist of a mixture of deliquescent salt and parasulphat-ammon* The investigation has confirmed this fact, as I have found only 67·47 per cent. of sulphuric acid in the mixed substance.

The hydrous sulphat-ammon is quite analogous to a salt ·which I discovered whilst examining the compounds of carbonic acid and ammonia,† and which consists of anhydrous carbonate of ammonia, with half an atom of water, necessary to convert the ammonium into oxide of ammonium. We may take the same view of the hydrous sulphat-ammon; and also with a compound of sulphat-ammon with sulphate of the oxide of ammonium $\overline{S}\ \underline{N}\ \overline{H^3} + \overline{S}\ \underline{N}\ \overline{H^4}$. The salt may, perhaps, be formed by saturating the first hydrate of sulphuric acid $2\ \overline{S} + \overline{H}$ contained in the Nordhausen oil of vitriol with dry ammonical gas.

The deliquescent salt, without doubt, originates in the parasulphat-ammon when dissolved in water, and permitted to remain for some time in contact with it. Very pure crystals of parasulphat-ammon, which are entirely free from deliquescent salt, if dissolved in water, and evaporated over sulphuric acid in vacuo, will always yield a considerable quantity of deliquescent salt among the crystals of parasulphat-ammon.

As the crystals of parasulphat-ammon become acid by long exposure to damp atmospheric air, it appeared to me of some interest to inquire into the nature of the change which they suffer. Selected pure crystals were reduced to a powder, and moistened some hours, by which treatment the salt acquired an acid reaction. It was then perfectly dried in a water bath

* * Poggendorff's Annalen, Bd. XLVII, S. 474
† Ibid. BXXXXVI. S. 373.

One hundred parts of the dried salt were dissolved in cold
water. The solution reddened litmus paper, but not very
strongly: and gave a precipitate with a solution of chloride of
barium. By the method often hitherto described, I obtained
198·19 parts of sulphate of barytes, which is equivalent to
68·13 per cent. of sulphuric acid in the salt. It appears from
this result, that the parasulphat-ammon, by moistening with
water, becomes partially converted into the deliquescent salt.
The acid reaction arises from the presence of free hydrate of
sulphuric acid.

It results from these investigations, that although sulphat-
ammon appears to dissolve in water without suffering decom-
position, yet, when the solution is brought to crystallization,
the obtained crystals, notwithstanding their similarity of
composition with those of sulphat-ammon, do possess many
properties which it does not exhibit. In the solution of sulphat-
ammon, the constituents of the water are more easily combined
with it by the operation of some re-agents, and the compound
thus becomes changed more readily. This is the case with the
crystallized sulphat-ammon, or the parasulphat-ammon which
withstand more powerfully the operation of such re-agents.
The conditions of sulphat-ammon and parasulphat-ammon may
be compared with the vitrious and crystalline conditions of
certain bodies, in which different properties are exhibited, not-
withstanding their solutions shew no such a difference of modi-
fication as those of anhydrous sulphate of ammonia.

The combinations of anhydrous sulphuric acid with ammonia,
may, according to Dr. Kane's views, be considered as a hydrate
of sulphuric acid; by supposing the ammonia as an amide of
hydrogen, and that amide to combine in a similar manner with
other bodies, such as oxygen and chlorine: with which the
amide of hydrogen becomes a body analagous to the oxide and
chloride of hydrogen. When, however, sulphuric acid is com-
bined with water, or other oxybases, it may possess very dif-
ferent properties to those which it exhibits when combined with
an amide of hydrogen. We have, in fact, recently become
acquainted with a great number of cases in which the sulphuric
acid, when combined with certain substances, as for example,
with the oxide of ethule, and other bodies of organic origin,
loses some of those properties by which we were previously
habituated to characterize it: especially that of forming an
insoluble precipitate with solutions of salts of barytes.

Hypothetical as this view may be, the explanation which Dr.
Kane gives of the compounds of ammonia with hydrous oxiacids
is equally so. It is well known that Berzelus considers these
salts as an oxide of ammonium; and by this view, the analogy
of them with those formed with other oxybases is maintained:

as is also the isomorphism of some salts of potash and of oxide of ammonium: which hypothesis became rapidly and almost universally adopted. But upon Dr. Kane's hypothesis, this numerous class of ammonical salts consists of combinations of acids with two bases, the oxide and the amide of hydrogen: and the sulphate of the oxide of ammonium will, accordingly with this hypothesis, become analogous to several sulphates, which, at higher temperatures, retain one atom of water. But the direct analogy and isomorphism of these ammonical salts, with the salts of potash, instead of being supported, will, according to Dr. Kane's views, be thrown into the back ground. Graham also explains them as a proper acknowledgment of the theory of Kane, and upon similar grounds, of the views of Berzelius.*

I will now make a few remarks on the analogy of the compounds of sulphuric acid with ammonia, and of bicarburetted hydrogen, (Elayl or Atherol of Berzelius) which was pointed out, by Dumas, a long time ago.† The elayl and the ammonium, when combined with hydrogen, one of the hypothetical radicals, æthyle . the other no less hypothetical radical, ammonium. Both radicals may be combined with sulphur, chlorine, bromine, and iodine. When united with the elements of water, one yields the base, oxide of æthyle; the other the base, the oxide of ammonium. Both bases unite with anhydrous oxyacids. Moreover, bicarburetted hydrogen, as well as ammonia, combine *directly* with anhydrous sulphuric acid. This acid will also combine with oxide of æthyle; which compound is found in the sulpho-tartaric acid, and in its salts: also in oxide of ammonium. The sulphuric acid also forms compounds with elayl, or rather with ætherol; as well as with ammonia, which contain so much water or its elements, that only one half of the bicarburetted hydrogen and the ammonia can be converted into the oxide of æthyle, or the oxide of ammonium. The former compound is the oil of wine (sulphate of the oxide of æthyle—ætherol:) the latter the deliquescent salt, retained in the mother-water from which the parasulphate-ammon has been separated by crystallization.

From these comparisons, however, we are not to place much importance; for they refer only to a certain analogy in the composition, which itself may be regarded as a remote one: because bicarburetted hydrogen and ammonia differ with respect to their number of elements. Still less will this comparison hold good by referring to the very different properties of those substances, which at no time have even a remote resemblance of each other.

* Elements of Chemistry, by Thomas Graham, p. 417.

* Poggendorff's Annalen, Bd. XLII. S. 452.

On *Atmospherical Electric Apparatus, and Experiments.* By
W. H. WEEKS, Esq., Surgeon, Lecturer on Philosophical
and Operative Chemistry, &c., described in a Letter to
the Editor.

High-street, Sandwich, 15th May, 1841.

MY DEAR SIR,

The phenomena originating in that peculiar property of
electricity, denominated the *lateral explosion*, as no one can be
better aware than yourself, have, from the time of the philoso-
phical Priestly, down to the present brilliant period in the
history of this important science, occasionally commanded the
attention of its most eminent professors. This subject, ever of
the highest order, in reference to the grand practical operation
of Franklin, has recently attained an accession of interest from
the laudable design manifested with a view to the protection of
the British navy, by the adoption of a system of marine lightning
conductors; and, I assure you, it is with feelings of great
satisfaction that I have witnessed the distinguished position you
have taken and maintained on the field of controversy, in which
the electricians of all countries have, from the vital interest of
the inquiry, been more or less induced to engage. My senti-
ments as well as my individual share of past experience, in relation
to such practical facts as tend most directly to elucidate the
extreme danger of *lateral discharges from lightning rods,* when
placed in vicinal situations with combustible bodies, or sub-
stances possessing even a very moderate capability of ignition,
must be already tolerably familiar to you, from the occasional—
and to me gratifying—correspondence of the last two or three
years; but I am now bound to tender you my especial thanks
for having, by your letters of the 30th March, and 19th April
current, renewed my attention to the subject; and at the same
time, suggested an experiment of a most decided character,
through the instrumentality of my atmospheric apparatus, very
briefly described at p. 89, vol 6, of the "Annals of Electricity,
Magnetism, and Chemistry."

In your communication of the 19th ultimo, you have most
philosophically remarked, that "although we frequently employ
an extensive coated surface of glass to mimic some of the
effects of lightning, that apparatus is very far from being
imitative of a cloud floating in the air: neither is its electric
condition, when charged, any thing like that of a highly
charged cloud. The electric condition of an extended wire in
the atmosphere, either by means of a kite, or in the manner of
your apparatus, is the nearest to that of a cloud, that we can
arrive at artificially; unless indeed, we could, elevate an

enormous coated balloon to a great height." Since the re-
organization of my atmospheric machinery, which, as you have
previously understood, encountered very serious derangement
from the tremendous gales of the late winter, every day's
experience has tended to confirm the truth of your observations;
Having, in the early part of the present month, completed my
aerial arrangements, and brought the apparatus into an ex-
tremely delicate state of insulation, I soon found that even the
passage of a very moderate current of electricity from the
terminus to the lower ball in connexion with the ground, was
sufficient to produce a lateral spark, when a third ball, as sug-
gested by yourself, was presented to the one intermediate and
uninsulated ; and this occurred not only when ordinary modifi-
cations of cloud were in progress over the line of wire, but also
during the continuance of a transparent blue sky, accompanied
by a moderate breeze from the *eastward*, a condition of atmos-
phere which never fails to induce an electric current from the
wire, attended by sparks, a pungent pricking sensation to the
skin of the hands, and smart occasional shocks to the unguarded
operator.   It was, however, reserved for the concentrated
energies of a magnificent thunder-cloud advancing over the
line of our atmospheric arrangements, about noon on Sunday
the 9th instant., to crown the evidence of this experimental
series.   From the cloud before mentioned, proceeded, during
nearly an hour, not only the ordinary winged messen-
gers of the storm, but copious showers of rain, and an
almost continuous violent discharge of immense hail-stones,
many of which measured seven-tenths of an inch diameter.
At seventeen minutes past twelve at noon,   the   prelude
to the approaching electrical drama was announced in the
usual manner, from a sudden commotion raised by a series
of chimes, purposely connected with the in-door apparatic
appendages.
    If I remember correctly, I have already, in a hasty notice
despatched to you, within some few hours after the event,
briefly alluded to the general phenomena attendant on this
splendid scene ; the principal features of which I am now
anxious to communicate to you more in detail.   Such, however,
would be the imperfection of language in this instance, as on
many other occasions, that I must solicit the favour of your
attention to the accompanying sketch in pencil, which rude as it is
in point of drawing, will prove a volume of illustration compared
with the best efforts of my pen.   The sketch in question, (see
plate vii.) represents on the right-hand, a section of the end wall
of a building employed for the general purposes of a laboratory.
Proceeding from this towards the left-hand, I have endeavoured
to pourtray an idea of the local positions directly presented to
the eye of an observer, and which occur at right angles with

the wall in section before mentioned.   The subjoined references will, I trust, now supply all the information incident to our purpose.

In plate vii. *a b* represents a post about twelve feet in height, placed outside the building, four feet from the window to which it is directly opposed.

c d An iron rod, three-eights of an inch in diameter, descending from the top of the post to which it is affixed, and penetrating one foot into the ground.

e A brass knob providing the means of escape for superabundant fluid, which it attracts from the insulated funnel *c*, and conducts down the safety-rod *c d* into the earth.

f A mid-wire descending from the insulated horizontal line ; which line extends about three hundred and sixty-five yards between the two principal spires of the town. This descending wire brings down the fluid from the horizontal line to the funnel *e*.

g An intermediate wire helix connecting the insulated funnels *c* and *h*.

i k A strong tube of glass passing, at an angle of forty-five degrees. through the main post, or wooden upright of the laboratory window-frame.   Within this tube is concentrically insulated a stout wire of brass, completing the connexion between the funnel *h* and a large ball *l ;* thus forming a terminus to the horizontal exploring wire.

m n A gun-barrel conductor passing horizontally outwards through the wooden upwright of window-frame before mentioned.   Into the open end *m* of this gun-barrel, the right angled shank of the ball *o* is ordinarily inserted, and by this means the electric current proceeding from the terminus *l* (when not otherwise directed), is securely conducted by an iron rod *n p* beneath the surface of the ground, where a metallic connexion is found with a well on a distant part of the premises.   The ball *o* derives both horizontal and vertical motion, occasionally, from an *insulating* handle and other appendages not necessarily included in the accompanying sketch.

g r s t u Shew the position of a broad shelf, placed eight feet from the ground, and extending the entire length of a wall at right angles with the right-hand sectional view.   Upon this shelf are placed numerous articles of apparatus, many of which are wholly of metal, and others have metallic mountings.

r v w An hydraulic pump, supplying water for general purposes of the laboratory ; the handle, crank and fitting-rod composed, as usual, of wrought iron ; the spout *w*, with the pump-head from which it proceeds, and also the adjoining cistern being of thick milled lead.

x An iron nail about three inches in length, used to secure the vertical shifting joint formed by the extremities of the pump-handle and crank communicating with the lifting-rod.

y A brass ball about two and a half inches diameter, from which proceeds a rod terminating in an open ring at the opposite end ; this rod obtains vertical motion from a pivot connecting it with the brass pillar and foot-stand *z* ; the latter having free communication with the earth through the medium of a stout copper wire descending from the table upon which the stand is placed.

To the extremity of the rod communicating with the ball *y* is affixed one end of a copper wire, one-sixteenth of an inch in thickness, and *seventeen feet, five inches in length* ; the other end of the said wire being wound in a *loose coil* round the iron nail *x* which, as before remarked, secures the vertical joint formed between the crank and handle of the water pump. The intermediate portion of copper wire forming several convolutions, depends at liberty, touching in various points the floor of the laboratory, and this floor is itself composed of materials constituting one of the best possible conducting mediums to the earth. A second wire *t* proceeds from that last described to a metallic disc, one foot in diameter, placed upon the shelf before mentioned ; while a third wire *s* connects the iron handle of the pump with a brass ball and wire projecting from the summit of a turned pillar and foot-stand of mahogany wood. I now venture to persuade myself that you will experience no difficulty in comprehending a clear outline of my arrangements as they existed within some two or three minutes after the commencement of the storm.

The ball *o* having been removed from its usual position, and that marked *y* brought within striking distance of the terminus *l*; imagine a rain and hail storm of the most tremendous character, in full operation to the extent of several hundred yards around ; with a succession of lightning flashes at short intervals, and the reverberating peals of somewhat distant thunder fast nearing the immediate scene of our operations. Conclude further, that the balls *y* and *l* are separated to a distance of three and a quarter inches, and that in the next instant a mighty torrent of dense sparks, so vivid as to dazzle the eye of the observer, attended by contemporaneous stunning reports, and fraught with an unusual intensity, rushes from the terminus to the ball in communication with the earth. At the same identical moment a furious current of *lateral sparks* takes place between the wire and the leaden spout *w* ; others are seen in like activity passing from the wires *s* and *t* to different parts of the shelf *q r s t u*, and the several articles of apparatus in their vicinity ; while on the out-side of the building a brilliant zig-zag flash plays uninterruptedly between the insulated funnel *e* and the ball of the

safety-rod *c d.* But now comes to be described the most re-
splendent feature of the scene before us ; the iron nail, serving
to connect the pump machinery, suddenly exhibits the appear-
ance of a magnificent fire-work, the splendour of which is
repeatedly enhanced, as *waves* of electric fluid rush through the
arrangement in obedience to each successive lighting flash from
the storm cloud ; and this sublime scene, with short intervals of
lesser energy in the electric current, continues through the
space of one hour and sixteen minutes. The combustion of
the iron nail forcibly reminded me of the appearance which
that metal exhibits when burnt in oxygen gas, or, rather, when
brought under influence of the oxy-hydrogen blowpipe ; though
the phenomenon was accompanied by a deep red kind of light
which does not belong to either of these comparisons. Per-
ceiving that the nail was at times in a state of actual combustion,
I was induced repeatedly to hold a sheet of writing-paper
beneath it, at a distance of some five or six inches, and from the
subsequent application of a pocket microscope, I found the
paper thickly strewed with minute globules of the fused metal,
mixed with irregular particles of an oxide previously coating
the nail. This remarkable phenomenon, arising partly, as I
conceive, from the *loose connexion* between the wire coil and the
nail, and partly from the *ragged* and *asperous* condition of the
wire towards its termination, would appear to be very materially
allied to the nature of the *second* kind of lateral discharge,
noticed in your *Fourth Memoir* ; p. 174, vol. 1V., "Annals of
Electricity, Magnetism, and Chemistry."

While the scene first mentioned was passing in full splendour,
I employed myself, by means of a brass ball mounted on a rod
of the same metal, about five feet long, in drawing lateral
sparks from every part of the copper wire in its course from the
stand *z* to the nail *x* ; and, in like manner, from the handle and
every part of the iron and lead work connected with the water-
pump ; strong sparks were also elicited from the metallic disc,
and the brass wire and ball to which the respective wires *s* and
t had been affixed ; in short, there was no metallic body in
contact or communication with the main wire passing across
the laboratory, from which sparks were not copiously drawn ;
and in many instances these sparks were followed by shocks
which I venture to predict few inexperienced operators would
decide on provoking a second time, if left the liberty of choice.
An interesting and intelligent little girl about fourteen years of
age, and who is in the habit of witnessing scenes of this
description, while handing to me some articles of apparatus
incident to my purpose, accidentally trod upon the wire, and
such was the severity of the lateral shock thereby incurred,
that she was sent reeling across the laboratory.

Notwithstanding the severity of the hail storm, I was in-
duced, about the time of its greatest vigour, to affix a copper

wire to the safety-rod *c d,* outside the building; and this wire trailing upon the ground, was carried over an adjoining garden *to the length of eighty-seven feet,* and the lateral spark was readily obtained from every part of it, when a brass ball, two inches in diameter, was suddenly presented. From the safety-rod, *c d,* and the gun-barrel conductor *m n,* and also the iron rod *n p,* when the ball *o* was returned to its usual situation, dense sparks in a continuous volley, *notwithstanding the ground had had become very wet,* were sent off to the ball and wire held in my hand ; but this latter experiment I soon felt disposed to relinquish in consequence of the severe shocks incurred by the operation.

To you, who have for so many years laboured in the field of electrical science, and that, too, with a measure of success not often equalled, it would be more than ordinarily superfluous to offer any comments on the preceding statement. I have aimed at nothing but the communication of facts, which I am bold to say have not in a solitary instance received the aid of factitious colouring, but that, rather, in this this respect, I have fallen short of actual experience. It may, perhaps, be useless to add that I am not only confident, but I think it is clearly obvious, that combustible substances might have been readily ignited by the lateral discharge from every part of my arrange-ments, had time and circumstances permitted the trial. The inference, I presume, must therefore be conspicuous to the minds of all unprejudiced lovers of scientific enquiry ; and, I trust, in the relation existing between principle and appliance, not less so to every philantrophic lover of his species.

> I am, my dear Sir,
> Sincerely yours,
> W. H. WEEKES.

To W. Sturgeon, Esq.

On the Prevention of Damage by Lightning. In a Letter from Mr. B. Cook, to Mr. Nicholson.*

My Dear Sir,

I have read with much concern almost every week for some time past accounts of some damage of one kind or other done to buildings, trees, and cattle, or in the loss of lives by light-ning ; indeed every year this country suffers very much, either by the destruction of tress, houses, and cattle, and what is far more distressing, the loss of so many lives by the electric fluid.

* From Nicholson's Journal of Philosophy and Chemistry for August
1815

I have endeavoured to form an idea of the loss sustained on an average ; and I find upon a moderate calculation, it cannot be far short per annum of 40 to 50 thousand pounds, and the loss of lives from 20 to 30. It is of so serious a nature, that I wonder no effort has been made to remove, if not wholly, at least a part of the evil. Looking at it in this light, and conceiving it to be the duty of every man to endeavour to propose some remedy, I have taken the liberty to hand you what follows for your consideration ; if you think it worth your inserting in your valuable journal.

Our kingdom from its high and rocky nature, from its bowels containing such vast masses of iron, copper, and other ores, all conductors of lightning, and also from its situation in the midst of the waves, itself becomes a conductor also ; all these circumstances conspire to collect the electric fluid together around us. If it was possible to find out means to carry off this very destructive element without danger, the country would experience a great and invaluable benefit from it. The loss of so many lives is a very serious consideration, and ought to engage the studies of the philosopher and the philantropist to propose some remedy, if only for their sakes, and if it is impossible to remove the evil wholly, at least it is possible to remove it partially.

The plan I with deference propose, and I feel satisfaction in proposing it first to you for your consideration, because if you do not approve of it, it will not meet the eye of the world, for no man is more competent to decide upon its merits than yourself. The plan is to erect conductors throughout the kingdom, at five or six miles distant, or in some instance nearer, according to the nature of the ground, on the most elevated parts, so that whenever the clouds moved surcharged with the electric fluid, the conductors would carry it down, so that it would be next to an impossibility for a collection of electric fire to accumulate, so as to produce a destructive discharge. I have very little doubt, but that all, or nearly all of the fluid would be carried off by these conductors, and little or no damage, or death would ever be occasioned by the lightning.

The expense of erecting conductors at different stations throughout the kingdom would be saved in a few years, and the safety of men's lives would be of more value than any expense that could be incurred. If every parish would agree throughout the kingdom to appropriate a part of the rate for the erection of 4, 6, or more conductors, according to the size of the parish, on the different parts that are most elevated, the expense would not be felt—indeed it would not be worth naming. If the different noblemen, gentlemen, &c., of the different parishes were to take into consideration, first consider-

ing the certain security it would provide for their cattle, build-
ings, and the lives of themselves and servants ; and secondly,
when they estimate the very small expense these conductors
might be erected for ; I do think every parish would instantly
be induced to adopt the plan.

But there are several great imperfections and objections
against the present iron conductors.

The first is, the very short time they stand without being
deeply corroded with rust, and when first put up the iron is so
very irregular on the surface, that it is a great hindrance to the
descent of the electric fluid, and calculated in a great measure
to cause it to fly off to any other conducting substance in its
way or near to it ; and when up for a few years, it becomes
still worse, and so incrusted with rust, that the irregularity and
imperfections of the conductor are increased. Another fault is,
that the tops of the conductors are not raised high enough
above the building they are placed to protect ; the point of
the rod is in general placed just above the chimney? The rod
ought to rise six or eight feet above the top of the house or
building, and to end in a single point only. If conductors are
used, in every instance the best materials should be used to
make them. Iron is the very worst material, and yet all con-
ductors are made of iron ; but this arises from the cheapness
of the article.

According to the experiments of Mr. Henly (published in Dr.
Ree's Cyclopedia, under the article Conductors), to prove the
best conductors, he found the same charge from an electric machine
melted 4 inches of gold wire, 6 inches of brass wire, 8 inches
of silvered copper, 10 inches of silver, and 10 inches of iron
wire ; so that gold is the best conductor, and iron the worst.
Brass stands next to gold in the quality of a conductor. Cavillo
says, that copper and brass are the best conductors, and also
that they never rust ; but to make them of copper or brass
would be a very great expense, and then, if not drawn through
plates, they would be very uneven on the surface, which is a
defect in electric rods.

I had in prospect the making of conductors on an improved
plan, so that they would be equal to solid brass in their use,
and come to as cheap or cheaper than wrought iron, in a
patent I have very recently obtained for combining different
sorts of metals, particularly brass or copper, with iron. By
this method we can plate or cover the tubes of iron 15 or 16
feet long, of any diameter, with a coat of brass, from one-six-
teenth of an inch, to any thickness; and so connected with the
iron by compression, that, when so combined, it appears a solid
piece of brass, but being hollow, is very light and portable,
and the method used in making them being by drawing them
through a polished draw plate, all the surfaces are as smooth

and uniform as it is possible to make them. Being made in convenient lengths, they may be sent to any part of the kingdom, and put up in a very short time, as one piece screws into another, so that, when screwed in, both edges of the brass meet, and join together. Conductors of this kind would never rust, as what is presented to the atmosphere is brass only. They would be two-third lighter than iron rods, would be put up in a very short time, would be quite as cheap as iron, and furthermore would be the best conductors you can possibly make. But as I said before that I had given the subject a good deal of thought, especially the probability of drawing off the electric matter by conductors, so as to prevent its getting to a head and causing by its discharge so many accidents ; when I considered the manner of the iron rods and their great defects, it set me a thinking how I could contrive a better conductor than iron, and I flatter myself I have succeeded. Therefore I leave it to every man to judge, whether what I have asserted is true, namely, the great damage done yearly by lightning, and also the great necessity of providing, if it is possible, some remedy ; and if conductors are the only means that promise a remedy, those conductors which afford the most beneficial and lasting results will certainly be chosen. *This is, Sir, very much like recommending my own invention ;* but if my rods are the best, which I leave to every candid man to judge ; and if society is benefitted, I see no reason why I should not be benefited also. The present conductors on shipboard, where any are used, are I believe constructed of chains which are the worst of all conductors, as the lightning has to run down the most irregular of surfaces, besides their being so clumsy. But my brass rod might be so attached to the mainmast, and the collecting point raised above the top, and where the joints of the mast are, there might be a round universal joint, that would bend in every direction with the mast. The rod might be carried down thus into the sea, and the expense of them would be so trifling, that one would hardly think any vessel would be without one, especially when it is considered they would be made of metal allowed by all who have written on electricity to be the best conductor of lightning. •

<div align="right">I am, dear Sir,   ,</div>

<div align="right">B. COOK.</div>

<div align="center">*Annotation.*</div>

The subject of conductors for lightning being still obscure, I have with pleasure inserted Mr. Cook's communication without considering, as at all needful, that an acquiescence in

its contents should be implied throughout on my part. Being founded on the generally admitted doctrine, it is many respects entitled to consideration, and, like all other ingenious researches, is calculated to excite investigation. On the present occasion I would remark, that the course, disposition, and striking places of thunder-clouds appear to be governed in a very great measure by certain conducting parts lying along or within the earth, either as ridges or internal masses, and that a stroke from a stratum of clouds many miles in length, seems to be determined, by an action which extends far beyond the influence of any metallic rod, even supposing this last to be inserted into the conducting mass itself : that the whole process of atmospheric evaporation and condensation appear to be accompanied with electric phenomena upon a very extended scale, but most strikingly manifest when the changes are rapid ; this last being the only difference between thunder storms and common squalls or showers; and that it does not seem probable that our rods can essentially modify the course of these effects. Other more remote considerations would offer, if they could; such as the possibility of an interruption of the ordinary course and frequency of showers, which Darwin thought within the reach of human power, and the greater probability that the atmospheric electricity of a whole country would soon destroy any series of conductors ; but the affair of the poor-house at Heckingham,* in Norkfolk, which, about thirty years ago, was struck and set on fire by lightning without touching any of the eight elevated metallic conductors attached to the building, has been considered as a proof of the very limited influence of these rods, and that their power of protecting a single edifice requires the condition, that all the conductors should be connected together, and with the metallic parts of the house.

<div align="right">W. Nicholson.</div>

On the Prevention of Damage by Lightning. In a Second Letter from Mr. Benjamin Cook, to Mr. Nicholson.†

Dear Sir,

In a former paper which you inserted in your valuable journal, on the advantage and security that I supposed the nation would enjoy, if electric rods were placed at certain distances on the most elevated parts of the country, or if attached to the highest buildings at different places, so that the electric fluid might be

* See Philo's Translation of that time.

carried off by the rods, as the clouds charged with the fluid passed
over them ; by your remark at the close of that paper, it did not
seem to strike you, as promising that advantage and security it did
me, and you named an instance where the rods had failed ; but if
one instance or two have happened where the electric rods do not
appear to have had any influence on the electric fluid so as to
carry it off without injuring the buildings, this is no proof of their
inutility. We ought, before we pass judgment upon them, to
have known the state of the rods, and their elevation. It is very
probable that these rods had been up for many years, and nearly
destroyed by rust ; and perhaps in some parts the nature of the
iron might have been completely changed or destroyed, and
nothing left but rust ; nay in some places, even the rods might
have been divided, or nearly so, by the rust ; so that a weak dis-
charge of electric fire would easily melt what was left ; or they
might have been carried in such directions across, or down the
sides of the building, as to pass by substances possessing greater
power to carry off the fluid, than such rusty decayed conductors ;
and thus the lightning might have been by their means conducted
so as to cause the very ruin they were intended to prevent.
Besides, the points of the conductors might have been placed very
low, so that clouds overcharged with the electric fluid might have
passed so near the buildings, that every part that was a conductor
drew down the fire as soon as the rods, which had lost a part of
their power by rust. I say all this might have been the case, and
we therefore ought not so say, that electric rods have been found
ineffectual to ward off destruction.

I am desirous this subject should be fairly investigated, indeed
it is a national concern, and I do wish some able person would
take up the subject ; and if any of your correspondents could
produce any one instance where the rods, having been found in
proper order and position have failed, it would in a great measure
prove their inutility. On the other hand, if any one instance
could be brought forward, where they have proved beyond a doubt
the protectors of a building that without them would have suffered,
some basis might be laid down to form a just idea upon. This is
certain that we have each year to record great losses, both in
property and lives, by the electric fluid ; and if some plan could
be devised to remedy, if but in part, the evil experienced and
complained of, great advantage and safety would be procured to
society. My opinion is that electric rods are sure and certain
preservatives to every house where they are properly attached, if of
the proper kind ; and if a house can be secured, why not by the
same means a whole parish, by a proper number of conductors ?

But conductors are of little or no use made in the way they
commonly are, of a piece of iron wire one quarter of an inch in
diameter, or perhaps less ; for in many instances I have examined
they have not been so thick, some merely a strong wire. These
in a year or two are nearly or quite corroded through with rust ;
and they are attached in a careless way, with a number of rusty

points at top, directed to every point of the compass, and rising just above the chimney. It appears that if a rod is placed against a house or building, no matter how, the buiding is supposed to be safe ; and if this house or building is injured by lightning, it is the rod that was to have protected it, that is declared inefficacious. These rods are generally put up by some carpenter, or builder, who knows nothing of the nature or properties of the fluid he is guarding against, and therefore brings the rod down any way that is most convenient, without considering whether it passes near or even touches any conducting substance in the building ; in which case the rod, instead of protecting, is calculated to bring on the building the mischief it was intended to prevent.

Electric rods should be three quarters of an inch in diameter, according to my opinion ; should not touch the building in any part by three inches ; and all their fastenings to it should be by non-conductors. They should end in a single point of brass and this point be elevated six feet at least, but ten feet if possible, above the highest chimney of the house. If the rod is not brass, or a tube of brass, a strong brass wire ought to be wound round it, connected with the point, and passing once round the rod in the space of twelve or eighteen inches, sufficient to keep the brass wire close to the iron, all down to the earth. I have no doubt upon my mind, from all the observations I have made, that electric rods of this nature will never fail to give perfect safety. Even on board vessels an iron chain, the worst of all conductors that can be called a conductor, has been known to preserve the vessel and crew. As a proof I will quote a passage from Captain Cook's Journal of his Second Voyage Round the World.

"April 25th, 1774.

" Otaheite.—This day we had a very violent tempest. We were obliged to get our electrical chain up to the topgallantmast head, to secure the masts. Removed all the iron off the decks, and secured down all the hatches. As the seaman who carried the chain up, was coming down he got foul of the chain, and it lightning at the same time, he received a slight blow on the leg, which, though it did him no harm, shook every bone within him."

Captain Cook had seen an instance of the great utility of the electrical chain in his former voyage, while at Batavia, which, being of a singular nature, I shall relate in his own words, or as they are given by Dr. Hawkesworth.

" About nine o'clock we had a dreadful storm of thunder, lightning, and rain, October 10th, 1770, during which the mainmast of one of the Dutch East Indiamen was split, and carried away by the deck. The main topmast and topgallant-

mast were shivered all to pieces ; she had an iron spindle at the main topgallantmast head, which probably directed the stroke. This ship lay not more than the distance of two cables length from ours, and in all probability we should have shared the same fate, but for the electrical chain, which we had but just got up, and which conducted the lightning over the side of the ship. But though we escaped the lightning, the explosion shook us like an earthquake, the chain at the same time appearing like a line of fire: a centinel was in the act of charging his piece, and the shock forced the musket out of his hand, and broke the ramrod. Upon this occasion I cannot but earnestly recommend chains of the same kind to every ship, whatever be her destination ; and I hope that the fate of the Dutchman will be a warning to all who shall read this narrative, against having a spindle at the mast-head."

Thus, even chains have been found protectors, and if proper conductors were attached to the main topgallantmast, running all down it with a joint at the place where the mast is jointed, it would always be in its place; and I again say, I am pretty confident, that vessels would be secured from the injury they but too often sustain from lightning, as well as houses. The rod would not be in the way of any of the rigging, and therefore I should think it would be a duty the masters of vessels owe to their sailors, as well as to the owners of the property they have on board, to be always provided against danger.

<div style="text-align:center">

I am, dear Sir,

Your obedient Servant,

B. COOK.

</div>

Birmingham, Caroline-street, Dec. 27th, 1811.

<div style="text-align:center">

Extract from the London Journal of Arts and Sciences. Vol. v. p. 253. Published in 1823.

To the Editor of the London Journal of Arts, &c.

</div>

Sir,

I have read in your very interesting Journal of Arts and Sciences, several accounts of a new method of conducting the electric fluid clear of ships, by a Mr. W. Harris, and that his method has lately been exemplified on the Louisa and Caledonia men-of-war at Plymouth, and also on a vessel in the Thames. His method "is by means of a copper conductor fixed in the masts through the bottom of the ships," and to be continued until the copper comes in contact with water.

It gives me great satisfaction to see, that most excellent
method from damage from the electric fluid, is likely to be
adopted in the navy, as I am confident it will derive immense
advantage from the use of electric rods, by the perfect security
they will give to all vessels protected by them.   •

I have no doubt but Mr. Harris supposes that he is the first
inventor, for I cannot for a moment suppose that had he read
my papers in Nicholson's Journal of Natural Philosophy and
Chemistry, vol. 29, p. 305, and vol. 31, p. 108, but that he
would have acknowledged that the idea of attaching copper or
metal to vessels, to protect them from lightning, belongs to me.
Situated as I am in the centre of the kingdom, I had no means
of getting them adopted ; I can only give the idea to the world,
hoping that some persons in the navy might read it, who had
influence, or felt it their interest to adopt them.   I beg to refer
you to my papers, to show what were my ideas respecting
electric rods, for houses as well as ships, and would wish parti-
cularly to call the attention of your readers to my papers on
this subject, nor do I think they would be uninteresting to a
very numerous class of your readers, if you were to republish
some extracts from them, especially as we of late have wit-
nessed so many accidents from lightning, which might have
been prevented, had my method of attaching electric rods to
buildings, &c., been adopted.

<div style="text-align:right">

I am, Sir,

Your obedient servant,

B. COOK.

</div>

Birmingham, April 8th, 1823.

On a new Phenomenon of Electro-Magnetism. By Sir H.
Davy, Bart., Prec. R. S.

On a subject so obscure as electro-magnetism, and connected
by analogies more or less distant with the doctrines of heat,
light, electricity, and chemical attraction, it is not difficult to
frame *hypotheses* : but the science is in a state too near its
infancy to expect the development of any satisfactory *theory :*
and its progress can only be ensured by new facts and experi-
ments, which may prepare the way for extensive and general
reasonings upon its principles.   Influenced by this opinion, I
am induced to lay before the Society an account of an electro-
magnetic phenomenon I observed about fifteen months ago in
the laboratory of the Royal Institution, and which I have lately
had an occasion of witnessing in a more perfect manner, through
the kindness of Mr. Peppys, by the use of a large battery, con-
structed under his directions for the London Institution, and

containing a pair of plates of about two hundred square feet. In describing this phenomenon, I shall not enter into very minute details, because the experiments, which led to the discovery of it, are very simple, and, though more distinct with a large apparatus, yet it may be observed by the use of a pair of plates containing from ten to fifteen square feet.

Immediately after Mr. Faraday had published his ingenious experiment on electro-magnetic rotation, I was induced to try the action of a magnet on mercury connected in the electrical circuit, hoping that in this case, as there was no mechanical suspension of the conductor, the appearances would be exhibited in their most simple form : and I found that when two wires were placed in a basin of mercury, perpendicular to the surface, and in the voltaic circuit of a battery of large plates, and the pole of a powerful magnet held either above or below the wires, the mercury immediately began to revolve round the wire as an axis, according to the common circumstance of electro-magnetic rotation, and with a velocity exceedingly increased when the *opposite* poles of two magnets were used ; one above, the other below.

Masses of mercury of several inches diameter were set in motion, and made to revolve in this manner, whenever the pole of the magnet was held near the perpendicular of the wire ; but when the pole was held above the mercury, between the two wires, the circular motion ceased, and currents took place in the mercury in opposite directions ; one to the right, and the other to the left, of the magnet.   These circumstances, and various others which it would be tedious to detail, induced me to believe that the passage of the electricity through the mercury produced motions independent of the action of the magnet ; and that the appearances which I have described were owing to a composition of forces.

I endeavoured to ascertain the existence of these motions in the mercury, by covering its surface with weak acids ; and diffusing over it finely divided matter, such as the seeds of lycopodium, white oxide of mercury, &c., but without any distinct result.   It then occurred to me, that from the position of the wires, currents, if they existed, must occur chiefly in the lower, and not in the upper surface of the mercury ; and I consequently inverted the form of the experiment.   I had two copper wires, of about one-sixth of an inch diameter, the extremities of which were flat and carefully polished, passed through two holes three inches apart in the bottom of a glass basin, and perpendicular to it ; they were cemented into the basin, and made non-conductors by sealing wax, except at their polished ends ; the basin was then filled with mercury, which stood about a tenth or a twelfth of an inch above the wires.   The wires were now placed in a powerful voltaic circuit.   The moment the contacts were made, the *phenomenon,* which is the principal object of

this paper, occurred. The mercury was immediately seen in violent agitation ; its surface became elevated into a small cone above each of the wires ; waves flowed off in every direction from these cones ; and the only point of rest was apparently where they met in the centre of the mercury between the wires. On holding the pole of a powerful bar magnet at a considerable distance (some inches) above one of the cones, its apex was diminished and its base extended ; by lowering the pole further, these effects were still further increased, and the undulations were feebler. At a small distance the surface of the mercury became plane ; and rotation slowly began round the wire. As the magnet approached, the rotation became more rapid, and when it was about half an inch above the mercury a great depression of it was observed above the wire, and a vortex which reached almost to the surface of the wire.

In the first experiments which I made, the conical elevations or fountains of mercury were about the tenth or twelfth of an inch high, and the vortices apparently as low ; but in the experiments made at the London Institution, the mercury being much higher above the wire, the elevations and depressions were much more considerable, amounting to the 5th or sixth of an inch. Of course the rotation took place with either pole of the magnet, or either wire, or both together, according to the well known circumstances which determine these effects.

To ascertain whether the communication of heat diminishing the specific gravity of the mercury, had any share in these phenomena, I placed a delicate thermometer above the wires in the mercury, but there was no immediate elevation of temperature ; the heat of the mercury gradually increased, as did that of the wires ; but this increase was similar in every part of the circuit. I proved the thing more distinctly by making the whole apparatus a *thermometer*, terminating with a fine tube filled with mercury. At the first instant that the mercury became electro-magnetic, there was no increase of its volume.

This phenomenon cannot be attributed to common electrical repulsion, for in the electro-magnetic circuit, similarly electrified conductors do not repel, but attract each other : and it is in the case in which conductors in the *opposite*, states are brought near each other on surfaces of mercury, that repulsion takes place.

Nor can the effect be referred to that kind of action which occurs where electricity passes from good into bad conductors, as in the phenomena of points electrified in air, as the following facts seem to prove :—Steel wires were substituted for copper wires, and the appearances were the same in kind, and only less in degree ; without doubt, in consequence of a smaller quantity

of electricity passing through the steel wires . and by comparing
the conducting powers of equal cylinders of mercury and steel
in glass tubes, by ascertaining the quantity of iron filings they
attracted, it was found that the conducting powers of mercury
were higher than those of steel : the first metal taking up fifty-
eight grains of iron filings, and the second only thirty-seven.

Again : fused tin was substituted for mercury, in a porcelain
vessel, into which wires of copper and steel were alternately
ground and fixed ; the elevations were produced as in the mer-
cury, and the phenomena of rotation by the magnet ; and it
was found by direct experiment, that the conducting power of
the tin, at, and just before its point of fusion, did not percepti-
bly differ : and that they were much higher than those of
mercury.   Lastly, the communication was made from the bat-
tery by two tubes, having nearly the same diameter as the
wires, filled with mercury, so that the electricity for some inches
before it entered the basin, passed through mercury, and still
the appearances continued the same.

From the rapidity of the undulations round the points of the
cones, I thought they would put in motion any light bodies
placed above the mercury, but I could not produce the slightest
motion on a very light steel, hung on an axle ; and when fine
powders of any kind were strewed upon the surface, they merely
underwent undulations, without any other change of place ; and
fine iron filings strewed on the top of the cone, arranged them-
selves in right lines, at right angles to the line joining the wires,
and remained stationary, even on the centre of the cone.   The
effect, therefore, is of a novel kind, and in one respect seems
analogous to those of the tides,   It would appear as if the
passage of the electricity diminished the action of gravity on
the mercury ; and that there is no change of volume of the
whole mass of the mercury appears from the experiment, de-
scribed in the preceding page, and this was shewn likewise by en-
closing the apparatus in a kind of manometer, terminating in a fine
tube containing air enclosed by oil ; and which by its expansion
or contraction, would have shown the slightest change of volume
in the mercury : none however took place when the contacts were
alternately made and broken, unless the circuit was interrupted
for a sufficient time to communicate sensible heat to the mercury.

This phenomenon, in which the same effects are produced
at the two opposite poles, seem strongly opposed to the idea of
the electro-magnetic results being produced by the transition
currents or motions of a single imponderable fluid.

On the conjectural part of the subject I shall not however
enter, for the reasons stated in the beginning of this paper ; but
I cannot with propriety conclude, without mentioning a cir-
cumstance in the history of the progress of electro-magnetism,
which, though well known to many Fellows o this Society,

has, I believe, never been made public;* namely, that we owe to the sagacity of Dr. Wollaston, the first idea of the possibility of the rotations of the electro-magnetic wire round its axis by the approach of a magnet; and I witnessed, early in 1821, an unsuccessful experiment which he made to produce the effect in the laboratory of the Royal Institution.

On the Thermo-Electricity of Quicksilver. By P. O. C. VORSSELMAN, *of Heer.**

According to the *L'Institute* for December 1837, p. 388, which I have received to day, it appears the M. Matteucci, has communicated to the Academy of Paris, the following inference.

"If, in place of bringing into immediate connection the two wires of the same metal united to the two extremities of a gal-vanometer and heated unequally, we were to plunge them into mercury; or what is better, if we were to keep them in con-nection with that metal, or any other metallic connecting medium, contained in two capsules, united by a syphon, one of which is hot, and the other cold, the anomalies, which are pre-sented by iron in thermo-electrical phenomena, will not be ob-served. Copper, platinum, and iron then give out currents, which always travel in the same direction, that is to say, from the cold to the hot part of the wire by which they are connected. It is to some cause of oxidation, on the surface then, to which the anomaly in question is due. It appears to me that mercury is by this means deprived of the property of developing thermo-electric currents."

Similar experiments were first instituted by M. Nobil.[†] He took similar wires of the same kind of metal, and attached one end of each to a good galvanometer. The farther extremity of one of these wires was rolled into a small knob in order that it might receive a greater quantity of heat from a spirit lamp; and, when heated, was brought into contact with the unattached end of the wire. Immediately a momentary electric stream traversed the circuit. [‡] The current usually traversed the heated wire from the point of union to the cold end, and through the galvanometer, from the cold to the warm end. By employ-ing iron, zinc, and antimony the opposite results were found.

* Poggendorff's Annalen der Physik and Chemie. Band. XXXXIX S. 1. p. 114.

† Annalen der Physik and Chemie. Band XXXIV. S. 629, and Bd· XXXXVII. S. 600.

‡ Bibl. Univers. 1828. T. XXXVII. p. 118.

These are the anomalies observed by M. Nibili, and confirmed by Becquerel, which now, by Matteucci, is given out as a general rule for all metals.

What is it that Matteucci has done ? He has stated that he brought a heated and a cold wire into imperfect contact, and made the connexion through quicksilver, indeed through quicksilver in two different vessels which were united by a syphon. He thus brings into the circuit another heterogeneous metal : and it is therefore very obvious that his results can be no otherwise explained than by the thermo-electric action of the quicksilver : although he brings these results forward in contradiction to those which have, long ago, been discovered by Seebeck.

In the following experiments, which may be repeated at pleasure, I availed myself of a very delicate galvanometer, which belongs to Mr. Kerhover, and was made by our skilful mechanic, Becker. The metallic wires were from one to one and a half milimeters thick, and from one to two decimeters long : and for these experiments were carefully cleaned : and those extremities which were connected with the galvanometer, were kept, as much as possible, at the temperature of the room. The experiments were often repeated, and varied with different wires :—

1st. I obtained two copper-wires, and rolled one end of one of them into a knob or compact coil. I then heated this end in the flame of a spirit-lamp, or by other means. On completing the contact between this and the other wire, the deflection became 110°. and the current flowed from the heated point of contact to the cold end of that wire.

2nd. By employing platinum, the current also flows from the warm to the cold end ; but with zinc and with iron it flows from the cold to the warm end. Bismuth, and Antimony, gave various results ; of which more will be said.

3d. With silver I constantly found a current from the cold to the warm end. This metal, therefore, must be placed in the same class as that in which zinc and iron stand. I obtained these results as well by the ordinary silver wire, as with wire of purified silver, which had been purposely prepared for these experiments.

4th. Instead of making the connexion *directly*, by means of bringing the wires into immediate contact with each other, I now made the connexion by means of a drop of quicksilver in a small cup, or basin. To illustrate the results, I will call one of the wires *a*, and the other *b*, it being understood, that one end of each is already joined to the galvanometer, and the other ends of these wires were occasionally made to touch quickcksilver. One of the wires was first connected with quickicksilver ; the end of the other was then heated, and

afterwards made to touch the mercury also. In this case, when the drop of quicksilver was not too large, the results were similar to those in experiments 1, 2, and 3.

5th. When I heated the quicksilver, and then touched it first with *a*, and then with *b* ; or first with *b*, and then with *a* I obtained very anomalous results, which prevented me from arriving at any thing decisive by this particular set of experiments.

6th. I now employed a flat bar of wood, in which a channel was made, one decimeter long, and two millimeters broad, which I filled with quicksilver. By this means I obtained a quicksilver wire, whose ends could be connected with the ends of any pair of homogeneous wires. By connecting one wire *a*, of a pair of the same metal, and then heating the other wire *b*, and immediately afterwards bringing it into contact with the other extremity of the line of quicksilver, I obtained a powerful current *through* the quicksilver, *from a* to *b* : or from the cold to the warm wire : and so on, from the warmed point to the cold one of it, which was in connexion with the galvanometer. This was the case, whether the pair of wires were of platinum, copper, zinc, silver, iron, or antimony.

. •7th. The same results were obtained when either of the extremities of the quicksilver was warmed, by touching it with a glowing hot needle, or stout wire.

8th. When, instead of a pair of wires, I took two small bars of bismuth, and proceeded as above, by first connecting one of the bars with one end of the quicksilver, and after heating the other bar, by bringing it into contact with the other end of the quicksilver, I obtained similar results to those described by M. Matteucci. There was a strong current *through* the quicksilver from the hot to the cold bar : and consequently through the heated point, *from* the bismuth to the quicksilver. Should Matteucci now admit these facts, he might also say that, the thermo-electric stream proceeds from the cold to the warm end, with all the metals, with the exception of bismuth, in which it takes the opposite direction. There would be then nothing more done than that of transferring the anomaly from the iron to the bismuth. But it appears to me to admit of no doubt, that these appearances only indicate the thermo-electric properties of quicksilver, which is less negative than bismuth, but more negative than the other metals : so that we have this arrangement—bismuth, quicksilver, platinum, copper, zinc, silver, iron, antimony.

9th. The correctness of this inference still more distinctly appears, by employing two distinct kinds of metal ; for instance, copper and platinum. When the end of the platinum wire is heated and then made to touch one end of the strip of quick-

silver, the copper-wire being previously in connection with the
other end of that strip, the current flowed through the heated
point, from the quicksilver to the platinum : but if we regard
the quicksilver to be indifferent, as Matteucci has done, we
would say that the current ran through the heated point, from
the copper to the platinum, in contradiction to the experience
of all philosophers.   In this case the current is due to the
thermo-electric element formed by quicksilver and platinum :
and arises from the same cause as those already described in
experiments 6 and 7.

If, by the rest which Matteucci says, he means that, " with
all other metallic alloys" the same results are developed, he
probably alludes to the attribute of bismuth ; which, according
to the experiments of Seebeck, is strongly negative,* and con-
sequently must thus operate like quicksilver.

10th. I said that a bar of bismuth and of antimony, show
anomalous effects.   At first I had much trouble to explain the
contradictions of my results with those of Nobili and other ex-
perimenters : though eventually, I believe, after many experi-
ments, I have been enabled to find out the cause.

If we employ two small bars, both of bismuth, or of antimony,
and after warming the one bring it into contact with the other,
immediately an electric current is produced: *whose direction
depends upon the difference of temperature.* If this difference
continues within certain limits, the current, with the bismuth,
proceeds from the cold to the warm end ; but with the antimony,
from the warm to the cold end. But as soon as the bar becomes
stronger heated, the direction of the current reverses, and pro-
ceeds by the opposite route.   I have very frequently observed
this reversing of the current, whilst experimenting with anti-
mony.   By a strong heat I obtained first a current from the
cold to the heated bar, as was shown by Nobili's experiments ;
I then broke the contact of the two bars to cool the heated one
a little in the air, and then again united them.   The needle im-
mediately turned round in the opposite direction, indicating a
current in the opposite direction to the former.   I presume that
Nobili had also employed a high temperature in his experiments.

We find also by the employment of one and the same metal,
what, a long time ago, was discovered by Becquerel in experi-
menting with a circuit of two metals such as copper and iron,
silver and zinc; and also what takes place by another circuit of
silver and iron, viz., a reversal of the current when the differ-
ence of temperature of the two bars becomes greater than a
certain limit.   We must also take care that the small bars have
a clean metallic surface, otherwise we obtain many anomalous
results, which, probably, are due to oxydation.   I hope still to

* Poggendorff's Annalen der Physik and Chemie. 1826.

arrange some further experiments on this topic. But it is also open for the enquiries and considerations of other philosophers, from which I expect that some light will be thrown on the subject.

At this time my object was only to clear up the thermo-electric property of quicksilver to M. Matteucci, and to show the existence of the anomalies ; which, perhaps, may soon become known as a necessary consequence of an universal law.

Something further on the Thermo-Electricity of Quicksilver.
By P. O. C. Vorsselman, of Heer.[*]

Since the publication of my experiments, in April, 1838, (described in the preceding paper) I have found a very extensive notice of Matteucci's experiments, in the *Bibl. Univers,* for Nov. 1837, p. 211 ; which places me under the obligation of once more returning to the subject.

Matteucci places great importance upon the following experiment. If we heat a small portion of quicksilver in a cup, and bring into contact with it, first one *a*, and then the other *b*, of two copper wires, *a b*, already in connexion with the galvanometer ; the contact thus produced, proceeds from *b* to *a*, or from the wire last, to the wire first immersed ; and therefore, says Matteucci, from the cold to the warm copper-wire.

I had in my experiments, § 5, observed this diviation, and considered it as an anomalous effect. On further consideration, I by degrees, found a new proof of the position which Matteucci denies. In the present case, what happens in the experiment? The copper wire *a*, is immersed in the heated quicksilver, and accordingly, positive electricity goes from the quicksilver to the copper ; and now, as soon as the circuit becomes completed by the introduction of the other wire *b*, a current must proceed from the quicksilver to the wire *a*; and although it appears to go from the cold to the warm copper, it in reality sets out at the heated point, from the quicksilver to the copper. The current is only a momentary one, for the temperature of the ends of the two wires soon become equal, and consequently annihilates the current.

If we make the experiment in precisely the same manner with two small bars of bismuth, then the current proceeds in

the opposite direction, or from the first to the last introduced bars, or from the quicksilver to the bismuth through the point of heat.

The accuracy of this explanation will be rendered undeniable by the following experiment. Whilst the wire *a* was immersed in the heated quicksilver, I touched it with the other wire, *b*, without permitting the latter to come into contact with the mercury. The current, in this case, proceeded through the point of contact, from *a* to *b*, or from the warm to the cold copper. In this case Matteucci's pre-supposed *cause d'oxidation on the surface,* is completely avoided.

In the before-mentioned notice, I see that M. Matteucci has attempted to obtain thermo-electric currents by connections with cold and warm quicksilver, independently of any other metal, but without success. He appears not to have trusted much to the accuracy of his galvanometer, for he says " although the wire of the galvanometer was, perhaps, a little long, I doubt, however, that on the surface of the mercury there was a development of any thermo-electric currents."

It appears, that in Matteucci's experiment, the wire of his galvanometer was too long, and the needle not sufficiently sensible. By the excellent instrument, for the use of which I am indebted to Mr. Kerkhoven, I can easily and distinctly observe these thermo-electric currents. In addition to that instrument, I have availed myself of the following arrangement : —

In a board I have a channel made, of about from 1 to 2 decimetres long, and in the middle of the channel is a round well of about two inches diameter, which can be closed on the under side by an iron or a glass plate ; under this I place a lamp, for the purpose of boiling the quicksilver with which the channel and well are filled, and a wire from each extremity of the channel of quicksilver proceeds to the galvanometer. By a small slip of wood or paper, or by other means, I can agitate the mercury in the well so as to move it towards one or other of the wires. When the mercury in the well was heated, and then by taking hold of the partition wall, the cold quicksilver was brought into contact with the heated, I immediately perceived a current, which proceeded through the point of contact from the warm to the cold metal. The deflection, however, is not great, perhaps not more than 6° or 10°, but its presence is unquestionable. In this case the disturbing influence of all foreign bodies is avoided. The quicksilver possesses no lack of power in combination with other metals, in exciting thermo-electric currents ; and can also alone, as well as other metals, become electric by an inequality of temperature.

When the quicksilver, as one of the metals, is in series with platina, or copper, the current proceeds through the point of

junction, from the warm to the cold metal; whilst on the other
hand, with iron, zinc, silver, it takes the opposite direction, or
from the cold to the warm metal. I have, however, also seen
that the direction of the current is changeable, with a change of
temperature; at least this is the case with some of the metals.
This anomaly has not existed in the cases now mentioned. I
should like to inquire, in this place, why should one direction
more than another be obliged to be viewed as normal or ano-
maly? I believe that all these varieties would become much
easier explained by referring them to one general cause. When
we have once determined with sufficient accuracy at what
temperature the current changes its direction, whether in a
circuit of one metal or of two metals, we shall have accom-
plished a great step in the knowledge of this still mysterious
power.

Specification of a Patent granted to GOLDSWORTHY GURNEY,
of the county of Cornwall, and FREDERICK RIXON, *of the
county of Middlesex, for their invention of improvements in
the apparatus for producing and distributing light.*—[*Sealed
8th June,* 1839.]*

This invention in the improvements in the apparatus for
producing and distributing light, is applicable to lamps or
burners, wherein oil or oleaginous matters, in a liquid state, are
the materials consumed for producing the illumination; and
our improvements also apply to various kinds of lamps, burners,
or lights, wherein the inflammable gas or vapour obtained by
the distillation of coal, oil, resin, asphaltum, or other betumi-
nous, resinous, or oleaginous matters is used, as the material
for illumination; such gases or vapours being previously ob-
tained, and then conveyed to the lamps or burners by pipes from
a reservoir or gasometer; and consist, in the first place, in im-
proved arrangements and constructions of conducting pipes or
tubes and cocks, and jets or burners, whereby we are enabled
to introduce into the interior of the flame of such lamps or
burners, a stream or jet of pure oxygen gas. The atmosphere
or atmospheric air being carefully excluded therefrom, that is,
from passing through the burner, or into the interior of the
flame. This jet or stream of pure oxygen is applied or given
to the purpose of producing a more intense ignition of the car-
bonaceous matters, and consequently a more brilliant light than
can be obtained where atmospheric air alone, or a mixture of
atmospheric air and inflammable gases, are used to cause com-
bustion.

* In order that our enquirers about the *Bude light* may have the best pos-
sible information, we have given the particulars as they appear in the spe-
cification,—EDIT.

It may be proper here to remark, that we are perfectly well
aware that mixtures of air and inflammable gases have been
applied to flame for the increased production of heat, and in
certain cases to produce light, (in a peculiar description of lamp)
which lamps burn the vapour or gas arising from the liquids
contained within themselves; we therefore wish it to be dis-
tinctly understood, that our improvements have nothing what-
ever to do with such mixtures of atmospheric air and inflam-
mable gases.

And we are also aware, that pure oxygen has been applied
to hydrogen gas for the purpose of producing intense heat for
blow-pipe purposes, and on lime for producing light; we there-
fore desire it to be understood, that our invention only applies
to administering to the flames of oil or gas lamps, or burners,
a jet or stream of pure oxygen, which, of itself, is not inflam-
mable; but, when applied to the flame in the manner hereinaf-
ter described, and at the proper place, will produce intense or
bright and clear light, which is caused by the oxygen or sup-
porter coming into contact with the combustible bodies or
inflammable gases, at the point of ignition. Therefore, as regards
our invention, this jet or stream of pure oxygen may be called
the source or cause of the extra light given out; and we have,
therefore, in contra-distinction to all other lights, called this
" The Olio-oxygen, or Bude Light."

And secondly, our improvements consist in the application
and use of peculiar and novel constructions and arrangements
of apparatus, whereby flashing or intermitting lights may be
caused, or produced, and used as signals for locomotive steam-
engines or carriages, and steam or other vessels, or in other
situations where they may be required; such intermitting or
flashing lights being caused or brought into operation by the
passage of streams or bubbles of pure oxygen through flame,
when brought in connection therewith, or of inflammable gases
when burnt alone without the jet or stream of pure oxygen,
such bubbles of inflammable gases being ignited by a small
stationary and continuous light; the bubbles of gases being
produced by their passing through an inverted syphon, con-
taining liquid, acting as an hydraulic valve. Or the same effect
may be caused by the actuating power of machinery or an en-
gine, and may therefore be made capable of indicating the speed
which at a locomotive engine or steam vessel is travelling, by
the rapidity with which the flashes are repeated.

This effect may be produced by any proper arrangements of
of mechanism depending on the revolution of the wheels, or the
strokes of the engines, or other moving parts of the machinery;
or in the former case by increasing or diminishing the column
of liquid in the inverted syphon, and thus causing a greater or
less resistance to the passage of the gas. The same effect may

also be produced by altering the capacity of the gas and water tubes, and thereby causing a quicker or slower pulsation of the light.

The construction of the lamp requires two supply pipes—one of which may lead from the street main, or supply gas pipe, gasometer, or oil or gas reservoir, or other source, as the case may be, and the other is the pipe through which the pure oxygen or non-inflammable gas is to pass, or be conducted to the jet or burner ; this pipe may lead from any suitable apparatus for obtaining the pure oxygen, or from a reservoir containing that gas.

The oxygen pipe is connected by an air-tight joint to the lower part of the burner or jet, and passes up the interior thereof, its mouth or exit aperture being on a level, or a little below that of the burner or surrounding flame.

Both of the pipes where inflammable gas is used, must be furnished with stop-cocks, to cut off or supply the two distinct gases. These cocks may be placed separately on the pipes, or they may be constructed with one plug, serving for both pipes, they being connected by air-tight joints to the separate channels. The plug has two apertures bored through it, which are opposite, and answer to the separate channels. The bore of these apertures should be somewhat smaller than that of the passages to allow for wear in the parts, or tightening down the plug.

It is desirable that the apertures should be so arranged, that the oxygen may be let on a little before the inflammable gas, and also shut off a little after it. This is readily done by making the apertures for the inflammable gas, a little smaller than the other.

It is requisite, in order to carry this part of our improvements into proper effect, that the two streams or jets of inflammable gas, and pure oxygen or supporter, should flow in proper proportions one to the other, therefore the pipes or channels must be furnished with regulating cocks, or set screw plugs, by which their capacity, or the passage of the gasses through them, shall be determined.

When oil, in the fluid state, is consumed for illumination, the pipe or tube, leading from the oil reservoir of the lamp to the circular channel within the burner, is made like a common Argand burner, excepting that the bottom part is closed, so that *no* atmospheric air can pass up the interior. The burner is furnished with a cotton wick, and means of adjusting its height, together with a gallery to hold a glass chimney, as usual. The pipe is for the passage of the pure oxygen from the reservoir to the flame, is passed through the side of the burner by an air-tight joint, and extends upwards within the burner. The

oxygen pipe is furnished with a regulator, plug, stop-cock, and
also with the other cock for the purpose of giving the supply of
oxygen to the lamp when it is once lighted, and cutting off the
same when it is to be put out. A moveable cup is screwed air-
tight to the bottom of the burner, which serves for the purpose
of stopping up the end of the middle channel, and receiving any
deposit from the oil or flame.

The pipes and the cutting off and supply cocks which, in this
instance, have their plugs separate from one another, are turned
simultaneously by one handle, the end of one of the plugs being
keyed or counter-sunk into a mortice in the other, so that they
must move together, although they may be set up or tightened
separately, as may be required.

The second part of our invention contains modifications of
our improved apparatus for producing flashing or intermittent
lights for signal purposes.

There are two ways of obtaining or producing this effect; the
one is by the *simple action* of bubbles or globules of gases pass-
ing through liquids; that is, of inflammable gas, when this
alone is used to cause the light, and also of the pure oxygen
when the flame of other matter is used in conjunction therewith,
the bubbles of either the inflammable or non-inflammable gases
passing from the supply pipe through an inverted syphon or
chamber, containing a column of liquid, which, acting as an
hydraulic valve, (the column being displaced before the bubble
can pass,) interrupts the passage of the gases through the pipe,
and produces pulsation at the burner. The *other* method is to
obtain the same effect by mechanical means, by alternately ex-
posing and hiding, or nearly shutting out the light. The effect
of this mechanical operation may be obtained by placing a re-
volving or moving shade around the flame of the lamp, which
shade shall hide the light, except at a part desired, say all but
through an aperture in the side of the shade.* This aperture
being furnished with a reflector to throw the light to a distance
and as the shade and reflector revolve around the flame, it
have the appearance or effect of a flashing or intermitting light
to any person placed before or behind it; or the same effect
may be produced by alternately opening and shutting the door
of a darkened lanthorn containing the light.   •

Another arrangement of mechanism for producing the same effect,
is by means of a supply and cutting-off cock or valve, placed in the
gas pipe when the inflammable gases are used alone, or the oxygen
when used with the flame of other bodies; which cock is to be al-
ternately opened and closed by some suitable connection with the
machinery, and by thus alternately supplying to, and cutting off the
inflammable gas, or the oxygen, as the case may be, from the flame,
produce a flashing, interrupted, or intermitting light.

When the inflammable gas is used for this purpose, there must be a small continuous flame in such position that the bubbles of inflammable gas, as they come in contact with it, will catch fire, and the flashing-light thereby be produced ; and when pure oxygen is used, the flame of the oil or other matters being kept continually alight, its intensity will be increased or diminished as the oxygen is supplied thereto, or cut off therefrom.

One arrangement or construction of apparatus, whereby the method of causing the intermitting or flashing light, by interrupting the passage of the gas or oxygen from the reservoir to the burner or flame, is effected, by means of a column of fluid being placed in its way. A gas pipe is connected, air-tight, to the closed chamber or well containing a given quantity of water or oil ; a pipe is carried down to near the bottom of the chamber, where its end is turned up, and opens into the inner tube, which serves as a guide for the bubbles as they arise, and determines the time of pulsation, or passage of the bubbles, by its area ; or the same may be determined by the height of the column of fluid.

It will be evident, that if the pressure of gases in the reservoir is properly regulated according to the height of the column of water in the chamber, (or vice versa,) the gases will only be able to escape from the descending pipe through the chamber to the burner in bubbles, or only at intervals, or whenever the pressure of the gases has overcome the weight of the column of water, and forced it out of the descending pipe into the chamber, and thus allowing the escape of the gases to the burner only at intervals. There is a pipe, furnished with a funnel and stop-cock, by which liquid can be introduced into the chamber, and a pipe and cock whereby it can be withdrawn.

This interruption of the flow of the gases can be obtained by several other modifications of apparatus, wherein water or oil is opposed to the direct or continuous flow of the gases, the fluid having to be displaced by the force of the gas before it can pass—the water or oil returning after each portion of gas has effected its escape ; therefore it will not be necessary for us to describe all such modifications of the simple apparatus ; but we will proceed to describe one or two modifications, or arrangements, or constructions of the mechanical means to be employed to produce this effect.

Another modification of apparatus, consists of a revolving shade or reflector, placed around the flame of the lamp, which will prevent the transmission of light, excepting through the face thereof ; and as this reflector turns round the stationary light, it will throw the rays of light in different directions, and by being enclosed in a dark lantern, with only one face or part open, will have the appearance of an interrupted or flashing light to any person stationed before or behind it. The jet or burner, is fixed in any convenient situation, and is furnished

with all the requisite pipes and tubes for oil or gases ; a hood or
reflector is fastened to the tube which surrounds the burner, and
rests on a shoulder or ledge formed upon it. Rotary motion is
to be given to this tube and reflector by any convenient me-
chanism connected with the machinery, as by a band passed
from a pulley, or any rotary part of the engine or machinery.

The same effect of flashing or intermitting light, may be
produced by alternately opening and closing a door or shutter
in a dark lanthorn, enclosing the flame, and may be done by a
suitable machine, having an alternate motion from any part of
the machinery.

Another mode is to cause the intermitting or flashing to be
produced in a stationary lanthorn and continuous light, but in
which the supply cock of the oxygen pipe is alternately opened
and closed, whereby the intensity of the flame is increased or
diminished, as the admission of the oxygen is allowed to pass
to, or is cut off from the flame.

An oil lamp, adapted for this purpose, may be furnished with
a moveable reservoir of oil and cotton wick burner in the
ordinary manner, and a pipe or channel for the pure oxygen,
leading from a reservoir to the top of the burner into the interior
of the flame. The part of this pipe, between the lamp and the
cock, may be made either of metal or flexible material, as cir-
cumstances may require. A supply and cutting off cock or
valve, in this instance, is intended to have a rotary or interrupted
rotary motion given to it by means of the pulley on its plug ;
but a common slide-valve or cock, alternately opening and
closing by means of a reciprocating or alternating motion,
obtained from the machinery, may be placed in the same
situation, and will produce the same effect. A lamp or lanthorn
may he fixed in any required position, and when flexible tubes
are used, may be made to take on and off its fittings when
required. The seat or bed of the cock or valve is to be fixed
in any convenient situation on the locomotive engine or vessel,
and a rotary or alternating rotary motion given to it, by means
of a band or strap passed to a pulley, from any revolving part
of the machinery, or by a toothed rack connected with, and
actuated by, any part of the machinery, which has a regular
alternating or reciprocating motion ; or the same effect may be
produced by an eccentric, connected by a rod to a crank, all of
which modifications or arrangements are so well understood,
that it is not necessary for us to describe them.

Having now described and ascertained our improvements,
and the manner of carrying the same into effect, we wish it to
be understood, that we do not intend to confine ourselves to the
precise forms, or arrangements of construction of apparatus or
mechanism, herein shown, as the same may be varied to suit
different circumstances ; and we claim as our invention, secured

to us by the above in part recited letters patent, as " our im-
provements in apparatus for producing and distributing light,"
first, the arrangement and construction of pipes or tubes con-
nected with jets or burners, and furnished with suitable stop-
cocks or valves, whereby a jet or stream of pure oxygen is
administered or given to the interior of the flame of either oil-
wick or inflammable gas lamps ; and, in the second place, we
claim, as our improvements in apparatus for producing and dis-
tributing light, the improved arrangement and construction of
apparatus or mechanism, whereby we are enabled to produce an
intermitting, or interrupted, or flashing light, to be used as a
signal light for railway, telegraphic, and navigation purposes,
either by passing the inflammable gas in bubbles, or when it is
used in connection with a small fixed continuous light, or the
pure oxygen when used in the interior of flame, obtained from
the combustion of other matters, the pressure of the gas over-
coming a column of fluid, and thereby causing pulsation or
passing of bubbles, before it can escape to the burner or flame ;
and also the improved apparatus or mechanism, whereby we
obtain the same effect of intercepting the passage of the gas,
either inflammable or non-inflammable, to the fixed or con-
tinuous flame by alternately opening and closing the valves,
cocks, or taps of the gas pipes, and thereby causing an inter-
mitting or interrupted light ; and also the improved apparatus
or mechanism, whereby we obtain the same effect, by the
revolving or moving shade or reflector surrounding the light, as
hereinbefore described.

ROYAL VICTORIA GALLERY OF PRACTICAL SCIENCE,
MANCHESTER.

CONVERSAZIONE, MARCH 18TH, 1841.

ALFRED BINYON. Esq., in the Chair.

On Warming Buildings by Hot Water.

The subject of this evening's conversazione, was the joint
report of Mr. John Davis, M.W.S., Lecturer at the Medical
School, Pine-street, &c., and Mr. G. V. Rider, Surveyor to
the Manchester Fire Assurance Company, to a Committee of
the Directors of that Company, appointed " to enquire into

the nature of the accidents that have recently occurred from the use of hot water apparatus in buildings, and to report thereon."

After a few preliminary observations Mr. Dávies proceeded to read the following report:—

Report on Mr. Perkins's System of Warming Buildings by means of Hot Water. By John Davies and George Vardon Ryder.

Associated in the observations and experiments, we have presumed that we might, with advantage, present a joint report.

Before we proceed to detail the experiments which we have made, we shall briefly describe the appearances observed, and the information obtained at a few of the principal places which have been visited. We shall then be enabled not only to confirm but to extend the statements in Mr. Ryder's first report. (See appendix A).

It has been found, on inspection, that Birch Chapel has, at various times, since the occurrence alluded to in the former report, sustained much damage. Wood, matting, and cushions have, in a variety of places contiguous to the hot water pipes, been charred to an alarming extent.

With respect to Mr. Barbour's warehouse, farther inquiry has fully corroborated the previous statements of its having been on fire, close to the pipes, at different times and in different places.

Of the Unitarian Chapel, in Strangeways, the Directors are already in possession of information from both Mr. Ryder (see Appendix A) and Mr. Rawsthorne, (see Appendix B); and this information seems to leave no doubt as to the injury which has resulted from the use of Mr. Perkins's hot water apparatus.

The heat in the Natural History Museum having been repeatedly stated to vary in different parts of the pipes, and to become, in some cases, the greatest at places remote from the furnace, the fact has been confirmed by our own observations and by our subsequent experiments. As this circumstance has excited much interest, and been generally questioned, we shall presently endeavour to assign the cause.

The apparatus, which it may be proper to notice in reference to its general form and construction, consists simply of a long, endless iron tube, carried, in different directions, from a furnace to which it returns, and in which about one-sixth of the whole length is inserted and formed into a coil, so as to be sufficiently exposed to the action of the fire. The tube is, at the commencement, filled, or nearly filled, with water, which, by the application of the heat, soon begins to circulate, and, in that way, to impart an increase of temperature to the apartments which it traverses. The

dimensions of the pipes are such that, on the average, eleven feet in length will contain one pint of water. Connected with the principal pipe are two others, seen in the diagram, which are opened by a screw, one to allow for the ultimate expansion, and both subservient to the introduction of water.

As far as lay in our power, we have made such experiments as occurred to us, repeatedly, and under every variety of circumstance.

Not having any instruments which would furnish speedy and adequate criteria for the determination of high temperatures, we have resorted to the inflammation of combustible bodies, and the fusion of others, depending on ·the recent and high authority of Professor Graham for the degrees which they indicated.

In the Natural History Museum we applied our tests, but were enabled to do so only to a very limited and unsatisfactory extent. Mr. Walker accompanied us to the establishment of Messrs. Vernon and Company, engravers, where we had the opportunity of trying the system rather better, but still imperfectly. Finally, Mr. Walker acceded to our request to have put up, on his own premises, a suitable apparatus, which was to be submitted entirely to our control. It consisted of an iron pipe upwards of 140 feet in ·length, 26 of which were coiled in the furnace, 20, at least being freely exposed to the full action of the fire.

In addition to the apparatus, as at first fitted up, we had a branch pipe and a stop cock, which enabled us, by cutting off at pleasure a great portion of the circulation, to perform our experiments on a contracted scale, and under a variety of modifications.

Mr. Walker, being from home at the time, placed his Foreman entirely under our directions, so that we had the opportunity of pursuing the investigation to any extent which we might think proper. It is but justice to state that this person rendered, very willingly and with much practical skill, all the assistance which was required.

The apparatus having, on Friday the 5th instant, been fitted up, and found, on trial, to be in proper condition, the experiments were commenced on the following morning, at ten o'clock, when the apparatus had arrived at a suitable state.

I. *First class of experiments, viz., those made with the whole length.*

1. The pipe from the furnace became very soon sufficiently hot to singe and destroy small feathers resting upon it.

2. Speedily afterwards, the same pipe exploded gunpowder.

3. On the highest pipe, within a foot of the expansion pipe, bismuth was readily melted, denoting a temperature exceeding

4. Feathers were singed instantly, and matches lighted at the same place.

5. Gunpowder inflamed readily in various parts of the flow pipe, and on the expansion pipe.

6. Blocks of wood, of five different species, were charred : from the deal wood the turpentine issued profusely.

7. Other combustible materials were also severally much charred.

II. *Class of experiments, with the shorter circulation. By this change a greater pressure was immediately observable, as the expansion pipe and several of the joints emitted steam, and admitted the escape of water.*

1. Cane shavings, on the pipe above the furnace, readily inflamed.

2. Lead melted at the same place ; and the temperature must, therefore, have exceeded 612°

3. Different wood shavings inflamed on the upper pipe.

4. Cotton ignited freely at the same place.

5. Matting inflamed at the same place.

6. Cotton, hemp, and flocculent matter, collected from Mr. Schunck's fustian room, ignited on the returning vertical pipe.

7. The blocks of wood, tied to different parts of the tube, were much acted upon and charred in a very short time.

Observing the expansion pipe to be in a state of considerable agitation, and warned of an explosion, the temperature was reduced, and the experiments were, for the time, suspended.

The pipes having, before three o'clock, been refilled and screwed up, for the express purpose of an explosion, the following experiments were made in the progress of the preparation.—

1. Mungeet was readily ignited.

2. Different sorts of paper and pack thread were destroyed.

3. Bismuth fused instantly.

4. Cotton inflamed.

5. Sheep's wool became speedily charred.

6. At five o'clock, the sheet lead, affixed to the upright pipe, freely melted, steam issued violently from the bend in one of the upper horizontal pipes, and, in three minutes afterwards, the explosion occurred in the furnace pipe, at the top of the seventh coil, which presented, on subsequent examination, a lateral aperture about two inches long, and about one-sixteenth of an inch broad.

In the lapse of two or three minutes after the commencement of the explosion, the furnace was entirely emptied of its contents,

which were propelled, in a divergent direction like one mass of fire, so as almost to fill the apartment. The force with which the ignited embers rebounded from the opposite wall, and other obstructions,, occasioned them to scatter in profusion like a shower of fire over every part of the place. The noise was so great as to bring to the spot a multitude of people from the adjoining streets. A number of articles in the shop, as, for example, packing cloth, paper, and hemp, were subsequently found to be on fire in different parts of the premises.

These appearances, and their immediate effect, seem to have been precisely similar to those which are said to have been witnessed at the explosion in the warehouse of Messrs. Crafts and Stell, and would, evidently, have been adequate, in the same situation, to produce all the consequences.

It may be here observed that the experiments clearly prove' that the heat, in different parts of the pipe, is not uniform. Generally it is greatest at the highest elevation, where its superior temperature appears to be of the longest duration under ordinary incidental changes. At the commencement of the operation, however, and a short time after fresh fuel had been applied, the temperature was highest in the flow-pipe contiguous to the furnace. Another circumstance, likely to produce an inequality of heat, may be adverted to : the tubes are far from being of uniform internal diameter ; the consequence of which must be, that as the same quantity of water has to pass, in the same time, through every part of the apparatus, the liquid must move with greater velocity at one place than another, and thus, from obvious causes, develope a greater quantity of caloric. The difference is sometimes so great in the relative bores of the tubes employed, that in some which were examined, one tube had an internal diameter of 9-16ths, and another of $\frac{3}{4}$ths of an inch, that is to say, in the ratio of 3 to 4 : or, taking the relative areas or sections of the tubes, which represent the relative quantities of fluid contained in a given length, in the proportion of 9 to 16. Thus, taking the velocity reciprocally as the section of the pipe, the velocity of the water at one part of the apparatus being represented by 16 feet, the velocity in another part would be 9, or the rapidity of the current would be at one place nearly double that which it was at another.

It is stated, in a work recommending the Hot Water system, that "the application of heat fills" the ascending or flow-pipe " with minute bubbles of steam which rise rapidly to the upper part of the tube, and become there condensed into water again : now, as condensed steam, wherever it occurs, produces about seven times as much heat as the same quantity of water at the same temperature, we have, at once, a reason for the heat of the pipe being generally greater at a distance from the

furnace than contiguous to it. This apparent anomaly, which has been repeatedly observed and denied, admits, therefore, of an easy explanation.

The explosion may, under different circumstances, occur from various causes.

1. As water expands in bulk about five per cent. from 40°, its point of greatest density, to 212°, the boiling point, the expansion must be very considerably more when raised to high temperatures. If, therefore, the pipes be nearly filled with water, and the expansion pipe not adequate or in proper condition, an explosion must be inevitable.

2. The conversion of the water into vapour, producing an expansion which is in the proportion of a pint of water changed into 216 gallons of steam, "with a mechanical force sufficient to raise a weight of 37 tons a foot high," must present a pressure upon the tubes sufficient to ensure their destruction.

3. It has been observed, as an ordinary occurrence, by those accustomed to the apparatus, that, in some cases, a quantity of gas is generated, and has been found to escape, in considerable quantity, when an aperture is made in the upper part of the pipes. The only gases which could be thus obtained are the elements of the water, oxygen and hydrogen. The former would, probably, be taken up in the oxydation of the metal. Now the hydrogen gas, which would remain, has never been deprived of its elasticity, and never made to change its state, by any compressing force hitherto applied. It is obvious, therefore, that inevitable danger must arise from its production.

4. The last source of explosion to which it is necessary to refer, arises from any casual impediment in the pipes; and it is freely admitted that in frosty weather such an impediment is likely to occur: it has been found to result from other causes, as in the case of extraneous matter accidentally getting into the pipes, an example of which was recently presented in the establishment of Messrs. Wood and Westheads.

In a very obliging letter received, in the course of the investigation, from Sir Robt. Smirke, it is stated that, though he has "never seen the pipes heated sufficiently to ignite wood, except on one occasion," yet, "if a fire is incautiously made when there is a stoppage in the pipes from frost or other accidental cause, the pipe within the furnace may be burst or made red hot near the furnace. I have known the pipe," he adds, "so heated only in one instance, when the red heat extended to a distance of upwards of 12 feet from the furnace."

Sir Robert concludes his letter by suggesting a protective modification of the apparatus. "Therefore," he observes, "to

prevent the risk of fire to a building, I would never place the furnace in a room or cellar that is not fire proof, nor would I have the pipes in any part of their circuit in *actual contact* with wood or other combustible material. Security," he continues, "is still more effectually attained by having a safety-valve upon the pipe near the furnace, by which explosion or excess of heat would be prevented."

That which has happened once, may, under the same circumstances, happen again. The exclusion from *actual contact* with combustible materials, could it be permanently ensured, would, when the red heat extended along the pipe upwards of twelve feet, afford, at least, very reasonable grounds for apprehension.

On this system of warming buildings, therefore, danger must be produced from either negligence in the feeding of the furnace, or any stoppage in the pipes : the former evil may be obviated by proper precautions ; but the latter, occurring unexpectedly, exists unobserved, and precaution and care must be equally unavailing.

    Signed,   JOHN DAVIES,
                        GEORGE VARDON RYDER.

· *10th March,* 1841.

•   APPENDIX A.

                        Manchester, February 11, 1841.

TO THE DIRECTORS OF THE MANCHESTER ASSURANCE COMPANY.

    GENTLEMEN,—Having been requested to state, in writing, the substance of certain inquiries which I have been led to make into the subject of fire, as occasioned by the use of Mr. Walker's patent process, of heating apartments, by means of hot water pipes, I beg respectfully now to lay before you the following report:—

    The first instance in which my attention was aroused to this danger, was at Birch Chapel, Rusholme, on Sunday, the 27th of December last; the floor, which is of oak, had been during the day, on fire, at the distance of several yards from the furnace, which is in a shed outside the Chapel; and I then expressed my conviction, that it had been caused by the hot water pipe which rested on the floor of the pews ; and I believe the churchwardens have since been of the same opinion, though the more important question, how the pipe became so heated, still remains matter of doubt.

•

The next place to which my attention was drawn was Messrs. Barbour and Brothers' warehouse in Portland-street. This was on Tuesday, the 9th inst., when I heard that it had been on fire, on the previous day, from some irregularity of their hot-water apparatus; and in confirmation of this report, I found that the warehouse had been on fire in not less than four different places at one and the same time, and in such parts as led me to the conclusion, that the hot-water pipes had unquestionably been the cause. On a personal inspection of the warehouse, and on examining the different persons employed therein, as well as Mr. Walker's superintendant, (Mr. Walker being himself out of town,) I ascertained that the furnace wherein the coil is heated, is fixed in the cellar, from whence the heated water ascends, by what is called " the flow pipe," perpendicularly through the next, or first story, into the second, where, after travelling to the expansion pipe, it flows on by what is called " the return pipe," (though in reality the same,) round nearly two sides of the room, and descends into the first story, where, after travelling about the same distance, it ultimately returns into the cellar, to the bottom of the coil in the furnace, where, being heated as before, it rises again, and takes the course above described. All this is supposing, at least, that the apparatus is acting correctly, and as intended.

I had, yesterday, occasion to inspect a stove in George-street Chapel, Strangeways, where I found that hot-water pipes had been in use, and on a close examination, I found the floor, in several instances, charred black; but how long ago this may have been done, I could not ascertain; and the pipe having burst, some time back, the apparatus has not since been used.

Understanding from Mr. Morton, that there had been some alarm created at the Natural History Society's Rooms, in Peter-street, I this morning visited that building, and found, that on Monday last, the matting on the floor of one of the rooms in the third story, had been so charred by the hot-water pipe as to require to have a portion cut off. This charred part Mr. Morton now has in his possession. The floor, also, at the same place, appears to have been scorched, but so far at least, as I was able to examine the rooms, I did not find any other similar effect. The two furnaces in this building are in the cellar, and I understand, that on the pipes being afterwards opened by Mr. Walker's men, a considerable quantity of strong gas was emitted; and the water, when pumped out, was very black, but bore no indication of the pipe being rusted. Mr Walker's manager says, that the gas found in these water pipes, will readily ignite, if a light be applied to the aperture through which the gas as emitted.

Being myself unable to account for these fires occurring at so great a distance from the furnace, as was the case at Mr. Barbour's warehouse, above alluded to, I asked Mr. Walker's manager for some explanation, but when questioned yesterday, on the subject, he seemed unable to account, satisfactorily, for the peculiar man-

ner in which these different fires had been caused. He, however, distinctly stated, that he has, in company with Mr. Walker, seen experiments tried at the works, upon pipes of this description, when they found, that the pipes may easily be made, if not of a red heat, at least sufficiently hot, to ignite any combustible matter that may be brought into contact with them, and that this effect may be produced, simply, by putting an additional quantity of fuel into the furnace.

The only conclusion which I have yet come to, under all these circumstances, is that the pipes have been so overheated by their contents, as to cause ignition to the combustible materials surrounding them, but how this actually takes place I do not attempt to explain.

I remain, Gentlemen,

Your very obedient Servant,

G. V. RYDER.

APPENDIX B.

Report, and Replies to Questions put by Mr. John Davies, in reference to the Hot Water Pipes lately removed from Strangeways Chapel.

QUESTION.	ANSWER.
1. When applied ?	In 1839, from Walker.
2. How long used ?	Barely two years.
3. Inconvenience experienced?	Deficiency of Heat, great consumption of Fuel, and offensive scent when heated.
4. Reasons for discontinuing them ?	Want of sufficient Heat, the bursting of the Pipes, and the danger apprehended.
5. Appearances when the pipes were removed?	The Floor underneath the pipes much charred, in some places nearly burnt through.

OBSERVATIONS.

These pipes were upon the high pressure plan, and were found very unmanageable ; the offensive scent of the combustion of wood occurred to such a degree as to cause the congregation much annoyance ; causing many to cough, &c. The floor appeared to be *most burnt* where the joints occurred. At present the Committee are trying two of Arnott's stoves.

The Chapel-keeper will show Mr. Davies, if required, the effect produced from using the pipes; the charred appearance is general, and the Committee think that the Chapel has had a fortunate escape from being burnt down.

<div align="center">

JOHN RAWSTHORNE,

</div>

1, *Claremont Terrace, Strangeways.*

Feb. 27th, 1841.

When Mr. Davies had concluded reading the joint report of himself and Mr. Ryder, with some additional observations of his own, the Chairman said, the meeting was much indebted to those gentlemen for their highly interesting, important, and valuable report, and for the series of experiments detailed by Mr. Davies, which were full of highly interesting matter. In answer to a question, Mr. Davies stated, that the internal diameter of the pipe with which the experiments had been tried, was half an inch.

Dr. Black asked if Mr. Davies had ascertained that water was really decomposed in any of the experiments?

Mr. Davies said, he should have much liked to ascertain that; but, though no opportunity was afforded him of doing so, he had no doubt of the fact, passing as the water did through nearly red-hot iron pipe. It was a common lecture-table experiment to show the decomposition of water, and the formation of hydrogen, by passing it over red-hot iron.

A Gentleman asked if the pieces of grooved wood on the table were grooved before they were applied to the pipes?

Mr. Davies said, they were slightly, in order to fasten them to the pipe; but the grooving had been considerably deepened by the charring.

The Gentleman asked if the distances of these pieces from the furnaces had been noted?

Mr. Ryder: One was about 12 feet; the other about 70 feet from the furnace; the total length of the piping being about 140 feet.

Mr. Hopkins wished to ask whether steam was formed in these experiments, and, if so, to what extent. It was known that the formation of steam was checked by pressure, and there would be great pressure in the force of water, which would tend to prevent the formation of steam.

Mr. Davies said, that the pipes were never completely filled with water, a certain allowance being made for their expansion;

and if that allowance were not made, an explosion would occur; and this allowance permitted steam to be generated. That steam was actually generated, they had a good opportunity of judging; for, near the close of the experiments, steam issued very clearly from various joints in the pipes.

Mr. Hopkins said, that steam would be formed wherever there was an aperture, with the water at so high a temperature, on its communicating with the external atmosphere, was clear; but the formation of steam within the pipe, supposing it to be entirely filled, would he presumed be very slow. It appeared to him desirable that that point should be considered, because the probability was, that, if steam were formed in the upper part of the pipe, the explosion would take place there, and not in or near the furnace.

Mr. Davies said, that under the condition Mr. Hopkins supposed, the pipe being entirely filled, there was not a possibility of steam being produced. Of this there was a beautiful illustration in Pappin's digester, in which water might be rendered absolutely red hot, and no steam was generated during the time.

Mr. P. Clare apprehended, that, if the pipes were not completely full, steam would exist under any circumstances, and at almost any temperature. The actual quantity of steam remaining in the pipes would be diminished by the expansion of the water contracting the space which the steam might occupy; therefore, he thought it hardly safe to go upon the presumption, that no steam would be generated under the circumstances in which these pipes had been placed. With respect to the effect produced by these small pipes containing water being heated to a high temperature, to which the public attention had not been called till lately, he might mention, that, as long ago as April, 1823, in the Annals of Philosophy, was published a statement of the manner in which Mr. Perkins heated the boiler which he used as his reservoir, for the water which he heated, to a high temperature; in which article it was stated, that he went upon the presumption, that water might be heated till it was red hot. Now, although no experiments that he (Mr. Clare) was aware of, had been made to prove that water ever had been made red hot, there were cases in which the pipes had been filled with the water, and the coil in the furnace had been heated red hot. In that case, the decomposition of water would take place; the hydrogen would be set at liberty, while the oxygen would unite with the iron pipe; and, in that case it was easy to conceive how hydrogen gas might be generated in the pipes, and, when they were opened, it would escape rapidly. It appeared, from the report, that the joints of the pipes were liable at times to allow steam to pass out. He apprehended that would be from the water, which, on being

exposed at a high temperature to the air, immediately formed steam, and was seen passing off from the joints. When that took place, the space which that water occupied in the pipes, would either be supplied by the expansion of the water remaining in them, or by the generation of hydrogen from the decomposition of the water ; or, it might be, that steam might exist. As to the danger of fire from high temperature in the pipes, he should have been glad if there had been any account of experiments to show at what degree of temperature wood became charred ; wood itself, when perfectly dried, being nothing but two atoms of water combined with three atoms of charcoal. If, therefore, wood were exposed to a moderately high temperature for a considerable time, it was very probable that these two atoms of water would be driven off, and nothing but the charcoal would remain. Now, charcoal was subject to spontaneous combustion ; and it was a question with him, whether a much lower temperature than is ordinarily required, if applied for a considerable time to charcoal, would not set it on fire. It became, therefore, a matter of considerable importance, in passing these pipes along buildings, that they should not be brought into contact with any combustible materials ; for the same reasoning might apply to any other combustible material, as well as to wood. If Mr. Davies was prepared to show at what temperature charcoal would take fire, he would greatly contribute to benefit the public by the communication.

Mr. Davies was sorry he had not with him a table shewing at what temperature wood became charred ; it was not a very high temperature, but the exact degree had escaped him.

Mr. Richard Roberts of the firm of Sharp, Roberts, & Co.) said, he had very frequently observed that wood became charred at a very low temperature, as he had seen in connection with low-pressure steam engines, at say 5℔ to 6℔ pressure to the square inch, at a temperature of not more than perhaps 212°.

Mr. Hopkins asked whether it did not require a considerable time to effect that charring ?

Mr. Roberts.—Yes.

Mr. Hopkins.—That corroborated Mr. Clare's statement, that it was probably a drying process.

Mr. Clare said, he did not allude to a drying process, but to the state of wood after it had become perfectly dry.

Mr. Hopkins asked if Mr. Clare meant that, on the two atoms of water being driven off, combustion took place ?

Mr. Clare said he merely offered it as a suggestion as probable that it would ignite after long exposure in that state to a moderate degree of heat.

Mr. P.H.Holland apprehended that what Mr. Clare had said, referred, not to drying. but to the supposition that even a moderately high temperature would be sufficient to effect the decomposition of the elements of which wood was composed. The question immediately before the meeting was, at what heat would the charred wood ignite. A temperature not very much less than red hot would be required to ignite the charcoal so as to make it burn, but that would not be exceedingly dangerous ; and a still higher temperature would be required to make it burst into active flame; and he thought there was no instance mentioned, in the reports of any dangerous fire, resulting from heat communicated from the pipes themselves.

Mr. Davies said, that he and Mr. Ryder had found, in the warehouse of Messrs. Barbour, Brothers, that thick pieces of wood, the legs of large tables, contiguous to the pipes, but very far from the furnace, had been set on fire from the pipes, and pieces of smaller magnitude were found to be on fire at different times.

Mr. Holland said, that besides the difference of temperature in the pipes, resulting from the distance of the furnace, there would appear to be other differences resulting from the internal diameter of the pipes.

Mr. Davies.—That is the speculative part of the paper.

Mr. Holland.—This, as a matter of speculation, he thought, was much more interesting than many other points. He had made the same supposition as the reporters, that it depended upon the different diameter of the internal parts of the pipes; but, while they attributed it to the greater velocity of the liquid at one place than another, he thought it was owing to the thinness of the metal in some parts, as compared with others.

Mr. Davies.—The metal is nearly uniform in thickness.

Mr. Holland.—That could hardly be, if, as was stated, the external diameter was about an inch, and the internal diameter, in some cases, 9-16ths, in others, 12-16ths, or three quarters of an inch. In some cases, then, the thickness of the metal would not be more than 4-16ths, or a quarter of an inch ; and if the water in this exceedingly thin piece of metal were as hot as melted lead, it followed, of course, that the external temperature of the metal there would be considerably hotter than where it was 9-16ths. Now iron was a bad conductor of heat. Every one acquainted with steam boilers knew the great difference of temperature between the external surfaces of the metal, and that internal surface next the water; and it was always considered desirable, on this account, to reduce the thickness of steam boiler plates exposed to the fire, as much as was consistent with safety. He thought, therefore, it was rather from the difference of temperature in the metal itself, from variation in thickness,

than in the water itself, that this different temperature took place. It might be thought contrary to this supposition, that the wood was most charred at the joints, where the pipes appeared to be of double thickness; but it was in appearance only, and the fact was that the wood was most charred where the pipes rested on the floor.

Mr. Davies said, in the very ingenious remarks of Mr. Holland, the very datum upon which all his reasoning rested was entirely destitute of foundation, viz. that iron was a bad conductor; for in fact it was an excellent conductor.

Mr. Holland said that iron was known to be a worse conductor than copper, and copper did not conduct so well as moving water. Iron did not conduct through its substance with any thing like the rapidity of copper, and speaking as amongst metals, iron was a bad conductor.

Mr. Davies said, that, as to the conducting power of water, Dr. Prout had drawn the distinction between the conveyance by motion and the conducting throughout the particles of a solid body, calling the latter _conduction_, and the former _convexion_.

Mr. Holland said, that he knew iron was a better conductor than wood; but he would appeal to those who had made or repaired locomotive engines, if they did not find that where the copper was thicker, as the heads of rivets, or in thickened double tubes or plates, they were liable to be burnt on the outside, because they became so hot, though the water inside had a pressure of 40lb or 50lb, proving that the copper outside was very much hotter than inside. If this were so, it shewed that copper was a bad conductor; and as it was a better conductor than iron, his assertion that iron was a bad conductor, he thought, was borne out by fact.

Mr. Davies, said, this was fiction against fact, to all intents and purposes. Every one knew the ordinary experiments of the lecture table, where a series of bars of different metals were put with a small piece of phosphorus or wax at the end, and a lighted taper was applied four or five inches from the extremity, on which the phosphorus took fire, or the wax was melted, showing the rapidity of conduction. There was in iron the property of uniform diffusion: and if the internal portion of a pipe twenty inches thick were made red hot, it would very soon be red hot outside.

Mr. William Read would like to know the practical bearings of the subject upon all those buildings in which this mode of heating was adopted; whether it was considered so safe as to allow of insurances being effected upon them, upon principles similar to those before employed: and he would also ask whether this admirable system of heating could be so far deprived of its dangerous tendency as that it could still be emp

ployed in the warming of buildings, without the possibility of those dangerous results to which—which nobody could deny, after the interesting and important experiments of Mr. Davies— it was exceedingly liable. It became a matter of very serious question to all who had now their manufactories, places of business, workshops, and large apartments heated in this manner, whether some such plan as the following might not obviate the difficulties: —If the steps or sleepers in which the pipes were now laid, were composed of iron, or any metallic substance so well capable of conducting heat as iron was, it was clear that the amount of risk was increased by those sleepers ; but if composed of incombustible substances, incapable of conducting heat, as, for instance, some of the stone with which Mr. Davies was doubtless acquainted,•so as to prevent any contact with wood or other combustibles ; and if also such a safety-valve could be used as would be a security for the circulation of the hot fluid within the tube, and to prevent danger from the malignant feelings of persons employed, he thought it became an object of the highest importance for scientific men to provide means by which these great evils might be obviated in every possible way.

Mr. Jeremiah Garnett was afraid, that no such expedients as those suggested by Mr. Read would render this means of warming buildings safe or tolerable ; because, though the pipes did not come in contact with any timber, yet at all times combustible substances were apt to be laid in contact with them. A pipe passing through the rooms of a warehouse filled with goods, might at any time have a pile of goods laid upon or thrown against it, and very great danger would result from that, if these pipes were permitted to be heated to the degree of which it was shown they were susceptible. It seemed to him, that by no means could this class of pipes be rendered safe unless some means were adopted to prevent the temperature from rising to the dangerous point ; and that he thought would be best effected by having one part of the pipe composed of a metal that would melt at a heat below that at which it began to be dangerous. Such part of the pipe might be made of bismuth, or of one of the mixtures of lead, tin, and bismuth, which melt at a very low temperature ; and then it seemed improbable that any danger could arise from it, because, before the series of pipes was heated to the dangerous point, this part would melt, the water would run out of the pipes, and there would be an end of the danger. Without that, or some such precaution, he thought it perfectly clear, both from these experiments and what had been previously observed, that no mode of warming buildings with closed pipes could be adopted by any man having the slightest regard for the safety of his property.

Mr. Read said he had made his observations because this mode had been extensively adopted ; and if, by means of future

experiments by Mr. Perkins and others, any method could be discovered that might at all events greatly lessen the danger or the risk incurred, it would be desirable. He mentioned it with the view that this great system, which had been extensively adopted, might not, without further trial, be abandoned. He thought it too good a principle to be at once totally laid aside, without the best efforts being taken to make it perfectly safe.

Mr. Gooch thought a safety valve would regulate the temperature at once, and parties could put as much pressure upon the valve as they chose ; and thereby have a fixed and certain heat, which could not be exceeded, as, at a certain point, the valve would blow off.

Mr. Davies said he had suggested a safety valve at an early point in the inquiry, as the most natural mode of prevention, and the only one calculated to insure any thing like perfect safety ; but he was told it was utterly impossible ; that contrivances of that kind had been tried, and found utterly unavailable. As to this mode of heating apartments, it appeared to him one of the most beautiful ever devised, and it was only under certain circumstances that danger could be apprehended ; first, when too much heat was applied, because then they had seen by what had occurred here and elsewhere, what was the inevitable result ; and, in the case referred to by Sir Robert Smirke, where the pipe was red hot at a distance of twelve feet from the furnace, it was obviously exceedingly dangerous in any place where there were combustible materials. The suggestion of Mr. Garnett pleased him, because it accorded with his own views. Lead would melt at a temperature below the dangerous point. He proposed, therefore, that one portion of the pipe, at its greatest elevation, should consist of lead : and if it became heated above 600°, the lead would melt, and the dangerous consequences would be wholly obviated. When he suggested this to one individual practically concerned in these affairs, he had scouted it as an idea altogether inadmissible.

Mr. P. Clare said, as to the application of pipes of metal melting at lower temperature, he thought he had heard Mr. Perkins say, nearly 20 years ago, that it was such a mode as he would himself recommend as a protection against bursting. One difficulty he thought existed which he did not see how to overcome, viz.—making use of such small pipes to convey water at a low temperature, would not warm the air and the walls ; and unless water were conveyed through such pipes at a high temperature, it would be wholly ineffectual for the purpose.

Mr. Eaton Hodgkinson asked whether any one had suggested the use of sheathing or a larger thin metallic pipe to surround the small one, which, while the surrounding air would be heated, would probably prevent all danger.

Mr. Davies said there would be this objection, that the water pipe within the larger pipe would be of course enclosed in a volume of air, which was one of the worst conductors, and thus the object of the apparatus would be entirely frustrated.

Mr. E. Hodgkinson said the pipe or sheathing might be perforated.

Mr. Davies said he agreed with Mr. Clare, that in these small pipes a low temperature would not give sufficient heat.

Mr. Garnett thought that was by no means conclusive against the use of this kind of apparatus, because there was a point at which the temperature was actually safe ; and, as experience showed, under ordinary circumstances was sufficient for the purpose of warming.   That temperature was considerably above that of boiling water ; yet it was obvious from many pipes never having been raised to a dangerous height, that, provided it could be prevented being raised beyond the safe point, it might still be raised to a height sufficient for the purpose.   If there were something to allow it to rise to 400°, but to prevent it rising to any thing like 600°, the danger would be prevented.

Mr. Hopkins thought they were met rather to receive information as to what had taken place, than to suggest remedies in this stage of the inquiry.   He was desirous of knowing whether steam was formed in the upper part of the tubes, because it appeared to him that if it formed there, a safety-valve would allow the steam to blow off.

Mr. P. H. Holland asked if Mr. Davies was aware at what temperature water began to be decomposed.   His own opinion was, that it was decomposed at all temperatures above the boiling point, perhaps lower ; but he could not call to mind any experiments.

Mr. Davies said he did not know whether the lowest temperature at which water became decomposed had ever been ascertained.   •

Mr. Garnett asked if Mr. Davies had observed thes tate of the coil shortly before the explosion, as to its appearance with respect to redness ?

Mr. Davies said they had examined the coil repeatedly before they got near that state ; but when the whole became ready for the explosion, and the door of the furnace became red hot, they thought " the better part of valour was discretion."— (Laughter.)   They had never seen the coil really red hot.

Mr. Garnett said he had asked the question, because told by a man at Messrs. Barbour's, that he had seen the coil red hot ; and it was stated also, by a workman of Mr. Walker, that he was satisfied it had been red hot at that warehouse.

Mr. Ryder said he had seen that many of the coils had been red hot ; but whether at the time in question, or some other, he could not say. One important thing they noticed, which Mr. Davies had not alluded to—viz., that these pipes being united by sort of nut, they found in several instances that the heat was decidedly greater on that side the joint where the water flowed, than on the other, within a distance of six or eight inches. The pipe during the joining must have bulged in (which they found had been the case with some of them) and, they found an additional pressure there.

Mr. Garnett asked if they had tried it so far as to say whether that was universally or usually the case ?

Mr. Ryder said they had not, but they found it so in several instances.

Mr. Davies added, that the difference of temperature was very great at very short intervals.

Mr. E. Hodgkinson asked whether the pipe was observed to be as hot on the under side as the top, and whether the charring took place to the greatest extent when the article was laid upon or placed below the pipe.

Mr. Ryder said he had found once or twice, that the pipe would freely ignite gunpowder at the top, and would not underneath.

Mr. Hodgkinson said so he conceived, and it might be doubted whether there was not a small film of steam generated, of extreme density, occupying the top of the pipe.

Mr. Davies said their attention had been more called to the circumstance, because Mr. Walker's foreman had conceived that the under surface was a great deal hotter than the upper. When combustible substances were placed round the pipe, they found that they entered into a state of combustion nearly equally all round.

Dr. Black asked at what temperature lead melts.

Mr. Davies.—About 612°.

Dr. Black.—Then he did not see why these pipes might not be made totally of lead. A heat of 400° would be sufficient to heat any apartments, however large, as he knew, having once fitted up in a dwelling house a pipe from the kitchen fire to heat water, at a height of three stories, for a bath. He placed an iron coil in the kitchen fire; and at six inches from the fire he joined it with lead pipe, and had a current up and down through the kitchen fire. He found the heat sufficient in the bath, perhaps 30 feet above the kitchen fire. He would suggest the adoption of a safety-tube at the highest part of the pipes carried to any height desired, and open to the atmosphere, which would regulate the heat, and with free communication with the atmos-

phere, no injurious pressure could take place. In order to diminish the temperature to the safe point, the coil in the fire need not be made so long or so large, and then the heat would not rise to a degree verging towards explosion. The safety tube would obviate the collection of steam in any part of the pipe, because, being elastic and light, it would ascend to the higher part of the pipes, and escape through the safety tube. He conceived that steam generated in any part of the pipes, usually at the joints, would act as an obstructing body, and prevent the ascent of the water through the pipes if there was no safety tube to afford a vent to the steam or gas. He thought the report so complete, and the experiments so satisfactory, that the public must be satisfied of the dangerous nature of these pipes as at present constructed:

Mr. Davies said, there was this objection to Dr. Black's ingenious plan, that if the pipes were left open, or had free access to the air, they would, in a comparatively short time, become totally emptied, and there would remain no water to circulate. They had found that even the expansion pipe blew off the steam, and assumed an oscillatory motion; and that a joint at the very bottom of the pipes, screwed very tightly, allowed the steam to escape, as it also did from other parts of the pipes. They had reason to believe, before those experiments were completed, that a considerable quantity of water had escaped ; and, if an aperture, however small, allowed the steam to escape, an apparatus such as Dr. Black described, would allow the whole of the water to escape in a few seconds.

Mr. Ryder said, that a few nights ago he took a short length of one of Mr. Walker's pipes, and had it filled with water, and screwed up, with a piece of wood in it, in the form Mr. Walker united all his joints, by a cone ; and he subjected two of these lengths to the heat of a blast furnace. One blew up immediately ; the other was red hot, and remained so for nearly an hour, during which time they did not think it safe to go to it. Then it was cooled ; and, when they came to unscrew it, there was a strong steam issued from it, just during the few minutes it took to unscrew it ; and, when opened, the pipe was found perfectly dry, and the wood was converted into a small quantity of dry charcoal not enough to have filled a child's thimble.

Mr. Hopkins asked, whether a warming apparatus similar to that described (Mr. Perkins's) was not employed by some parties with certain modifications ?

Mr. Jeremiah Garnett said, there were hot-water pipes of much larger diameter, with one pipe open to the air ; but they were on a totally different principle, and it required pipes of a much larger calibre to produce the requisite temperature.

Mr. James Woolley asked, why such a safety tube as that proposed by Dr. Black could not be secured by means of a column of mercury.

Mr. Jeremiah Garnett said, that was only another mode of applying a safety valve.

Mr. Davies said, that a column of mercury of about 30 inches would be required to counterbalance one atmospheric pressure ; but, at one period of their experiments, they had a pressure equal to 75 atmospheres.

Mr. Fothergill said, that Mr. Roberts (of Messrs. Sharp, Roberts, & Co.) had once fitted up two rooms with warming apparatus. He had a "long cast-iron jar of four to six inches diameter internally, with a coil of pipes in the furnace similar to those made by Mr. Walker ; and this apparatus was tried for two successive winters, but the heat from the pipes was not sufficient to warm the two rooms, and from that and other reasons Mr. Roberts removed the pipes, and substituted steam pipes.

Mr. Roberts said, his principal reason for doing so, was, that in the first winter, when the fire was lighted one morning, all the water was blown out of the rising pipes and out of the cistern, as nearly as possible. The return pipes had got frozen. and it was a very hard frost when he had most occasion to use the pipes.

Mr. Alfred Binyon felt compelled to give his reluctant testimony to the accuracy of the experiments which Mr. Davies had detailed : he did so, because he considered the interests involved in this question were most extensive ; and, after a practise of six or seven years in the use of these pipes, he might state, that, under bad management, they were capable of doing very serious injury ; but, under common and ordinary management, and if the parties having the care of the apparatus had proper instruction respecting its use, that danger, he apprehended, might be lessened in a very great degree. With respect to the use of another metal, introduced into part of the circuit, he conceived that that would in many cases prove utterly useless, because when the flow did not take place, he had himself observed, in one portion of the pipe, from ten to twelve feet of the portion of the apparatus nearest the furnace would become red hot, and on one occasion had had a narrow escape from fire, the pipe being too closely in contact with some timber near.   He would read the observations of the watchman on the works at Mayfield, on that occasion, July 29, 1838 :—
" About a quarter to four o'clock on Sunday morning, a fire was discovered in the ceiling or floor over the drug-room, occasioned by the heat of the flue and pipes.   The fires are situated in the drug-room for heating the block-printing rooms

As soon as the fire was discovered, it was extinguished. One of the principal beams of the floor had become ignited: and there is not the least doubt, if the atmosphere had come into contact with the fire, a great conflagration would have been the consequence at Mayfield." He (Mr. Binyon) considered this apparatus presented so many advantages, in reference to its neatness, compactness, and the salubrity of the rooms in which it was used, as to recommend it to attention ; and he should be glad if Mr. Davies would give any idea of the comparative cost of this mode of heating, and of any other. His own opinion was, that it was very economical; and, though there was some failure, he thought we need not be unnecessarily alarmed with respect to this mode of heating buildings.

Mr. Garnett said it was very easy to use a portion of lead piping near the furnace, and that was the position in which he should be disposed to place it ; then it would act as a safety valve either in cases of overheating or of obstruction. He did not precisely know at what temperature gunpowder explodes, or whether Mr. Davies and Mr. Ryder made any observations to show the actual temperature of any part of the pipes at the time they ignited gunpowder. Mr. P. H Holland thought danger might be obviated by careful management and the use of the safety valve ; or, if not by that, by the application of Perkins's beautiful apparatus, the pyrometer. Other dangers had not been alluded to—those to the health of the inmates of buildings. When air was heated by any surfaces to a high temperature, it was deteriorated for the purposes of respiration, especially if the hot surfaces were liable to come in contact with inflammable materials, the decomposition by combustion evolving gaseous products. In open shops or buildings, this inconvenience would not be so great, but in close buildings it was very injurious. Cases had occurred of this character—one in the Custom House, London, and another in a public building in Glasgow, in which very serious injury and loss of health had ensued. In the custom-house, the loss of health to the clerks engaged was distinctly and decidedly traced to the use of stoves, at a temperature not much higher than that of those pipes ; and the only remedy for this was to heat the place with metallic or other surfaces, at a lower temperature than would be injurious to health ; and this could only be done by making the surfaces proportionally large. The plan of Mr. Hodgkinson, of casing the small pipes in larger ones, would effect this object ; but it was perhaps better to have a large pipe alone, and buildings could be sufficiently heated by using large pipes at the common temperature of boiling water ; and, with a feed pipe, open to the air, the temperature might be kept not much higher than the boiling point with perfect safety. They would be somewhat more expensive than the small pipes, and would occupy much more room ; but, though these were inconveniences, he

thought their greater safety (and it would be more difficult to
keep up a sufficiently rapid current through a small pipe than
a large one) more than counterbalanced these, and the conveni-
ences were greater, for they effected the object of warming suffi-
ciently, conveniently, healthfully, and comfortably.   In his
opinion, for rooms in dwellings, the small pipes would never do,
as regarded their effects on health.   .

Mr. A. Bmyon said, he did not recollect any instance in
which any of their workmen, who, for years, worked daily
in seven rooms warmed by this apparatus, had suffered in
health from any injurious influence of the pipes

Mr. Holland said, that might arise from the damp which
prevailed in rooms used for calico printing ; for moisture would
considerably reduce the injurious effects on health.   Besides,
in such works it was necessary to provide means for a charge
of air, in order to the proper drying ; and this would also
protect the workmen from deleterious effects.   But the whole
of the clerks occupying one room in the London Custom heated
by stoves, had been laid up at once, while other clerks under
circumstances in all but this alike, were not ill ; and when
this cause was removed, the clerks in this room were not ill ;
and, therefore, the inference was, that the heating by stoves
made them ill.   A similar case was mentioned by Dr. Ure as
occurring at Glasgow, and he and others engaged traced it to
a similar cause.

Mr. J. H. Stanway said, that in the former case it was not in
consequence of the air becoming deteriorated by the action of
the hot water pipes, that ill health arose ; but it was from the
air passing through a hot copper.   The other case was a fire-
office, under the management of Mr. Beaumont, at the lower
end of Regent-street ["The County Fire Office"], and there
the copper was made red hot.   When Mr. Perkins first made
known his invention, he wished the temperature to be kept
down to 350°, which he thought a safe heat with respect to dan-
ger, and also that it would not interfere with the salubrity of
the atmosphere through which the pipes were taken.   And he
proposed, as one means of reducing the temperature to that
point, that the coil in the furnace should be proportioned to the
length of the pipe ; that no coil should be attached to a pipe
which would allow of its acquiring a higher temperature than
350° when fully heated.

Mr. Davies : one-sixth of the length of the pipe is his pro-
portion for the coil.

Mr. Ryder : the coil used in the experiments was rather less
than one-seventh of the whole length.   '

Mr. Stanway though it should be known that these pipes had
been so fixed as to do a greater amount of duty than was re-

quired. Some parties wished to reduce the expense as much
as possible, and the erector was induced to put as short a length
as possible.

The Chairman said, that as chairman he might be expected
to say a few words, which he did with considerable diffidence,
as the subject was one with which he was not practically fami-
liar. It appeared to him, that pipes of so small a diameter,
heated to so high a temperature, never could be rendered safe.
A safety-valve applied anywhere but at the top, he thought,
would be inoperative ; for, as a matter of course, the pipe
would give way where it was the weakest, and where it was
subjected to the greatest pressure. Now, it was both weakest
and subject to the greatest pressure in the coil ; for there, for
every yard of perpendicular height, there was one pound addi-
tional pressure upon every circle of the coil ; and the pipe would
be the hottest, and therefore weakest in the coil, unless some
weaker point was made by the introduction of some metal that
would melt at a lower temperature. He thought Dr. Black's
remark a just one, that the generation of steam would prove an
obstruction to the upward flow of water ; because the pressure
being less at the top, the steam, which would be kept from
being evolved by the greater pressure below, would accumu-
late at the top, and, having once been evolved, it could not be
pressed downward. He knew, from extensive experience in
water works, that water pumped into the pipes conveyed with
it some air, and that, in all elevated points of the pipes, that air
would accumulate ; and unless it were permitted to escape by
some contrivance, the flow of water could not pass that point,
but had a severe struggle with the air. In the pipes for the
supply of Edinburgh with water, a person was employed to
turn a cock, and to let the air out now and then ; but he (Mr.
Buck) had obviated that by making the tap self-acting. He
constructed a valve on such a principle, that, when in contact
with the water, it kept shut ; but, when the air depressed the
water, the valve opened. Such a contrivance might be applied
to these hot-water pipes. A pipe, ascending a little distance
above the highest point of the circulating pipes, containing a
ball, into which a small quantity of steam might enter, and
allow the water to pass on, might obviate the difficulty ; and it
might be so contrived, that it should not necessarily open,
except under the pressure assigned by the inventor, 350° If,
for the sake of economy, it were desirable to continue this mode
of heating, the introduction of some such contrivance would
render the pipes considerably more safe.

Mr. Hopkins asked if there was any objection to the use of
large pipes but the additional expense. It appeared to him, that
all the objects desired might be attained by making the pipes of

larger diameter ; and, if that was the only difference, it was better to incur additional expense, than run the risk of great danger to property and life, by fire or explosion.

. Mr. Ryder said that Mr. Perkins had made a number of experiments with that object in view,—to see if his pipes could be made larger ; and he found an inch pipe the largest that he could conveniently bend cold for the coil and the different angles.

The Chairman said that, in an excellent practical work of Tredgold's, published seven years ago, he had stated, on the authority of Dr. Ure, that air heated by contact with any substance of a temperature higher than boiling water, was deteriorated for the purposes of respiration ; and his system of heating was by steam. He said that no pipe should be less than three nor greater than six inches diameter. He supposed that Tredgold's beautiful work must be familiar to all engaged in warming public buildings ; if not, he would advise them to read it.

Mr. Ryder said it was necessary, in order to prevent the pipe collapsing, during its curvature into the coils, to bend it cold ; and they found they could not, accomplish that with pipes of greater diameter than one inch ; and it was also found, that, in proportion as they tried larger pipes, it was next to impossible to make the joints perfect; and upon this high pressure principle, all Mr. Perkins's experience had brought him to the conclusion that that was the largest pipe they could use.

Mr. Garnett said, that, at the Blind Asylum, larger pipes were used on a different principle—at a lower temperature, and with less pressure.

Mr. Davies said that the pipes there were about four inches in diameter, and they answered the purpose very well. Both Mr. Ryder and himself highly approved of that mode of heating apartments; they thought it much better than the one under discussion; but they had no idea that other modes were now to be discussed, but that the conversation was to be confined to Mr. Perkins's system.

Mr. Garnett asked whether any experiments had been made to throw light on the actual temperature of the pipes when lead was melted, or gunpowder ignited.

Mr. Davies said lead would melt at 612°.

Mr. Garnett said he was aware of that ; but, while the lead melted was 612°, the pipe melting it might be 1,000° or near it.

Mr. Davies said they had never tried any temperature of the pipes when gunpowder exploded ; it would not be 1,000.°. They got gunpowder decomposed at one temperature, and it

exploded at another. Sulphur would melt at 230°; and probably a few degrees higher would explode gunpowder. In answer to a question by Mr. Hodgkinson, as to whether the lead had been attached to the pipe some time before melting, Mr. Davies said they had portions of lead and bismuth attached to different parts of the pipes; and when they found that these melted, they applied other pieces, and found the metal to melt suddenly afterwards.

Mr. A. Binyon thought that though wood might be charred, it might be doubted whether it would ignite, were it not for the intervention of sweepings, &c., which, becoming ignited, set fire to the wood.

Mr. Davies said that cotton from the factory would have a little oil mixed with it, and hence would have a tendency to spontaneous combustion ; and if its temperature was raised, this process was hastened.

Mr. Binyon added that in like manner a large quantity of turpentine in wood would be a means of its speedily taking fire.

Mr. Davies said that in one case he had seen the turpentine actually boil.

Mr. Ryder said that shavings had blazed freely several times, and even cane took fire.

The Chairman asked if the pipes were kept as low as the temperature recommended by Mr. Perkins [350°], whether they would then be dangerous in warehouses.

Mr. Davies said not more dangerous than any other expedient of a similar kind that could be devised, with this one important exception, that, of the magnitude generally made, the circulation was much more likely to be interrupted than any other.

Mr. Ryder said that in putting a pipe through a floor, minute particles of mortar fell in, and might lodge in sufficient quantity to cause a stoppage, which no foresight could prevent, as in the case of Messrs. Wood and Westhead's warehouse.

Mr. A. Binyon then moved the thanks of the meeting to Messrs. Davies and Ryder for their valuable communication on the subject which had evidently excited the greatest interest. Indeed it was of momentous consequence to the town of Manchester, which employed such an immense quantity of piping on this plan ; and he was sure all present were greatly indebted to these gentlemen—(Applause.)

Dr. Black said he had great pleasure in seconding the motion. It passed unanimously and with applause ; and the proceedings then terminated, shortly after half-past nine o'clock.

Address to the readers of the Annals of Electricity, Magnetism, and Chemistry, &c.

As it is possible that some of the readers of the annals may have seen in the Nautical Magazine, for February; and in the Philosophical Magazine, for March last, Mr. Snow Harris's latest attempts to evade the force of my observations on his electrical experiments, his reasoning, and the marine lighting conductors, which he has had the imprudence to claim as his own original invention; they may have expected that I should have introduced those productions of Mr. Harris's pen to the pages of the annals, and commented on them, as I have hitherto done with all his previous essays on that subject. I, however, have various reasons for not paying any further attention to that kind of argumentation which Mr. Harris appears so eminently calculated to manage; nor to that kind of data which, alone, he seems to be provided with.

It must have been noticed by every reader of Mr. Harris's struggle against the force of the *expose* in my fourth memoir, that he has shown either a natural tendency, or a wil'ul proneness, to employ a style of language not usually current in discussion on scientific subjects; and that he has, in the most direct and unreserved manner, endeavoured to pervert the meaning of vaf ous parts of my statements, for the very obvious purpose of deceiving his own readers; and, it is to be feared, from motives of a very different character to those which either true science or true philanthropy, could in any case form an association.

This mode of proceeding, to the extent that Mr. Harris has carried it, would, alone, have been a sufficient inducement to decline all further controversy with him on that or any other subject; and as Mr. Harris has now made it appear that he is totally ignorant of the existence, and consequently of the effects of lateral *discharges,*—that he attempts to ridicule the advantages to be derived from series of experimental enquiries by electrical kites,—that he confesses he has no idea of electrical waves in the atmosphere.—that he knows of no difference in the electrical condition of a conductor, when carrying a discharge, and when not, and above all, when he proves, by his own mode of reasoning, that he has no knowledge whatever of the *magnetic effects* of electrical discharges through good conductors, and even denies in toto, so well an established fact, it becomes very obvious indeed that he is in a totally unprepared condition to hold any argument on the subject: and, consequently, any further notice of his elaborate effusions, could not be attended with any useful reults whatever.

I believe it will be acknowledged, by all parties, that I have given Mr. Harris every advantage that he could possibly wish for since the controversy first began. I have placed the whole of his efforts before the readers of the *Annals,*

most of which I have transplanted from other journals, which have favoured his views in the most unreserved manner. And, in order that he might have an opportunity of displaying his electrical powers to the fullest extent, and exercise his abilities in support of that plan of conductors to which he attaches so much importance as his own invention, without labouring under the torments of suspected *plagiarism*, I have purposely kept Mr. Cook's claims entirely out of sight. Being very well aware that, as the original plan of Mr. Cook, by having cylindrical conductors and keeping them clear of the masts, are much less dangerous than the metamorphosed shape and position given to them by Mr. Harris; the merits of the invention, whatever they may be, must eventually revert to Mr. Cook. Moreover, as I cannot view any lightning conductors, which pass through the body of the ship, otherwise than dangerous, I had no wish, even now, to associate Mr. Cook's name with the controversy, had I not been requested to do so·

Mr. Harris might possibly, before this time, have had an opportunity of adding some other facts to his stock of information, besides those he has already picked up since the commencement of this controversy, had he treated those I first made known to him in the gentleman-like manner I had a right to expect. These, however, I soon found necessary to keep in reserve, until a more convenient season, and until I had an opportunity of convincing others of their existence and importance.

I have frequently taken sparks, and even violent shocks, from an iron bar, stuck deep into moist ground, whilst that bar was receiving a torrent of electric fluid from the insulated kite string; and also from every part of a long copper wire, in connexion with that bar, laying on moist ground, and the farther extremity formed into a coil and thrown into a pond of water. But as I have made no electric kite experiments since some time before I left my residence at Woolwich, now about three and a half years ago, till the 29th of last month, I have had no opportunity of shewing those facts to any scientific individual beyond my own assistant. On that day, however, I had a kite up, for the first time in Manchester; and although we had a cloudless sky, I was enabled to convince several of our directors, and other scientific gentlemen present, of the existence of lateral discharges from conductors in connexion with the ground. On Thurday, the 6th instant, we had the kite up again; and although the electric action in general was very feeble, yet, on pulling the kite down again, an electric wave caused by an approaching light cloud, produced such a shower of shocks to the whole of four or five persons who had hold of the string, as to make them skip away from it with their best speed. This occurred several times before the kite reachd the ground.

Being, however, in correspondence with Mr. Weekes on the subject, and knowing the facilities, perseverance, and skill of manipulation which that gentleman possesses, I, in the mean time, (as will be seen by his letter, page 446) requested the favour of his making a few experiments, which I then suggested to him ; and he has very kindly undertaken to put my suggestions into operation, the first opportunity that presented itself; and I have only to solicit the attention of the reader to a perusal of Mr. Weekes's paper, to make him acquainted with the result. He will find a description of the most splendid set of experiments that were ever yet recorded on the subject of atmospherical electricity.

It is particularly gratifying to me, to have so many of the the phenomena of atmospherical electric action, which I had so clearly pointed out in my fourth memoir, to be thus so beautifully and strikingly demonstrated upon the magnificent scale which Mr. Weekes has described, and more particularly so, when I find a philosopher of such high standing in this branch of physics entering, without hesitation, and in the most unqualified manner, into the precise views which I had taken, not only of the danger of *lateral discharges* from lightning conductors : but of the effects of *electric waves*, which that gentleman so emphatically describes, and which his late experiments have afforded him such ample opportunities of observing.

The brilliant scintillations from the iron nail in the pump were evidently the effects of *lateral discharges* of the *second kind*, and the sparks and shocks taken from the insulated wire, and from the various vicinal metallic articles, were, as obviously the effects of *lateral discharges* of the *third kind*. (See my Fourth Memoir, vol. iv. p. 174, of these Annals.)

And as Mr. Weekes has referred the whole of these phenomena to the influence of *electric waves*, (for no flash of lightning from a cloud struck any part of the apparatus,) I am justified in concluding that the views which I have taken of the effects of *electrical waves* and *lateral discharges*, are now amply proved and satisfactorily acknowledged, by an authority which no electrician is likely to call in question.

It only remains that I acknowledge the obligations which Mr. Weekes has placed me under, not only for the readiness with which he has undertaken to make those experimental inquiries that I pointed out him ; but for the very handsome and gentlemanly manner in which he has attributed those inquiries to my suggesions.

<div align="center">WILLIAM STURGEON.</div>

Royal Victoria Gallery of Practical Science,
    Manchester.

    May, 1841.

Remarks on the Daguerrotype Process.   By   Dr. DRAPER, Pro-
fessor of Chemistry in the University of New York.

The first object is to expose a surface of pure silver to the
action of vapour of iodine, in order to give rise to a peculiar
iodide of silver, which under certain circumstances is exceed-
ingly sensitive to light.   The action of hyposulphite of soda, or
the process of galvanism, are to free the plate of its sensitive
coating, after the operations are completed.

Iodide of silver turns black in the solar ray, the whole success
of the daguerrotype artist depends on his checking the process
before the change shall have supervened.

The coating of iodide is not *immediately* necessary to the
production of images by the mercurial vapour. The condition
appears to be traceable to the metallic surface. If it be cleansed
of its mercury, polished thoroughly with rotton-stone, and
washed with nitric acid till it exposes a brilliant surface ; yet
the original picture will re-appear by exposure to mercurial
vapour, if the plate be not exposed to heat.

Oil and rotton-stone answer very well for giving the plate
the requisite polish.   It is then to be heated and washed with
nitric acid, and finished by rubbing with dry whitening powder.
To coat the silver with iodine, a box of a suitable size is used,
in the bottom of which is placed the iodine ; the plate is placed
over this substance at the short distance of half an inch.   In
from one to three minutes the coated golden hue appears per-
fect and uniform.   By keeping the prepared plate in darkness
for twelve, or even twenty-four hours, its sensitiveness
becomes increased ; other advantages are also obtained.   Some
of the finest specimens that I have obtained, were produced by
a common spectacle glass, of fourteen inches focus, arranged at
the end of a cigar-box as a camera.   A lens of this size
answers very well for plates four inches by three : reproducing
the objects with admirable finish.   Copperplate engravings are
represented in the minutest details : the mark of the tools
becoming quite distinct under the magnifier.

In the mercurializing process the plate may be placed at any
angle below 45°, or even horizontally, over the mercury.   It
is sometimes advantageous to heat the mercury a second time
when the proof is not distinct by the first operation.   In some
cases the proofs will not come out at a low temperature.

When the mercurial process is complete, the plate is first dip-
ped in cold water, and to be put into a solution of common salt of

moderate strength; it is then to be touched at one corner by a piece of bright zinc, the yellow coat of iodide moves off like a wave and disappears. The zinc is not to be left in contact too long, otherwise it deposits stains: washing in clean water finishes the process.

By the aid of a lens and a heliostat, Dr. Draper caused the moon beams to converge on a plate, and in half an hour a very strong impression was obtained. With another arrangement of lenses he obtained a stain nearly an inch in diameter, and the general figure of the moon, in which the places of the dark spots might be indistinctly traced.

Singular Electric Storm of January 1841.—We have traced this storm through the following range of localities :—Dreadful thunder and lightning at Stannder, Scotland, very early in the morning.—At Manchester a tremendous clap of thunder awakened the inhabitants about 3 o'clock in the morning. There were heavy hail showers at the time, and much vivid lightning, with a gale of wind, but no damage done —The storm arrived at Wolverhampton between four and five in the morning ; the lightning struck the parish church and set the wooden cross on fire.—At Bidlington there was a dreadful storm of lightning, thunder and hail in the morning.—The same storm visited London a little before 7 o'clock, and at ten minutes before 7 the lightning struck Spitalfields Church, twenty feet from the top of the steeple.—Streatham Church steeple was struck by lightning, between seven and eight in the morning. The steeple took fire and was burnt to the ground.—At Boulogne there was a dreadful storm with very vivid lightning during the night.— On the Eastern side of England, we understand, that the storm at Lincoln happened in the morning, and lasted about three quarters of an hour, from shortly after four to five o'clock. There was much vivid lightning, thunder and heavy hail. The wind blew a gale. A wind-mill was struck by the lightning and set on fire. At Boston, there was a dreadful storm of wind, hail, lightning, and thunder. During the transit of this storm over England we hear of three churches, and one wind-mill being struck by the lightning. The most remarkable of these events, in a scientific point of view, is that of Spitalfields Church, which was struck twenty feet below the top of the steeple ; evidently the effect of an *oblique* discharge from a neighbouring cloud.

LECTURE VIII.

The particulars I have already pointed out respecting the Electric Machine, such as keeping it clean, warm, and dry, are applicable to all kinds of electrical apparatus. Every particle of dust presents a virtual point to the surrounding air, and delivers to it a great quantity of fluid as decidedly as the metallic point at the extremity of the prime conductor delivered fluid to the air in one of the experiments of our last lecture. If you will pay attention to the prime conductor, which was perfectly clean when the machine was first put into action, you will observe that it is completely covered with a film of the finest particles of dust, which looks almost like smoke. This covering has been formed by virtue of the prime conductor, whilst in an electric condition, attracting the dust from the contiguous air; and now, instead of presenting a smooth surface it presents an asperous one, every point of which looses some portion of the electric fluid which passed to it from the machine, and consequently, under these circumstances, you have not so much *disposable* electric fluid from the prime conductor, for experiment, as when that apparatus is perfectly free from dust: and as every other piece of apparatus, whilst in use, would loose the electric fluid into the air, from a similar cause, you will be sensible not only of the advantages, but of even the necessity of keeping every part of an electric apparatus perfectly clean. Hence, also, every part of the surface of those instruments that are not purposely intened to *dissipate* the electric fluid into the air, whether they may be constructed of wood, ivory, or metallic matter, ought to have their surfaces well polished, as convex as possible, and without sharp edges; even the operations of the milling taol should be carefully avoided. The loss of fluid from any piece of apparatus from these or any other cause, may very conveniently be called *dissipation*.

Now it very often happens that one, or more, of the collecting points of the prime conductor, dissipates the fluid whilst the others are collecting it; and especially when the prime conductor is very well insulated. This fact may be very well ascertained by looking at the points whilst the machine is in motion; for the receiving points are tipped with a luminous star, and the delivering points exhibit pencils of electric light. This kind of dissipation occurs most frequently from the outermost points of the series.

There is also another source of loss of fluid which may be well to mention in this place. If the cushion has been newly amalgamated, the glass cylinder becomes partially covered with streaks of the amalgam after it has been in action for a short time; and as these streaks are of metallic constitution they are conductors, and carry a portion of the excited fluid entirely round to the cushion again: and thus prevent it from being taken up by the collecting points belonging to the prime conductor. When this happens, the prime conductor gives but very feeble sparks, or any other electric action; indeed,

the machine is out of order. Therefore these streaks and spots of amalgam which stick to the surface of the cylinder must be immediately removed as fast as they appear.

The distribution of electric fluid over the surface of the bodies, is a topic of great importance in the study of this branch of physics, whether it be viewed in a theoretical or in a practical point of view. With non-conducting bodies an equable dissemination of the fluid is not easily attained within any moderate limits of time, whilst on the surface of good conductors, it is accomplished in a moment; and therefore the best conductors, the metals for instance, are best adapted for contemplating the laws of electric distribution.

Let us suppose that we have two flat pieces of metal of equal surface, insulated, and that I communicate to each piece a certain quantity of the electric fluid. Now as each piece has received the same quantity they will be both electric alike; and because they have equal powers of dissemination, the distribution of the fluid in the one piece will be equal to the distribution in the other: for, although the distribution will not be *equable* over the whole of the surface of either of them, it will be similar in both pieces; so that the electric forces at the *centre* and at the *edge* of one plate, will, respectively be equal to the electric forces at the centre and at the edge of the other.

The same reasoning holds good with metallic cylinders with convex ends, such as the prime conductor of the machine. Similar portions of the electric fluid on each cylinder will render them similarly electric on corresponding parts of their surfaces. But in no instance can the distribution be *equable*, unless the body be perfectly *spherical*, and surrounded on every side by equable electric forces. On this subject I shall have much more to say in a future lecture, my present object being only to show you that, with the same electric machine, the electric forces exhibited by the prime conductor will vary with its size and figure.

If the prime conductor be spherical, as is sometimes the case, you may take a spark from any side of it you please, and you will find the pungency of that spark nearly alike from which ever side of the sphere it be taken: more especially if the sphere be not or large dimensions. But if the conductor be a long narrow cylinder with convex ends; the pungency of the spark is greater from the end most remote from the machine, than from its sides.

Now, in order to become acquainted with the effects of prime conductors of different magnitudes, for the same machine, it will be necessary that we first make ourselves acquainted with the electric conditions of two unequal metallic bodies charged with similar quantities of the electric fluid. If, for instance, I were to employ two insulated metallic spheres, whose surfaces were as one to two ; (one a square foot, the other two square feet), and to each sphere I were to transmit a certain quantity of the electric fluid, say

10 particles : It is very obvious that as these 10 particles occupied only half the space on one of these spheres as on the other, the electric density on the small sphere would be twice the electric density on the large one : and as the intensity of the force depends upon, and is proportional to, the density of the fluid, the intensity of force on the smaller sphere is twice that on the larger.

Now, although, as I have before stated, the electric condition of a cylindrical conductor is not equable on every part of its surface, yet the intensity of force in the prime conductor with the same machine, will be in same inverse ratio of the size of the conductor : or, in other words, a small conductor becomes more intensely charged than a large one. On the other hand again, the *quantity* of fluid which a conductor can hold, is in some direct ratio of its size ; or, if you please, a large conductor will hold more fluid than a small one. But it must be observed, that a large conductor, by exposing a more extensive surface to the surrounding air than that exposed by a smaller one, has a better opportunity of dissipating the fluid ; and would require a larger supply from the machine to keep up any certain degree of intensity. There is also another point to be taken into consideration. When two conductors of the same magnitude are charged to different degrees of intensity, that which has the highest intensity will have the greatest propelling force ; and, consequently, will have the greatest dissipating power : but it is found by experience that the dissipation from a large conductor is much greater than from a small one, by employing the same machine. Therefore from all these considerations, it appears, that with a glass cylinder of certain dimensions, say ten inches diameter and fifteen inches long, we may keep a small prime conductor continually charged to a tollerably high degree of intensity : but that the same cylinder could not keep up the same degree of electric intensity in a large conductor. Indeed, the surface of a conductor might be so large as to dissipate the greater portion of the whole of the fluid excited by the cylinder.

Again, the *striking distance*, (or the thickness of the plate of air that the first spark from a conductor will penetrate,) is proportional to the intensity of the fluid in that particular part of the conductor from which the spark is taken : therefore the higher the intensity the greater the length of the first spark. I here take into calculation the *first spark* only, because after the plate of air is once broken through, the succeeding sparks of a series, will be regulated by the *quantity* as well as by the *intensity* of the fluid transmitted.

Let us now suppose that a spark taken from a conductor at a certain distance is constituted of the whole quantity of fluid which the conductor contained at the precise time of its emission : and that the degree of intensity is constantly the same. It is very obvious that a spark from a large conductor would be more powerful than a spark taken from a small one : because there is more fluid in the spark from the large conductor than in that from the small one, and because the velocity is the same in both cases. Therefore, in

order to obtain formidable sparks from the action of a machine of given power,
we must endeavour to regulate the size of the conductor to the power of the
machine. If it be too small the sparks will be small, pungent and rapid.
They are small and rapidly produced because the quantity of fluid constituting
each spark is small, and the supply is comparatively great: and they are
pungent because they are discharged with great velocity and impinge on a
mere point on the skin. If the conductor be too large, the intensity of the
fluid can never rise to a great hight, because the supply cannot be kept up
by the machine. The sparks will be infrequent and will produce a dull heavy
blow to any person taking them. But when the conductor is of a proper size
to be kept well supplied at a high intensity, the sparks are delivered rapidly,
are extremely severe on any part of the body on which they impinge: give a
brilliant white light and produce a continuous loud rattling noise.

When the glass cylinder of the machine is of the dimensions already stated,
the cylindrical prime conductor with convex ends, may be three feet long and
six inches diameter : and the supply of fluid will be sufficient for a discharge
of sparks of great intensity and power. As the greatest degree of intensity
of a prime conductor is invariably found at its remote end, and greater in
proportion to the smallness of the ball which terminates the conductor; the
striking distance is greater, and consequently the sparks are longer when
taken from a small ball than from a large one. We will prove this fact by
the following experiment.

I will take away the ball from the prime conductor C. C., fig. 9, page 421,
and put in its place the apparatus A. B. C., which is a bent brass wire with a
ball at each end, one of which is much larger than the other. To the bend
of the wire is fastened a stem, to be introduced to the hole in the end of
the conductor. Now I will take sparks from the large ball to another ball
which I hold in my hand, and you will observe that when the distance between
them is small, the sparks are very large and dense, and give a bright white
light, with a great deal of noise; and if I stand on a chain, or any other metal
which is in good connection with the ground, I feel a dull, heavy, disagreeable
blow on my shin and ancle bones. I will now withdraw my hand farther from
the ball of the conductor, and you will perceive that sparks are not so frequent
as before, but that they are more brilliant, and make a greater noise, and the
shocks on my legs are much more severe. When I seperate the two balls to
about three inches, the sparks appear at very distant intervals, and a little far-
ther off they entirely disappear. Certainly none can pass through four inches of
air.

Having now ascertained the maximum length of a spark from the large ball
A, I will shew you what will happen at the small ball, C. You perceive that
I can take a spark from this ball, when the ball in my hand is five or six inches
distant from it; and by increasing that distance gradually, I obtain a spark of

sparks ten or twelve inches long; and in some cases they may be made to pass through a space of eighteen inches. * In this case, the sparks have none of that brilliancy of white light as is developed by the sparks taken from the larger ball; they are of a red colour, inclining to purple, and travel in very crooked lines, for reasons already pointed out in the last lecture. Whilst these long straggling sparks are passing between the two balls, there is a great dissipation of fluid into the surrounding medium, as you may see very clearly when the room is well darkened. The dissipating part of the fluid forms beautiful ramifications of purple light, which dart into the air from the angles of the zig-zag path described by the main body of the stream of sparks.

AWARD OF PRIZES,

FOR

COMMUNICATIONS TO THE SIXTH VOLUME,

OF THE ANNALS OF

ELECTRICITY, MAGNETISM, & CHEMISTRY, &c.

The most pleasing duties and the noblest actions of Scientific life are those of conceding the honour of discovery, or of invention, to those of our fellow labourers to whom it is justly due, and of appreciating the advantages derivable from those laws of physical science which such discoveries have developed; or from the application of those laws, whether *directly* contributing to the comforts and happiness of man, or *indirectly* accomplishing that end, by the production of novel implements of research, that may facilitate future investigations in the developement of still more valuable truths.

Neither can there be a greater honour devolving on man than that of having, by his own talent and persevering investigation, brought to light any of those hidden laws of nature which, till then, had been curtained from the keen eye of scientific research; or that of demonstrating the exactness of such laws which previously been either but obscurely developed, or but little known to the cultivators of science. Of the high value of such discoveries and of such demonstrations we have ample proof in almost every department of physicial knowledge. They are ever considered of a high order in promoting the interests of philosophy; and, in the affairs of civilized life, their amplitude of usefulness surpasses all calculation, and can only be limited by the precincts allotted to time.

Before the commencement of these Annals, there was obviously a great want in this country, of a proper channel through which the experimental

labours, of those persons who were either but obscurely, or not at all, known to the scientific world, could be brought to light and properly recorded with that degree of promptitude and candour which alone give to discoveries their true value, and confer upon their authors that extent of true merit which they so justly have a right to expect. The desideratum being supplied, many experimentalists have become known whose names were never before implanted in the history of science: and these Annals have the honour of bearing original records of experimental discoveries which have not been surpassed since the first appearence of the work. And there is every reason for entertaining the most flattering hopes, that, by the further inducement now held out for entering the field of honourable competition, a spirit of still more ardent and extensive research will shortly be manifested by every class of enquirers in experimental science.

The prizes which fall within the precincts of the specifications in the list of subjects, for the present volume, are awarded to W. H. Weekes, Esq. and to J. P. Joule, Esq. To the former gentleman for his paper descriptive of his brilliant Atmospheric Electric Experiments: and to the latter gentleman, for his paper descriptive of the most powerful Electro-Magnet in proportion to the weight of iron employed.

Mr. Weeks has long been an arduous and successful labourer in the field of experimental science, and has contributed opulently to the "*Annals of Electricity, &c.*," whose pages have been highly enriched, both by the interesting character of his investigations and by the flowing energy of his pen. The following are the titles of those of Mr. Weekes's papers that have appeared in various volumes of this work.

1st.—Description of the Hydroplasson, a portable, simple, and perfectly secure, self-acting instrument, for exhibiting the composition of water from the combustion of its gaseous constituents in a state of previous admixture. Vol. 3, P. 345.

2nd.—On the decomposition of water by the agency of growing plants; more particularly the Aquatic Confervæ—the Lemna, a genus of the Monœcia Diandria Class, &c. Vol. 4. P. 25.

3rd.—Memoir, on the employment of the Oxy-hydrogen gas in its applications to the purpose of Chemistry and the Arts, by means of the Silex-safety Blow-pipe; an instrument adapted to the perfectly secure combustion of explosive gaseous mixtures in any continuous quantity, and with jets of extraordinary dimensions. Vol, 4. P. 192.

4th.—A Letter to Thomas Pine, Esq. of Maidstone, on some Experiments undertaken with a view to Illustrate his Theory of Electro-Vegetation, &c.

5th.—Synoptic view of the precise amount of pure carbon yielded, by the rigid analysis, from Charcoals of thirty principal known woods. Vol. 4. P. 397.

6th.—Description of a Meteor, seen at Sandwich, Kent, Vol. 4. P. 566.

7th.—A Report of some experiments in Atmospherical Electricity, made in the Autumn of 1840, during which the disengagement and insulation of Ozone were effected. Vol. 6. P. 89.

8th.—Observations on the Electrical phenomena of high pressure steam, as recently exhibited in the vicinity of Newcastle, &c. Vol. 6, P. 232.

9th.—On Atmospherical Electric Apparatus and Experiments. Vol. 6. P. 446.

This last series of experiments by Mr. Weekes, will no doubt give to the satisfaction that even the most sceptical could wish for, respecting the existence and the effects of atmospheric Electrical waves. It is a topic of the highest importance in the study of atmospheric electricity: and, as is well known to the readers of this work, it is one which I have been exceedingly anxious to get properly understood. There are not many experiments on record, which have yielded such brilliant results from the effects of electric waves as those which Mr. Weekes has had the good fortune to observe. Indeed, with the exception of those observed by M. De Romas, * and some which I have seen at various times,† I am not aware of any on record which are to be compared with them in point of splendour and importance. Nor have even these the importance of the Sandwich experiments, which were so laudibly undertaken for the purpose of a specific enquiry, in which the operator had no interest but the cause of scientific truth: and which have set at rest the most important questions,‡ on atmospherical electricity that have presented themselves to the philosophical world since the solution of the great problem respecting the indentity of electricity with lightning.

Mr. Joule is one of those indefatigable, and now well disciplined experimentalists, whose first essays on experimental science these Annals have the honour of recording. His enquiries have been directed, principally, to the department of Electro-Magnetics, in which he has been peculiarly successful. Mr. Joule's communications to the Annals will be found under the following heads.

1st.—Description of an Electro-Magnetic Engine. Vol. 2. P. 122.

2nd.—Description of an Electro-Magnetic Engine with Experiments. Vol. 3. P. 436.

3rd.—On the use of Electro-Magnets made of iron wire for the Electro-Magnetic Engine. Vol. 4. P. 58.

4th.—Investigations in Magnetism and Electro-Magnetism, first communication. Vol. 4. P. 131.

5th.—Investigations in Magnetism and Electro-Magnetism, second communication. Vol. 4. P. 135.

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* Annals of Electricity. Vol. v. P. 69.

† Ibid————————Vol. iv. P. 181 : also Phil. Mag. also my letter to Mr. Weekes, dated April 19th. 1841.

‡ The existence of Electric-Waves, and the *lateral discharge* from lightning conductors.

6th.—Description of an Electro-Magnetic Engine. Vol. 4. P. 203.

7th.—Investigations in Magnetism and Electro-Magnetism, third communication. Vol. 4. P. 474.

8th.—Investigations in Magnetism and Electro-Magnetism, fourth communication. Vol. 5. P. 187.

9th.—Investigations in Magnetism and Electro-Magnetism, fifth communication. Vol. 5. P. 470.

10th.—Description of a New Electro-Magnet. Vol. 6. P. 431.

Mr. Joule's theoretical views, from which he has been enabled to produce such powerful magnets, are stated in those papers which appear in Vol. 4. P. 131, 135, 474; and in Vol. 5. P. 187, 470, which may be perused with great interest.

In awarding prizes for Scientific discoveries and inventions, it will sometimes be found difficult to form a satisfactory decision which may have the greatest claims in point of importance and value; especially when several of great interest present themselves at nearly the same time. On the present occasion, however, nothing of the kind could possibly interfere in the decision already made. Mr. Weekes's paper which has so justly entitled its author to one of the prize volumes, is not only descriptive of experiments of a peculiar character, but as regards importance they have no parallel in the history of Atmospheric Electricity.

Mr. Joule, whose claims to a prize volume rests on the production of an Electro-Magnet of great power with a comparatively small weight of iron, has also other claims on this branch of science for his previous valuable communications, already referred to, and with which this is so intimately connected.

Science, however, is not always benefited by making too nice distinctions between discoveries or inventions specified within certain limits, and those which approach the line of demarcation so closely as to be of nearly, if not quite, of equal interest. Hence it is, that whilst Mr. Joule enjoys the honour of theoretical investigation, and priority of carrying out his views of Electro-Magnetic action, by the production of magnets of quite a novel form, Mr. Radford, and Mr. Roberts have been eminently successful in the practical part of the subject, by producing their respective Electro-Magnets described at Pages 231, and 166: and as the object of these prizes is the promotion of Electricity and Magnetism in all their branches both theoretical and practical, these fine specimens of Electro-Magnets produced by Mr. Radford and Mr. Roberts ought not to pass unnoticed in the award of the prize volume in which they are recorded. A prize volume is therefore awarded to Joseph Radford Esq. for producing a perfectly original, and curiously formed Electro-Magnet of great power: and a prize volume is also awarded to Richard Roberts Esq. for producing the most powerful Electro-Magnet hitherto recorded.

END OF VOLUME VI.

THE ANNALS

OF

ELECTRICITY,

MAGNETISM, AND CHEMISTRY;

AND

Guardian of Experimental Science,

CONDUCTED BY

WILLIAM STURGEON,

Superintendent and Lecturer of the Royal Victoria Gallery of Practical
Science, Manchester, fromerly Lecturer on Experimental Philosophy at the
Honourable East India Company's Military Academy, Addiscombe, &c.

AND ASSISTED BY GENTLEMEN EMINENT IN THESE DEPARTMENTS OF PHILOSOPHY.

VOLUME VI.

LONDON :

INDEX TO VOLUME VI.

A.

INDEX.

O.

P.

R.

S.

T.

V.

W.

Page 18 Line 15, for "Places" read "placed"

   34 — 41, for "last" read "least"

   35 " 45, for "amouroses" read "amaurosis"

   89 " 12, for "even" read "ever"

   90 " 46, for "operation" read "\ hor"

   91 " 15, for "formed" read "proceeded" and dele both commas

   " " 22, for "arise" read "arrive"

   98 " 37, for "maintained" read "maintaing"

   95 " 22, for "but" read "bent"

Bottom line but one for "occasioned" read "occasional"

   96 " 6, for "powerful" read "powerfully"

  112 " 8, from bottom, for "manifestations" read "manipulations"

  113 " 9, for "glased" read "glazed"

  " " 11, for "formed" read "poured"

  " " 37, after "named" dele the comma.

  714 " 12, dele comma after "tubes", and introduce comma [after

  161 " 31, for "accorded" read "afforded"

  254 " 10, after "fur" introduce "produce similar effects"

  255 " 20, from bottom, for "a" read "the"

  296 " 33, after "and" introduce "of" and dele "through"

  298 " 50, for "pumping" read "jumping"

  334 " 34, for "larger" read "longer"

  446 " 13, for "operation" read "application

  " " 22, space wanted between "sentiments" and "a

  447 " 11, for "laternal" read "lateral"

  " " 44, for "plate VII" read "plate VIII"

  " " 4, for ... Do ... Do.

  " " 27, for "upright" read "upright"

  " " 28, for "found" read "formed"

  " " 45, for "fixing to" read "fixing

  " " 48, after "under" introduce the

  " " 16, dele "this"

  " " 28, for "philantrophy" read "philantropy"

  502 " 26, for

II. *Fourth Letter on Voltaic Combinations, with refer-
ence to the Mutual Relations of the Generating and
Conducting Surfaces.* Addressed to MICHAEL FARA-
DAY, *Esq.,* D.C.L., F.R.S., *Fullerian Prof. Chem.
Royal Institution, &c. &c. &c.* By J. FREDERIC
DANIELL, F.R.S., Prof. Chem. in King's College,
London.

MY DEAR FARADAY,

In my second[*] letter to you upon Voltaic Combinations,
I suggested that, in a theoretical point of view, the most
simple and perfect combination would probably consist of
a solid sphere, (or rather active point) of a generating
metal, surrounded by a hollow sphere of an inactive con-
ducting metal, with an intervening liquid electrolyte; the
circuit being completed by a conducting wire properly
disposed for connecting the two metals. Further reflec-
tion led me to believe that a series of experiments com-
mencing as nearly as possible with these most simple con-
ditions of the problem, might throw some light upon the
relative dimensions, positions and actions of the generating
and conducting plates of voltaic combinations in general;
which appeared to me not to have received all the elucida-
tion of which they might be susceptible. The subject, it
is true, has not escaped the attention of experimenters;
but most of the results with which I am acquainted are so
involved in the errors arising from the variable condition
of the source of the current itself, which I have already
pointed out, as to leave it in a very unsatisfactory state.
I moreover proposed to myself, in the investigation which

[*] Annals of Electricity, &c. Vol. I. p. 102.

I consequently undertook, to trace the self-distribution of
the force from its origin, by the indications of reduced
copper, in the manner described in my first letter.* I
have thus been led to some results which I trust you will
consider of sufficient importance to justify my troubling
you with another communication upon the subject.

My first apparatus, which it is necessary to describe,
consists of two hollow hemispheres of brass, fitting together
water-tight by means of exterior flanges half an inch
wide, and a collar of leather, and thus forming a sphere,
the interior diameter of which is 9¼ inches; consequently
exposing a surface of about 268·8 square inches. The
lower hemisphere is fitted into a frame carrying buttons,
by which the upper can be securely wedged down upon it;
and underneath there is a small cock, by which any liquid
in the interior may be drawn off. The upper part of the
upper hemisphere terminates in a tube of about one inch
in length, forming an opening through which a membra-
nous bag may be introduced, and from which it may be
suspended. The electrolyte which I employed in all the
following experiments was the same as that which I em-
ploy in the constant battery, viz. eight parts of water, by
measure, to one part of oil of vitriol in contact with the
generating surface, and the same diluted acid, further
saturated with sulphate of copper, in contact with the con-
ducting surface :—

The measures of the force to which I had recource were,
in the first instance, a coarse galvanometer consisting of a
single needle seven inches in length, with a coil of twenty
turns of wire one-twentieth of an inch in diameter. The
large scale of the experiments precluded me from employ-
ing delicate instruments with astatic needles. Being
dissatisfied with this measure, I afterwards employed a
Bréguet's thermometer fitted up according to the plan of
Professor De la Rive†, which measures the force of a
current which passes through it by the differences of its
heating power upon the compound spiral of platinum and
silver of which it consists, the degrees into which it is divided
being directly proportioned to such differences. I had every
reason to be satisfied with this instrument; and have no
doubt of its accuracy, in the case, at least, of currents of
such low tension as those for which alone I have hitherto
it.

In my first experiment the sphere was charged with the solution of copper, and the membrane with the plain acid, a small sphere of amalgamated zinc, one inch diameter (and exposing a surface therefore of 3·14 square inches) was suspended, by means of a well-varnished copper wire in the centre of the latter: the other extremity of the wire was connected with one of the cups of the galvanometer, and the circuit was completed by a wire leading from the other cup to a small mercury cup upon the upper brass hemisphere, placed at a distance of two inches from the tube. The deviation of the needle was 60°; and it remained steady for a long period, during which the experiment was repeated and varied. When, instead of the galvanometer, the circuit was closed with a piece of platinum wire one inch in length and $\frac{1}{15}$ inch diameter, it continued red hot for a period of five hours. The circuit remained closed for seventeen hours, and the apparatus was then opened and examined. The zinc ball had dropped off the wire, and was reduced to about one half of its original size. The upper hemisphere was found coated with reduced copper, beautifully marked half-way up from the equator with concentric circles of alternate dark and light stripes of pink and red; these were followed by a broad even band of pink, which reached to a circle within $1\frac{1}{2}$ inch of the aperture, which was composed of the unchanged surface of the brass, and which evidently had not been in contact with the liquid. The lower hemisphere, which had been insulated from the upper by the collar of leather, had no copper precipitated upon it.

The sphere was again put together, and charged as before, with a new zinc ball. The circuit was closed, as in the first experiment, with the galvanometer in contact with the upper hemisphere, and the deviation of the needle was 60°. The connection was then broken with the upper hemisphere, and made with the bottom of the lower hemisphere; the deviation was again 60°.

When connexion was made at the same time with both hemispheres, the deviation was the same. If, while in this state, either wire were lifted singly from its connexion, the needle remained perfectly steady.

When the wire from the zinc ball was lifted from the cup, there was a spark upon breaking the circuit; and there was also a spark when the wires from the two hemispheres were disconnected from the circuit together, but not when lifted singly.

Upon leading the conducting wire of the lower hemisphere into direct communication with that of the zinc ball, while the wire of the upper hemisphere was connected as before with the galvanometer, or vice versa, the needle only receded to 40°. From this it appeared that, although the whole amount of force originating at the zinc was capable of passing off by means of either hemisphere singly, when both passages were open it distributed itself between the two, notwithstanding the additional resistance in one by the interposition of the galvanometer.

Extra contacts made with different parts of the two hemispheres made no difference in any of the preceding experiments.

The apparatus was now left for eighteen hours with both hemispheres in connexion with one extremity of the galvanometer, and the zinc ball with the other. Upon examination the needle was found still deflected 25°; and upon agitation of the zinc ball it rose to 55°, but almost immediately declined to 25°. The acid had become nearly saturated with oxide of zinc. The sphere was opened, and both hemispheres exhibited a perfectly even coating of beautiful pink copper, through which the rings of the upper hemisphere were however still discernible.

It will perhaps be advantageous, before I proceed further, to collect into one point of view the principal conclusions which, I conceive, may be drawn from the preceding experiments with the spherical combinations.

1st. The force emanating from the active zinc centre diffused itself over every part of the upper hemisphere, from which there was a good conducting passage for its circulation, to an amount which is measured by 60° of the galvanometer; and which was capable of maintaining an inch of platinum wire $\frac{1}{50}$ inch diameter at red heat.

2nd. The same amount of force was maintained by either hemisphere indifferently; but when both conducting hemispheres were in metallic communication there was no increase of force. The transfer of half the power from one hemisphere to the other occupied no appreciable interval of time.

3rd. Although the force was not increased, it spread itself equally over the whole sphere, as manifested by the diffusion of the precipitated copper.

4th. When one hemisphere was connected with the zinc centre by a short wire capable of affording circulation to the whole force, and the other hemisphere was connected

by a long wire through the galvanometer with the same
centre, the diffusion of the force over the whole sphere
was maintained, although the half of it was obliged to
overcome the much greater resistance of the longer circuit.

5th. There was no greater accumulation of precipitated
copper about the points with which the conducting wires
were brought into contact, and towards which the force
diffused over the sphere must have converged, than at any
other point; proving that the force must have diverged
from the centre equally through the electrolyte, and could
only have drawn towards the conducting wires in the
conducting sphere itself.

I now destroyed the insulation of the two hemispheres
by fixing to the lower one a thin ring of brass, which came
in contact with the upper when wedged down in its position
by the means already described. The sphere, thus in good
metallic communication in every part, was charged as
before; the precipitated copper having been previously
cleaned off by a little nitric acid and rotten-stone. The
deviation of the galvanometer was 55°, and it was perfectly
steady, whatever number of connexions were made with
the sphere, or at whatever point the circuit was completed.
It made no difference in the amount of force which circu-
lated, whether the contact with the brass was made as
near as possible to the zinc conducting wire, or at the
point the farthest removed from it.

I next proceeded to ascertain what would be the effect
of increasing the surface of the generating metal. For
this purpose two amalgamated zinc balls of the same
diameter as before were placed in contact upon a varnished
wire, and substituted for the single ball in the centre of
the sphere: the deviation of the galvanometer only in-
creased 5°, rising from 55° to 60°. Upon replacing the
single ball it fell again to 55°, and so alternately rose and
fell to the same amount upon frequent repetitions of the
change. An amalgamated zinc rod six inches in length,
and ⅛ inch in diameter, was then substituted for the balls,
but the deviation did not rise higher than 60°. Thus the
generating surface was increased from about three square
inches to six and 9·4 with very little increased effect.

These experiments were all made with the generating
metal placed as nearly as possible in the centre of the
conducting sphere: this position was now changed, and
sometimes the zinc ball was placed at the bottom of the
membrane almost in contact with the sphere; sometimes

it was drawn up nearly to the top; again it was placed in the centre; but none of these changes produced any appreciable alteration in the deviation of the needle, the galvanometer constantly indicating 55°.

From this I was led to consider (although I am quite aware that the measures may not have been sufficiently accurate to determine the point with precision,) whether the force emanating from the zinc ball might not diffuse itself over the surrounding conducting sphere in obedience to the well-known law of radiant forces of the inverse square of the distance; since, although the cases may be dissimilar, according to this law an attractive point placed within an hollow sphere of attractive matter remains in equilibrio, whatever its position may be. Or the analogy, perhaps, is stronger of light diffusing itself from a luminous point within the sphere in the same relative positions as those of the generating ball.

This conclusion, if correct, appearing to me to be of great importance, I was desirous of confirming it by repetition and variations of the experiment.

For this purpose I made use of the calorific galvanometer (as it has been named by M. De la Rive,) instead of the magnetic; and the zinc ball was a little larger than n the preceding experiments, measuring 1½ inch in diameter, and presenting therefore a surface of 3·94 square inches. The sphere was charged as before, and when the ball was placed in the centre

> The index advanced 90
> When drawn nearly to the top 90
> When thrust nearly to the bottom...... 87

The index remained perfectly steady during several repetitions of the experiments; and when the connection with the brass sphere was changed to opposite points. There can be no doubt that these results indicate the same equality of action as before, the slight difference in the lower position being, probably owing to accidental circumstances, such as the unequal thickness of the membrane at that point, or the speedy saturation of the acid when the solution cannot fall away from the zinc. Thus it would appear, that in none of these positions was there any virtual approximation of the generating and conducting surfaces. The diffusion of the precipitated copper was, however, very much influenced by the position of the ball; when near the top or the bottom, it was thrown

down in a compact layer on a segment immediately in its
vicinity, and became thinner and thinner over the more
remote parts of the sphere. It was only when exactly in
the centre that the diffusion was perfectly equal. To ob-
serve this effect with the greatest distinctness, the action
in each case should not be allowed to continue more than
ten or fifteen minutes; for the deposition becomes com-
pact in every part in a longer time, when it is not so easy
to judge of the difference of thickness.

My next step was to measure the effects of different
portions of the sphere in combination with the zinc ball.
I took the lower hemisphere alone of the apparatus and
filled it with the solution of copper, and placing a wooden
bar across it which supported the membranous bag to
carry the acid, I had it in my power to immerse the ball
in any required position.

I first placed the same ball as in the last experiment
just below the surface exactly in the centre, and the
calorific galvanometer indicated 90°: upon removing it
within half an inch of the bottom it rose to 115°.

On another occasion I placed the ball in three different
positions upon the surface of the liquid in the hemisphere,
namely, in the centre and close to each side: in each
position the instrument marked 86°. Upon lowering it
as close as possible to the bottom it rose to 100°. In all
these experiments the precipitated copper was diffused
over the whole hemisphere.

It is worthy of remark, that the amount of force thus
called into action at the surface of the liquid in the hemi-
sphere, is nearly the same as that from the whole charged
sphere, or either of its hemispheres.

These results, I conceive, are not in opposition to the
law of radiant forces suggested by the experiments with
the entire sphere; although I cannot vouch for the com-
parative measures being absolutely correct or uninfluenced,
in some degree, from day to day by extraneous circum-
stances, and particularly by changes of temperature: nor
can it, indeed, be expected that the experimental deduc-
tions from a ball of the dimensions which I employed can
do more than approximate to the mathematical demon-
strations of the relations of an active point within an
attractive sphere.

An accidental circumstance next furnished me with an
interesting variation in the combination of the whole
sphere. My intention had been to fill it as before, and

repeat the experiments; but I afterwards found that the
liquid only reached to within 45° of the vertex. The
zinc ball which I made use of was ¼ inch in diameter, and
the membrane was full of the dilute acid. The experi-
ments were made in three different positions of the ball,
and with three variations of the wire connecting the cir-
cuit with the brass sphere.

The first connection was made near the top at a point
which was not within the contact of the included liquid;
the results were

Caloric Galv.

At the top	45°
Centre	55
Bottom	65

Connexion with the bottom of sphere.

Ball at the top	45°
Ball at the centre	70
Ball at the bottom	77

Connexion with the sphere both at the top and bottom.

Ball at the top	45°
Ball at the centre	55
Ball at the bottom	65

The two last series may be taken to be identical, but I
am a loss to explain the difference of the first series from
those two. It is probable that the zinc ball, though
wholly immersed in the acid in the membrane, was not
below the surface of the exterior solution in the sphere.
The results confirm in a general way the conclusions
which have been previously suggested.

There was yet another combination which I thought it
desirable to try; namely, with the generating ball placed
within the charged sphere with its two hemispheres insu-
lated. The zinc ball which I employed for this experi-
ment was one inch diameter, and I tried it first with the
standard acid with which all the other experiments had
been made, and repeated it with the same acid diluted
with an equal bulk of water, in order that I might ascer-
tain whether the law of action was influenced at all by the
amount of force put into circulation. The results were
perfectly consistent with each other.

	Standard Acid.	Dilute Acid.
The zinc ball placed near the bottom, connexion with top insulated hemisphere ...	52	42
Connected with bottom	59	50
The zinc ball placed near the top, connexion with bottom insulated hemisphere	51	41
Connected with top	59	50
Both hemispheres connected	59	

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* In these experiments that hemisphere alone with which
the connexion had been made could have influenced the
results as a conducting surface.

A question now occurred to me of extreme interest:
admitting that the force generated in the preceding com-
binations where it was limited in its diffusion by the con-
cave surfaces of spherical forms, ·follows in its action the
law of radiant forces, we know that it is not a *simple*
radiant force like that of gravity, but a molecular force
propagated from particle to particle, and possibly modified
by other forces with which the same particles may be en-
dowed. The law of radiation, if established, may, there-
fore, be the result of the concurrence of more than one
force, limited by the spherical combinations with which we
have been dealing. The physical law, in ordinary cases,
supposes both the sphere and the point with to consist of
similar attractive matter; and were the force which we
are now investigating of the same nature, it would make
no difference whether the sphere consisted of merely con-
ducting metal and a small interior generating ball, or
whether the sphere consisted of generating metal and the
ball were an inactive conductor,—inactive I mean with
regard to the electrolyte. What experimental results,
therefore, would the latter combination afford?

I caused two hemispheres of zinc to be made of exactly
the same form and dimensions as those of copper, and
fitted together in the same way; and the interior surfaces
of these were thoroughly amalgamated with mercury.
When wedged together by their flanges and ring of lea-
ther, metallic contact was preserved between the two by a
small interior ring of amalgamated zinc. The sphere was
charged with dilute acid, and the interior membranous bag
with the acid solution of copper. A copper ball of one
inch diameter fixed to the end of a varnished wire was

then immersed in the latter, and a circuit formed by contact with different parts of the zinc sphere with the intervention of the calorific galvanometer.

The indications of the instrument were as follow :

Ball at the top............	36
Ball at the centre	36
Ball at the bottom	36
Again at the top.........	35

The index was steady for a short time, but began slowly to decline in each instance for about 10°. Upon agitation of the ball in the solution it always rose to its previous amount.

Upon repeating the experiment and keeping the ball always agitated, the following results were obtained :—

Ball at the top............	40
Ball at the centre	42
Ball at the bottom	42

Another repetition gave

Ball at the top............	45
Ball at the centre	46
Ball at the bottom	44

The index always fell when the ball was not agitated, and the decline may probably be ascribed to a change in the saturation of the liquid in immediate contact with the ball, which agitation prevented by keeping the solution in an uniform state.

I now separated the hemisphere and experimented with the lower one alone. The results were as follow :—

Ball at the top............	35
Ball at the bottom	80
Ball at the top............	35
Ball half-way between...	60

When the ball was placed at the centre of the surface and at the sides almost in contact with the hemisphere, the index rose to 36° in all the three positions.

Hence it appears that the same law was maintained; although the force which circulated was reduced to one half of the amount of that from the first combination.

Before I venture to offer two or three remarks upon this difference, I shall proceed to lay before you the results of some experiments upon combinations of generating and conducting surfaces of other forms, commencing with the cylindrical as approaching in simplicity to the spherical, and for the purpose of connecting my observations with that form which I have found most practically advantageous in the construction of my battery.

I took one of the cells of the small battery, six inches in height and eleven inches in circumference, charged in the usual way, and found that the single zinc ball of one inch diameter, produced with it a deflection of the magnetic galvanometer of 55°. Two similar balls only increased the deflection to 60°, and its own rod, six inches in length and ⅜ inch diameter, did not increase the effect. These were exactly the same amounts as were produced by the same generating surfaces in the brass sphere.

An amalgamated zinc cell of exactly the same dimensions, charged with the diluted acid and solution of copper in the membrane, with a copper ball one inch diameter, deflected the galvanic needle 45°.

With the calorific galvanometer it moved the index to 25°.

Being desirous of ascertaining whether the same difference between the reversed combinations would exist in a series as in single circuits, I compared together three copper cylinders with three zinc rods, and three zinc rods, and three zinc cylinders with copper rods, by means of a voltameter. The former produced in ¼ hour 3·6 cubic inches of gas. The latter produced in ¼ hour 1·75 cubic inches: indicating, as before, that when the generating surface constituted the circumference of the arrangement, the force was only the half of that which was evolved when it formed the centre.

In the copper cylinder it made no difference whether the zinc ball or rod were placed in the centre, or nearly in contact with the side.

I next took an oval copper plate, the diameters of which were 13½ inches by 10 inches, and soldered a copper wire at one extremity of the longest diameter, in a perpendicular position, and placing it in an earthen pan, covered it with a depth of 1½ inches of the acid solution of copper. Over its centre I suspended, by means of a cross bar, a bag of membrane filled with the dilute acid; and nearly at the bottom of this I placed an amalgamated zinc ball, connected with a varnished copper wire. I then formed a circuit by

means of the magnetic galvanometer, and the needle was deflected 55°. It remained perfectly steady for half an hour, when the plate was taken out and examined. It was found covered with fresh precipitated copper, the coat being a little thicker at the centre, and becoming thinner by almost insensible gradations towards the edge. At one point it had begun to turn round the edge and to diffuse itself on the other side.

The zinc ball was next placed just below the surface of the liquid in the pan, so that its least distance from the conducting plate was the same as when placed in the centre of the sphere. Upon completing the circuit the needle was deflected 45°.

The zinc ball was now drawn up in the tube so as to remain immersed in the acid, but two inches above the level of the solution; the needle was still deflected 35°. Upon again replacing it below the level it returned to 45°, and fell to the same amount upon once more drawing it up. In this case the lateral diffusion of the efficient force must have been prevented for the first two inches of its course; the mode of its after propagation was the subject of my next inquiry. While the apparatus was in this position, one zinc rod, and afterwards two, connected together, were immersed into the acid, so as to extend through the whole depth of the solution, without deflecting the needle more than 55°.

In these experiments the stem of the copper wire which was soldered to the copper plate was covered with precipitated copper as well as the plate. In subsequent experiments it was protected by varnish.

In order effectually to cut off the lateral radiation from the zinc ball, I placed it in a glass tube, six inches long; and over the lower aperture, which was 1½ inch diameter, I tied a piece of membrane: the tube was then filled with dilute acid and plunged into the solution of copper contained in the brass hemisphere, against the bottom of which it rested. The ball was supported within half an inch of the diaphragm. Upon closing the circuit by means of the galvanometer, the needle indicated 40°. At the expiration of five minutes the solution was drawn off and the hemisphere examined: there was found a beautiful well-defined circle of pink copper, two inches diameter, surrounded by a halo of darker colour, evidently of thinly precipitated copper of less thickness, but not extending over more than a fourth part of the surface. From this

experiment it was evident that the force had diverged from the aperture of the glass tube, as from a centre, after it had entered the solution; the circle of pink copper being of a diameter half an inch greater than the aperture, and the fainter halo extending some inches around.

I now moved the tube into such a position, that, the zinc ball, remaining where it was in the tube, might be just below the level of the solution in the hemisphere; and now, notwithstanding the greater distance at which it was placed from the conducting surface, the needle rose to 45°, and the precipitated copper made its appearance over the whole hemisphere.

While the zinc ball was in its last position, the glass tube itself was pressed down till it again rested upon the bottom of the hemisphere; all lateral diffusion was thus cut off for a distance of 4½ inches, through which the force was propagated, and it could only spread after it emerged from the tube; the galvanometer indicated 30°. When the glass screen, as it may be called, was again drawn up, the needle returned to 45°.

Similar experiments were often repeated with the substitution of large flat plates for the hemisphere. When the zinc ball was thus confined in a tube with a diaphragm, and placed within an inch of the plate, the precipitation always commenced with a circle a little larger than the aperture, and gradually extended itself, so that after some hours action it formed a circle of four or five inches diameter, and sometimes turned the edge, and made its appearance on the under surface.

Being desirous of ascertaining to what amount the under surface of a plate thus immersed in the electrolyte would affect the action, I covered the upper surface of the oval copper plate with lac varnish, and replaced it in its pan; I then placed the zinc ball in the tube within half an inch of the diaphragm, and plunged it just below the surface of the solution; upon completing the circuit the needle indicated 50°. In ten minutes time I examined the plate, and found the under side covered with beautiful pink copper with the exception of an irregular oval space, whose diameters were about 4½ inches by 4. The precipitation had evidently begun upon the edge, where it was thickest.

The plate was returned to its position, and the galvanometer again indicated 50°. When a double ball was substituted for the single it rose to 55°. It was again

examined after an hour's action, and on the under side
presented an appearance of a border of compact pink
copper, varying from 1½ inch to ¼ inch in width, but not
wider at the sides than the ends; and the remainder was
covered with precipitated copper of a darker red colour,
into which the former graduated. On the upper varnished
side there were a number of little rosettes of copper, which
were loose, giving it a gritty feel, and which evidently
proceeded from a number of minute points to which the
varnish had not adhered. Fig. 1. (Plate I.) may convey
an idea of the appearance of the precipitated copper.
Both surfaces of the copper plate having been cleaned, it
was replaced in the solution, and the double zinc ball in
the membrane, instead of being placed over the centre,
was suspended over that end of the oval which was far-
thest removed from the conducting wire. The galvano-
meter indicated 48°, and remained perfectly steady for an
hour, during which the action was continued. At the
expiration of this time the plate was examined, and the
pink copper was found deposited only at the extremity
under the ball, forming a segment of a circle, of which the
ball was the centre, but still not sharply defined, but
imperceptibly shading off by a darker colour to the un-
changed surface at the other extremity.

Here the efficiency of the generating ball was evidently
impeded by the deficiency of the conducting plate on one
side. The radiation could only take place towards the
other side; and although the conductor was of great ex-
tent, the increasing distance of its several points caused it
to act at a disadvantage.

After these experiments, the plate was cleaned and
replaced in the solution; and in order to determine the
action of more than one generating point, at a distance
from one another upon the same conducting plate, I first
placed a single zinc ball in a membrane close to the con-
ducting wire of the plate, and found the deviation of the
galvanometer 42°·5. I then broke this connexion, and
placed a double ball of zinc over the farthest extremity of
the oval; the galvanometer again marked 42°·5. When
the circuit was completed with both together, the galva-
nometer only rose to 45°. Upon making or breaking
contact with either ball singly, while the other remained
in circuit, the needle only varied 2° or 3°. When the
positions of the double and single ball were changed, the
difference upon breaking the contact with it was 5°. After

four hours action, the plate was examined, and the pink copper was found deposited in two segments of circles, of which the balls had been the respective centres; and the remainder of the plate was covered with a precipitation of a redder colour. (fig. 2.) This thinner precipitation gave no indications of an increased substance as evidence of additional action at the points where the effects spreading from the two centres must be conceived to have been superimposed.

The plate having been once more cleaned, the double ball was placed nearly over its centre, and when connected with the galvanometer in circuit it affected it 55°. The single ball placed also nearly over the centre, but about two inches removed from the position of the double ball, affected it singly 45°. When the connexion was made with both the deflection increased to 57°. After four hours action with both the balls, a beautiful well-defined oval of pink copper precipitate was found immediately under the two balls, of the diameter of 4 inches by 3½ nearly, beyond which there was a general diffusion of darker red precipitate (fig. 3). The surface of this oval was perfectly smooth and compact, and presented no appearance of greater thickness or inequality at the centre than at any other part. The two balls seem to have acted as one oblong piece of generating metal would have done; and, upon the hypothesis of a radiant force from each, no increase was perceived at the points upon which the double set of rays might have been supposed to impinge.

I was desirous to repeat once more the experiments with the opposite sides of the conducting plate, and to connect them more closely with some of the previous series, by measuring the effects upon the calorific galvanometer, and by varying the form of the plate. For this purpose, I took a circular plate of copper of the same diameter as the sphere, and immersed it in the copper solution. A zinc ball of 1½ inch diameter, placed in a membrane of acid over the centre, gave the following results:—

	First Series.	Second Series.
Ball at top............	79	82
Ball at middle	89	92
Ball at bottom	103	105

After these experiments, the copper was found diffused over both sides of the plate, but did not extend to the centre of the under side.

The upper surface of the plate having been thoroughly covered with lac varnish, the experiment was repeated with the following results:—

Ball at top 69
Ball at middle 75
Ball at bottom 70

The precipitated copper was here found deposited upon the under side in a ring about two inches in breadth from the edge, and there was very little in the centre of the plate, and none upon the upper surface.

I finally covered the under surface with varnish, leaving the upper exposed, with the following results:—

Ball at top .... × 73
Ball at middle 83
Ball at bottom 93

Hence it appears, that the under surface, which by itself is capable of sustaining an action from the ball in the centre of the solution nearly as great as the upper surface, when combined with the latter, adds no more than 10%, or about one-eighth to its efficiency. It appears also that, whereas with the upper surface, the action increases in some inverse ratio of the distance, of the generating from the conducting surface, with the under surface, there is a maximum point, on both sides of which it decreases. This point is doubtless dependent upon the angle at which the force, which radiates (as it were) from the ball, meets the edge of the plate.

Wishing now to draw the preceding experiments into closer comparison with similar ones which had hitherto, generally, been made in a different form, I had a square glass cell constructed, measuring 3½ inches wide and four deep. This was cut in two in the middle, and the edges having been ground, admitted of being clamped together, with the interposition of a piece of bladder. When thus put together it formed a cell divided into two by a diaphragm of membrane in the liquid, on either side of which different generating and conducting plates might be immersed. The forms which I selected were copper and zinc plates 3½ inches square, and copper and zinc balls of one inch diameter: the results obtained by different combinations of these, at different distances apart, measured by the galvanometer, are contained in the following Table. The electrolyte which was employed was, in one

series, the standard acid on both sides of the diaphragm,
and in the other the same acid in contact with the zinc,
and the solution of copper with the copper. In the near
distance the surfaces were about half an inch apart; in the
farther, three inches; and the balls were always placed
opposite to the centre of the associated plate.

	Equal Plates.		Copper Plate and Zinc Ball.		Zinc Plate and Copper Ball.		Equal Balls.	
	Close.	Distant.	Close.	Distant.	Close.	Distant.	Close.	Distant.
Acid alone . . .	15	15	81	10	8	7	9	7
Acid & Sol. Cop.	75	51	67	53	32	30	26	24

The precipitated copper in all these cases had been
pretty evenly diffused over the near surface of the plates,
and was to be traced to all parts of the opposite surface,
but more upon the edges than towards the centres.

Upon this Table we may remark :— ·

1st. That the energy of the force was about sextupled
by the absorption of the hydrogen upon the conducting
surface, except in the case of the equal plates, when it
was more than quadrupled.

2nd. That the effect of distance was much more decided
in the instances where the amount of the circulating force
was greater, than in the contrary cases.

3rd. That the amount of force put into circulation from
a large surface of zinc towards a central ball of copper
was, as in former instances of similar combinations, about
one half of that from the reverse arrangement.

4th. That a ball of zinc exposing a surface of 3·14
square inches placed over the centre of a plate of copper
exposing on its two sides a surface of twenty-eight square
inches, sustained an action of nearly the same amount as
a plate of zinc of the same dimensions as the copper,
placed at the same distances.

This result, as well as the small effect produced in pre-
ceding experiments by substituting two equal balls, or a
rod, for one ball of zinc, may, upon the supposition of a
force of the nature of a radiant force, be probably ex-
plained by the interference of the rays at points where
their directions cross each other.

Let A B C D, fig. 4, represent the sphere, and *a* and *b* two
active points within ; the force radiates from *a* to *c* and A
and *d*, and from *b* to *e* and *f* and C without interruption.

But the rays proceeding from *a* to *g* and from *b* to *g*, encounter one another, and the force would appear to be directed in the diagonal of the two, or from *g* to B, and in a direction parallel to *a c* and *b c*.

A rod may obviously be considered as a succession of such balls or radiant points, and hence the force would be propagated in a direction at right angles to its axis towards a circumscribing sphere or cylinder.

Let *a b c d*, fig. 5, represent the radiant points, then will the rays *a g* and *b g* interfere at *g* and pass on to *i*, *b f* and *c f* to B, and *c e*, *d e* to *l*. Of the same letters may represent radiant points in a plate A C, the rays of which will thus pass in a parallel direction to the opposite points of a conducting plate D E.

Without attaching, however, any importance to the geometrical diagrams, I would merely suggest that the resultants of all the radiant points acting from *a* to *d* towards D E may be parallel, and that such an hypothesis would account for the phenomena. The demonstration of this, if possible, would go far beyond my power in mathematical science.

The rays thus supposed to pass between two qual-plates become parallel, and hence the decrease of the force will be directly as the distance, as Mr. SNOW HARRIS found it by direct experiment.

Now nearly the whole of the preceding experiments, except those of the zinc sphere, had been made before I had the pleasure of reading the Eleventh of your Series of Experimental Researches on Electric Induction;[*] and I had been led to the supposition, which I believe I mentioned to you, that the force which is developed by voltaic combinations might be subject to the law of radiant forces; but I had been utterly at a loss to understand how, upon this hypothesis, it could extend its influence to the side of a plate opposite to that to which it was directed in right lines; how, in short, (to make use of a term which you have happily employed to describe, what I now believe to be, a perfectly analogous phenomenon), it could "turn a corner." Since the perusal of that paper, however, every thing seems to me to fall in so naturally with the general views which you have therein explained, that I almost feel as if I were intruding upon ground which is properly your own in venturing to apply the laws which you have estab-

lished of the " Essential and Fundamental Principle of
Induction,"* to the explanation of some of the foregoing
results. Supposing my views to be correct, you must
have been led to them in the natural course of your inves-
tigations; and nothing in my own opinion could justify my
interference in a work which must have been more com-
pletely performed by you, but the circumstance that I was
led to it by the obvious direction of my own previous
inquiries into voltaic combinations.

The direction of the force through an electrolyte placed
between a generating and conducting surface of two
metals, may, I conceive, be expressed in the very words
which you have employed to describe that of the *direct
inductive force* in statical electricity.

" It may be conceived to be exerted in lines between
the two limiting conducting surfaces, and is accompanied
by a lateral or traverse force equivalent to a dilation or
repulsion of these representative lines ; or the attractive
force which exists amongst the particles of the electrolyte
(dielectric) in the direction of the current (induction) is
accompanied by a repulsive or diverging force in the
transverse direction†."

The proof of this is exactly of the same nature as that
which you have brought forward in the parallel instance
of induction, namely, the turning round the corner of a
plate ; and I cannot but advert to the complete analogy of
the case in which brought your carrier ball near to the
middle of a flat disc of metal placed upon an excited shell-
lac cylinder when no charge was communicated, although
one was obtained at the edge of the disc ; and that of the
deposition of a ring of precipitated copper round the edge
of the under surface of a brass plate while the centre was
free from it (fig. 1.).

This "lateral tension of the lines of force on one ano-
ther" is quite consistent with their *divergence* from an
active centre; may it not even be considered as the cause
of their radiation; it is most particularly evinced by the
results of those experiments, in which the immediate
divergence of the force from the active centre was pre-
vented by placing the latter in a glass tube, or by drawing
it up above the general level of the surrounding electrolyte.
In these instances the first impulse must have been pro-
pagated in a perpendicular direction; but the instant it

was at liberty to influence the general mass, the molecules of the latter were thrown into the polarized state, and the direction of the force opened out as from a centre.

• On the other hand, the same "repulsive force in a transverse direction" must be opposed to the *convergence* of the lines from an active sphere towards an interior conducting point, when the force is not stationary but current : may not this opposition account for the reduced action of a sphere of zinc upon a ball of copper ? The difference of the *statical induction* and the *current induction* is, that in the former the force is not progressive, while in the latter it is in a state of perpetual flux ; the state of polarity, however, and of tension, is maintained in both.

The transfer of the elements of the electrolyte in opposite directions under that peculiar molecular arrangement or polarity, "which is the first step in all electrolyzation," is quite compatible with their unequal distribution upon the limiting conducting surfaces, according to the varying relations of their dimensions and distances, as was evidenced by the unequal precipitation of the reduced copper in several of the preceding instances ; but no correspondent inequality of the force can exist upon the surface of the conductors themselves, upon all parts of which it can instantaneously distribute itself with comparative facility.

The principal circumstance which might be supposed to limit the power of an active point within a conducting sphere in any given electrolyte, is the resistance of that electrolyte, which increases in a certain ratio to its depth or thickness ; and this thickness may be considered virtually the same, wherever the included point may be placed, but to increase with the diameter of the sphere. It is also the same, and consequently the resistance is also the same, when placed anywhere within the plane which divides the sphere into two hemispheres. But in an insulated hemisphere, the approximation of the active point to the lower surface virtually decreases the thickness of the electrolyte through which its action has to be propagated, by increasing the extent of surface which cuts the divergent lines of force; and consequently the force increases. In this respect the action of a point upon a plate may be considered the same as upon an indefinitely large hemisphere, towards which as the point approaches the force increases.

It appears to me that practical consequences of some importance flow from the preceding conclusions, upon which I may be tempted to address you again at no distant period.

I remain, my dear Faraday,
Ever faithfully yours,
J. F. DANIELL.

King's College, January 13, 1838.

Contributions to Electricity and Magnetism, No IV. On Electro-dynamic Induction. (Continued.) By JOSEPH HENRY, L.L.D., Professor of Natural Philosophy in the College of New Jersey, Princeton.[*]

Introduction.

1. IN the course of my last paper, it was stated that the investigations which it detailed were not as complete in some parts as I could wish, and that I hoped to develope them more fully in another communication. After consi- derable delay, occasioned by alterations in the rooms of the physical department of the college, I was enabled to resume my researches, and since then I have been so fortunate as to discover a series of new facts belonging to different parts of the general subject of my contributions. These I have announced to the Society at different times, as they were discovered ; and I now purpose to select from the whole such portions as relate particularly to the prin- ciple subject of last paper, namely, the induction at the beginning and ending of a galvanic current, and to present to them as a continuation, and, in a measure, as the com- pletion, of this part of my researches. The other results of my labours in this line will be arranged for publication as my duties will permit me to give them a more carefu examination.

2. In the course of the experiments I am about to describe, I have had occasion to repeat and vary those given in my last paper ; and I am happy to be able to state, in reference to the results, that, except in some minor particulars, which will be mentioned in the course of this paper, I have found no cause to desire a change in the accounts before published. My views, however, of

the connexion of the phænomena have been considerably modified, and I think rendered much more definite by the additional light which the new facts have afforded.

3. The principal articles of apparatus used in these experiments are nearly the same as those described in my paper, namely, several flat coils and a number of long wire helices (III. 6, 7, 8.*) I have, however, added to these a constant battery, on Professor Daniel's plan, the performance of which has fully answered my expectations, and confirmed the accounts given of this form of the instrument by its author. It consists of thirty elements formed of as many copper cylinders, open at the bottom, each five inches and a half in height, three inches and a half in diameter, and placed in earthen cups. A zinc rod is suspended in each of these, of the same length as the cylinders, and about one inch diameter. The several elements are connected by a thick copper wire, soldered to the copper cylinder of one element, and dipping into a cup of mercury on the zinc of the next. The copper and zinc, as usual, are separated by a membrane, on both sides of which is placed a solution of one part of the sulphuric acid in ten parts of water; and to this is added, on the side next the copper, as much sulphate of copper as will saturate the solution. The battery was sometimes used as a single series, with all its elements placed consecutively, and at others in two or three series, arranged collaterally, so as to vary the quantity and intensity of the electricity, as the occasion might require.

4. The galvanometers mentioned in this paper, and referred to in the last, are of two kinds; one, which is used with a helix to indicate the action of an induced current of intensity, consists of about five hundred turns of fine copper wire, covered with cotton thread, and more effectually insulated by steeping the instrument in melted cement, which was drawn into the space between the spires by capillary attraction. The other galvanometer is formed of about forty turns of a shorter and thicker wire, and is always used to indicate an induced current, of considerable quantity, but of feeble intensity. The needle of both these instruments is suspended by a single fibre of raw silk.

5. I should always state, that in all cases where a magnetizing spiral is mentioned in connexion with a helix, the

article is formed of a long, fine wire, making about one hundred turns around the axis of a hollow piece of straw, of about two inches and a half long: also the spiral mentioned in connexion with a coil is formed of a short wire, which makes about twenty turns around a similar piece of straw. The reason of the use of the two instruments in these two cases is the same as that for the galvanometers, under similar circumstances, namely, the helix gives a current of intensity, but of small quantity, while the coil produces one of considerable quantity, but of feeble intensity.

SECTION I.—*On the Induction produced at the moment of the beginning of a Galvanic Current, &c.*

6. It will be recollected that the arrangement of apparatus employed in my last series of experiments gave a powerful induction at the moment of breaking the galvanic circuit, but the effect at making the same was so feeble as scarcely to be perceptible. I was unable in any case to get indications of currents of the third or fourth orders from the beginning induction, and its action was therefore supposed to be so feeble as not materially to affect the results obtained.

7. Subsequent reflection, however, led me to conclude, that in order to complete this part of my investigations, a more careful study of the induction at the beginning of the current would be desirable; and accordingly, on resuming the experiments, my attention was first directed to the discovery of some means by which the intensity of this induction might be increased. After some preliminary experiments, it appeared probable that the desired result could be obtained by using a compound galvanic battery, instead of the single one before employed. In reference to this conjecture the constant battery before mentioned (3.) was constructed, and a series of experiments instituted with it, the results of which agreed with my anticipation.

8. In the first experiment, coil No. 2, which it will be remembered (III. 7.) consists of a copper riband of about sixty feet long, and coiled on itself like the main spring of a watch, was connected with the compound battery, and helix No. 1 (III. 8.), formed of one thousand six hundred and sixty yards of fine copper wire, was placed on the coil to receive the induction, as is shown in Figure 1, which is again inserted in the next page for the convenience of the reader.

Fig. 1.
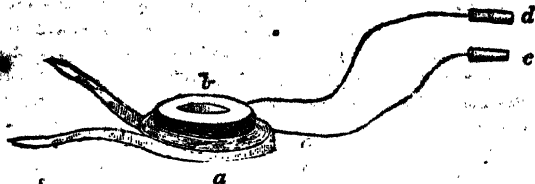
a represents No. 1, *b* helix No. 1, and *c*, *d*, handles for receiving the shock.

This arrangement being made, currents of increasing intensity were passed through the coil by constantly retaining one of its ends in the cup of mercury forming one extremity of the battery, and successively plunging the other end into one of the cups which served to form the connexions of the several elements of the battery. With the current from one element, the shock of breaking the circuit was quite severe; but at making the same it was very feeble, and could be perceived in the fingers only or through the tongue. With two elements in the circuit, the shock at beginning was slightly increased; with three elements the increase was more decided, while the shock at breaking the circuit remained nearly of the same intensity as at first, or was comparatively but little increased. When the number of elements was increased to *ten*, the shock at making contact was found fully equal to that at breaking, and by employing a still greater number, the former was decidedly greater than the latter; the difference continually increasing until all the thirty elements were introduced into the circuit.

9. In my last paper, a few experiments are mentioned as being made with a compound battery of Cruickshank's construction; but from the smallness of the plates of this, and the rapidity with which its power declined, I was led into the error of supposing that the inducton at the ending of the current, in the case of a short coil, was diminished by increasing the intensity of the battery (see paragraph 19. of No. III.); but by employing the more perfect instrument of Professor Daniell in the arrangement of the last experiment, I am enabled to correct this error, and to state that the induction at the ending remains nearly the same when the intensity of the battery is increased. If the induction depends in any degree on the quantity of current electricity in the conductor, then a

slight increase in the induction should take place; since, according to theory, the current is somewhat increased in quantity in the case of a long coil, by the increase of the intensity of the battery. Although very little, if any, difference could be observed in the intensity of the shock from the secondary current, yet the snap and deflagration of the mercury appeared to be greater from the primary current when *ten* elements of the battery were included in the circuit, than with a single one. The other results which are mentioned in my last paper, in reference to the compound battery, are, I believe, correctly given.

10. The intensities of the different shocks in the foregoing experiments were compared by gradually raising the helix from the coil (see Fig. 1), until, on account of the distance of the conductors, the shock in one case would be so much reduced as to be scarcely perceptible through the fingers or the tongue, while the shock from another arrangement, but with the same distance of the conductors, would be evident, perhaps, in the hands. The same method was generally employed in the experiments in which shocks are mentioned as being compared, in other parts of this paper.

11. Experiments were next made to determine the influence of a variation in the length of the coil, the intensity of the battery remaining the same. For this purpose, the battery consisting of a single element, and the arrangement of the apparatus as represented in Fig. 1, the coil was diminished in length from sixty feet to forty-five, then to thirty, and so on. With the first-mentioned length the shock, at making contact with the battery, was, of course, very feeble, and could be felt only in the tongue; with the next shorter length it was more perceptible, and increased in intensity with each diminution of the coil, until a length of about fifteen feet appeared to give a maximum result.

12. The diminution of the intensity of the shock in the last experiment, after the length of the coil was diminished below fifteen feet, was due to the diminution of the number of spires of the coil, each of which, by acting on the helix, tends to increase the intensity of the secondary current, unless the combined length of the whole is too great for the intensity of the battery. That this is the fact is shown by the following experiment: the helix was placed on a single spire or turn of the coil, and the length of the other part of the copper riband, which did not act on the

helix, was continually shortened, until the whole of it was excluded from the circuit; in this case the intensity of the shock at the beginning was constantly increased. We may therefore state generally, that, in the beginning of the battery current, the induction of a unit of its length is increased by every diminution of the length of the conductor.

13. In the experiment given in paragraph 11, the intensity of the shock at the *ending* of the battery current diminishes with each diminution of the length of the coil; and this is also due to the decrease of the number of the spires of the coil, as is evident from an experiment similar to the last, in which the helix was placed on a coil consisting of only two turns or spires of copper riband; the shock at the ending, with this arrangement, was comparatively feeble, but could be felt in the hands. Different lengths of coil No. 2, were now introduced into the same circuit, but not so as to act on the helix; but although these were varied from four or five feet to the whole length of the coil (sixty feet) not the least difference in the intensity of the shock could be perceived. We have, therefore, the remarkable result, that the intensity of the ending induction of each unit of length of the battery current is not materially altered, at least within certain limits, by changing the length of the whole conductor. From this we would infer that the shock depends more on the intensity of the action than on the quantity of the current, since we know that the latter is diminished in a given unit of the conductor by increasing the length of the whole.

14. We have seen (8.) that, with a circuit composed of ten elements of the compound battery and the coil No. 2, the shock, at the beginning of the current, was fully equal to that at the ending. It was, however, found, that if, in this case, the length of the coil was increased, this shock was diminished; and we may state, as an inference from several experiments, that however great may be the intensity of the electricity from the battery, the shock at the beginning may be so reduced, by a sufficient increase of the length of the primary circuit, as to be scarcely perceptible.

15. It was also found, that when the thickness of the coil was increased, the length and intensity of the circuit remaining the same, the shock at the beginning of the current was somewhat increased. This result was produced by using a double coil; the electricity was made to pass through one strand, and immediately afterwards

through both : the shock from the helix in the latter case was apparently the greater.

16. By the foregoing results we are evidently furnished with two methods of increasing, at pleasure, the intensity of the induction at the beginning of a battery current; the one consisting in increasing the intensity of the source of the electricity, and the other in diminishing the resistance to conduction of the circuit while the intensity remains the same.

17. The explanation of the effects which we have given, relative to the induction at the beginning, is apparently not difficult. The resistance to conduction in the case of a long conductor and a battery of a single element is so great, that the full development of the primary current may be supposed not to take place with sufficient rapidity to produce the instantaneous action on which the shock from the secondary current would seem to depend. But when a battery of a number of elements is employed, the poles of this, previous to the moment of completing the circuit, are in a state of electrical tension; and therefore the discharge through the conductor may be supposed to be more sudden, and hence an induction of more intensity is produced.

18. That the shock at both making and breaking the circuit in some way depends on the rapidity of formation and diminution of the current, is shown by the following experiment, in which the tension just mentioned does not take place, and in which, also, the current appears to diminish more slowly. The two ends of the coil were placed in the two cups which formed the poles of the battery, and permanently retained there during the experiment; also, at the distance of about six inches from, say the right hand end of the coil, a loop was made in the riband, which could be plunged into the cup containing the left hand end. With this arrangement, and while only the two extreme ends of the coil were in connexion with the cups of mercury, of course the current passed through the entire length of the riband of the coil; but by plunging the loop into the left hand cup, the whole length of the coil, except the six inches before mentioned, was excluded from the battery circuit. And again, when the loop was lifted out of the cup, the whole length included. In this way the current in the coil could be suddenly formed and interrupted, while the poles of the battery were continually joined by a conductor; but no shock with either single or

a compound battery could be obtained by this method of operation.

19. The feebleness of the shock at the beginning of the current, with a single battery and a long coil, is not entirely owing to the cause we have stated, (17.) namely, the resistance to conduction offered by the long conductor, but also depends, in a considerable degree, if not principally, on the adverse influence of the secondary current, induced in the primary conductor itself, as is shown by the result of the following experiment. Helix No. 1. was placed on a coil consisting of only three spires or turns of copper riband; with this, the shock, both at making and breaking the circuit with a single battery, could be felt in the hands. A compound coil was then formed of the copper ribands of coils No. 3. and 4. rolled together so that the several spires of the two alternated with each other, and when this was introduced into the circuit so as not to act on the helix by its induction, and the battery current passed through, for example, coil No. 3, the shock at making contact with the pole of the battery was so much reduced as to be imperceptible in the hands, while the shock at breaking the contact was about the same as before this addition was made to the length of the circuit. The ends of coil No. 4, were now joined so as to produce a closed circuit, the induced current in which would neutralize the secondary current in the battery conductor itself; and now the shock at making the contact was nearly as powerful as in the case where the short conductor alone formed the circuit with the battery. Hence, the principal cause of the feebleness of the effect at the beginning of the battery current, is the adverse action on the helix of the secondary current produced in the conductor of the battery circuit itself. The shock at the breaking of the circuit, in this experiment, did not appear affected by joining or separating the ends of coil No. 4.

20. Having investigated the conditions on which the inductive action at the beginning of a battery current depends, experiments were next instituted to determine the nature of the effects produced by this induction: and first the coils were arranged in the manner described in my last paper (III. 79.), for producing currents of the different orders. The result with this arrangement was similar to that which I have described in reference to the ending induction, namely, currents of the third, fourth, and fifth orders were readily obtained.

21. Also, when an arrangement of apparatus was made similar to that described in paragraph 87 of my last paper, it was found that a current of intensity could be induced from one of quantity and the converse.

22. Likewise, the same screening or rather neutralizing effect was produced, when a plate of metal was interposed between two consecutive conductors of the series of currents, as was described (III. Section IV.) in reference to the ending induction. In short, the series of induced currents produced at the beginning of the primary current appeared to possess all the properties belonging to those of the induction at the ending of the same current.

23. I may mention, in this place, that I have found, in the course of these experiments, that the neutralizing power of a plate of metal depends, in some measure, on its superficial extent. Thus a broad plate which extends, in every direction, beyond the helix and coil, produces a more perfect screening than one of the same metal and of the same thickness, but of a diameter only a little greater than that of the coil.

24. The next step in the investigation was to determine the direction of the currents of the different orders produced by the beginning induction, and for this purpose the magnetizing spirals (5.) were used, and the results obtained by these verified by the indications of the galvanometer. It should be stated here, as a fact which was afterwards found of some importance, that although the needle of the galvanometer was powerfully deflected when the instrument was placed in the circuit of the secondary current, yet a very feeble effect was produced on it by the action of the current of the third, fourth, or fifth order. The directions, however, of these currents, as indicated by the feeble motion of the needle, were the same as those given by the magnetizing spiral.

25. The direction of the different currents produced at the making of the battery current, as determined by these instruments, is as follows: namely, the direction of the secondary current is, as stated by Dr. Faraday, adverse to that of the primary current, and, also, the direction of each succeeding current is opposite to that of the one which produced it. We have, therefore, from these results, and those formerly obtained (III. 92.), the following series of directions of currents, one produced at the moment of beginning, and the other at that of ending of the battery current.

	At the beginning	At the ending.
Primary current	+	+
Secondary current .....•...	—	+
Current of the third order..	+	—
Current of the fourth order	—	+
Current of the fifth order ...	+	—

26. These two series, at first sight, may appear very different; but, with a little attention, they will be seen to be of the same nature. If we allow that the induction at the ending of a galvanic battery should be opposite to that at the beginning of the same, then the sign at the top of the second column may be called minus instead of plus, and we shall have the second series — + — + alternating precisely like the first.

27. In connexion with the results given in the last two paragraphs, it is due to Mr. Sturgeon that I should state that, in a letter addressed to me, and published in the Annals of Electricity, he has predicted, from his theory, that I would find on examination, the series of alternation of currents for the beginning induction which I have here given. I may, however, add, that it appears to me that this result might have been predicted without reference to any theory.† There was no reason to suppose the induction at the beginning would be different in its nature from that at the ending, and therefore the series which would be produced from the former might be immediately inferred from that belonging to the latter, by recollecting that the direction of the induction at the beginning should be opposite to that at the ending. I do not wish it to be supposed, however, from this remark, that I had, myself, drawn any inference from my experiments as to the alternations of currents which might be produced by the beginning induction; the truth is, that this action was so feeble with the arrangement of apparatus I employed, that I supposed it could not produce a series of currents of the different orders.

28. In the course of the experiments given in this section, I have found that a shock can be produced without using a coil, by arranging about ten elements of the battery in the form of a circle, and placing the helix within this. The shock was felt in the hands at the moment of closing the circuit, but the effect at opening the same was scarcely perceptible through the tongue. An attempt was also made to get indications of induction by

† We have not time at present to make any remarks on this point, but they may be expected in the next No.—EDIT.

placing the helix within a circle of dilute acid, connected with a battery instead of a coil, but the effect, if any, was very feeble.

29. I have shown, in the second number of my contributions, that if the body be introduced into a circuit with a battery of one hundred and twenty elements, without a coil, a thrilling sensation will be felt during the continuance of the current, and a shock will be experienced at the moment of interrupting the current by breaking the circuit at any point. This result is evidently due to the induction of a secondary current battery itself, and on this principle the remarkable physiological effects produced by Dr. Ure, on the body of a malefactor, may be explained. The body, in these experiments, was made to form a part of a circuit, with a compound galvanic apparatus in which a series of interruptions was rapidly made by drawing the end of the conductor over the edges of the plates of the battery. By this operation a series of induced currents must have been produced in the battery itself, the intensity of which was greater than that of the primary current.

30. In this connexion I may mention that the idea has occurred to me, that the intense shocks given by the electrical fish may possibly be from a secondary current, and that the great amount of nervous organization found in these animals may serve the purpose of a long conductor.* It appears to me, that in the present state of knowledge, this is the only way in which we can conceive of such intense electricity being produced in organs imperfectly insulated and immersed in a conducting medium. But we have seen that an original current of feeble intensity can induce, in a long wire, a secondary current capable of giving intense shocks, although the several strands of the wire are separated from each other only by a covering of cotton thread. Whatever may be the worth of this suggestion, the secondary current affords the means of imitating the phænomena of the shock from the electrical eel, as described by Dr. Faraday. By immersing the apparatus (Fig. 1,) in a shallow vessel of water, the handles being placed at the two extremities of the diameter of the helix, and the hands plunged into the water parallel to a line joining the two poles, a shock is felt through the arms; but when the contact with the water is made in a line at right

* Since writing the above, I found that M. Masson has suggested the same idea in an interesting thesis lately published.

angles to the last, only a slight sensation is felt in each
hand, but no shock.

31. Since the publication of my last paper, I have ex-
hibited to my class the experiment (No. III.. Sect. III.)
relative to the induction at a distance on a much larger
scale. All my coils were united so as to form a single
length of conductor of about four hundred feet, and this
was rolled into a ring of five and a half feet in diameter,
and suspended vertically against the inside of the large
folding door which separate the laboratory from the lecture-
room. On the other side of the doors, in the lecture-
room, and directly opposite the coil, was placed a helix,
formed of upwards of a mile of copper wire, one-sixteenth
of an inch in thickness, and wound into a hoop of four
feet in diameter. With this arrangement, and a battery of
one hundred and forty-seven square feet of zinc surface
divided into eight elements, shocks were perceptible in the
tongue, when the two conductors were separated, to the
distance of nearly seven feet; at the distance of between
three and four feet, the shocks were quite severe. The
exhibition was rendered more interesting by causing the
induction to take place through a number of persons
standing in a row between the two conductors.

SECT. II.— *On apparently two kinds of Electro-dymanic Induction.*

32. The investigations arranged under this head had
their origin in the following circumstances. After the
publication of my last paper, I received, through the kind-
ness of Dr. Faraday, a copy of the fourteenth series of
his researches, and in this I was surprised to find a state-
ment which appeared in direct opposition to one of the
principal facts of my communication. In paragraph 59,
I state, in substance, that when a plate of metal is inter-
posed between the coil transmitting a galvanic current,
and the helix placed above it to receive the induction; the
shock from the secondary current is almost perfectly
neutralized. Dr. Faraday, in the extension of his new
and ingenious views of the agency of the intermediate
particles in transmitting induction, was led to make an
experiment on the same point; and apparently, under the
same circumstances, he found that it " makes not the least
difference, whether the intervening space between the two
conductors is occupied by such insulating bodies as air,

sulphur, and shell-lac, or such conducting bodies as copper
and other non-magnetic metals."

33. As the investigation of the fact mentioned above
forms an important part of my paper, and is intimately
connected with almost all the phænomena subsequently
described in the communication, I was, of course,
anxious to discover the cause of so remarkable a discre-
pancy. There could be no doubt of the truth of my
results, since a shock from a secondary current which
would paralyze the arms was so much reduced by the in-
terposition of plates of metal, as scarcely to be felt
through the tongue.

34. After some reflection, however, the thought occurred
to me that induction might be produced in such a way as
not to be affected by the interposition of a plate of metal.
To understand this, suppose the end of a magnetic bar
placed perpendicularly under the middle of a plate of
copper, and a helix suddenly brought down on this; an
induced current would be produced in the helix by its
motion towards the plate, since the copper, in this case,
could not screen the magnetic influence. Now, if we
substitute for the magnet a coil through which a galvanic
current is passing, the effect should be the same. The
experiment was tried by attaching the ends of the helix to
a galvanometer,* and the result was as I expected; when
the coil was suddenly brought down on the plate the needle
swung in one direction, and when lifted up, in the other;
the amount of deflection being the same, whether the plate
was interposed or not.

35. It must be observed in this experiment, that the
plate was at rest, and consequently did not partake of the
induction produced by the motion of the helix. From my
previous investigations, I was led to conclude that a
different result would follow, were a current also generated
in the plate by simultaneously moving it up and down with
the helix. This conclusion, however, was not correct, for
on making the experiment, I found that the needle was
just as much affected when the plate was put in motion
with the helix as when the latter alone was moved.

36. This result was so unexpected and remarkable, that
it was considered necessary to repeat and vary the experi-

* The arrangement will be readily understood by supposing in Fig. 1.
the handles removed, and the ends of the helix joined to the ends of the
wire of a galvanometer; also, by a plate of metal interposed between the
helix and the coil.

ment in several ways. First, a coil was interposed
instead of the plate, but whether the coil was at rest or in
motion with the helix, with its ends separated or joined,
the effect on the galvanometer was still the same; not the
least screening influence could be observed. In reference
to the use of the coil in this experiment, it will be recol-
lected that I have found this article to produce a more
perfect neutralization than a plate.

37. Next, the apparatus remaining the same, and the
helix at rest during the experiment, currents were induced
in it by moving the battery attached to the coil up and
down in the acid. But in this case, as in the others, the
effect on the galvanometer was the same, whether the
plate or coil was interposed or not.

38. The experiment was also tried with magneto-elec-
tricity. For this purpose, about forty feet of copper wire,
covered with silk were wound around a short cylinder of stiff
paper, and into this was inserted a hollow cylinder of sheet
copper, and into this again, a short rod of soft iron; when
the latter was rendered magnetic, by suddenly bringing in
contact with its two ends the different poles of two mag-
nets, a current, of course, was generated in the wire, and
this, as before, was found to affect the galvanometer to the
same degree when the copper cylinder was interposed, as
when nothing but the paper intervened.

39. The last experiment was also varied by wrapping
two copper wires of equal length around the middle of the
keeper of a horse-shoe magnet, leaving the ends of the
inner one projecting, and those of the outer attached to
the galvanometer. A current was generated in each by
moving the keeper on the ends of the magnet, but the effect
on the galvanometer was not in the least diminished by
joining the ends of the inner wire.

40. At first sight, it might appear that all these results
are at variance with those detailed in my last paper, rela-
tive to the effect of interposed coils and plates of metal.
But it will be observed, that in all the experiments just
given, the induced currents are not the same as those
described in my last communication. They are all pro-
duced by motion, and have an appreciable duration, which
continues as long as the motion exists. They are also of
low intensity, and thus far I have not been able to get
shocks by any arrangement of apparatus from currents of
this kind. On the other hand, the currents produced at
the moment of *suddenly* making or breaking a galvanic

current, are of considerable intensity, and exist but for an instant. From these, and other facts presently to be mentioned, I was led to suppose that there are two kinds of electro-dynamic induction ; one of which can be neutralized by the interposition of a metallic plate between the conductors and the other not.

41. In reference to this surmise, it became important to examine again all the phænomena of induction at suddenly making and breaking a galvanic current.* And in connexion with this part of the subject, I will first mention a fact which was observed in the course of the experiments given in the last section, on the direction of the induced currents of different orders. It was found, that though the indications of the galvanometer were the same as those of the spiral, in reference to the direction of the induced currents, yet they were very different in regard to the intensity of the action. Thus, when the arrangement of the apparatus was such that the induction at making the battery circuit was so feeble as not to give the least magnetism to the needle, and so powerful at the ending as to magnetize it to saturation, the indication of the galvanometer was the same in both cases.

42. Also, similar results were obtained in comparing the shock and the deflection of the galvanometer. In one experiment, for example, the shock was so feeble at making contact that it could scarcely be perceived in the fingers, but so powerful at the breaking of the circuit as to be felt in the breast ; yet the galvanometer was deflected about thirty-five degrees to the right, at the beginning of the current, and only an equal number of degrees to the left, at the ending of the same.

43. In another experiment, the apparatus being the same as before, the magnetizing spiral and the galvanometer were both at once introduced into the circuit of the helix. A sewing needle being placed in the spiral, and the contact with the battery made, the needle showed no signs of magnetism, although the galvanometer was deflected thirty degrees. The needle being replaced, and the battery circuit broken, it was now found strongly magnetized, while the galvanometer was only moved about as much as before in the opposite direction.

44. Also, effects similar to those described in the last two paragraphs were produced when the apparatus was so

arranged as to cause the induction at the beginning of the battery current to predominate. In this case the galvanometer was still nearly equally affected at making and breaking battery contact, or any difference which was observed could be referred to a variation in the power of the battery during the experiment.

45. Another fact of importance belonging to the same class has been mentioned before (24.), namely, that the actions of the currents of the third, fourth, and fifth orders produce a very small effect on the galvanometer, compared with that of the secondary current; and this is not alone on account of the diminishing power of the successive inductions, as will be evident from the following experiment. By raising the helix from the coil, in the arrangement of apparatus for the secondary current, the shock was so diminished as to be inferior to one produced by the arrangement for a tertiary current; yet, while with the secondary current the needle was deflected twenty-five degrees, with the tertiary it scarcely moved more than one degree; and with the currents of the fourth and fifth orders the deflections were still less, resembling the effect of a slight impulse given to the end of the needle.

46. With the light obtained from the foregoing experiments, I was the more fully persuaded that some new and interesting results might be obtained by a re-examination of my former experiments, on the phænomena of the interposed plate of metal, in the case where the induction, was produced by making and breaking the circuit with a cup of mercury; and in this I was not disappointed. The coil (Fig. 1.) being connected with a battery of ten elements, the shocks, both at making and breaking the circuit, were very severe; and these, as usual, were almost entirely neutralised by the interposition of a zinc plate. But when the galvanometer was introduced into the circuit instead of the body, its indications were the same whether the plate was interposed or not; or, in other words, the galvanometer indicated no screening, while, under the same circumstances, the shocks were neutralized.

47. A similar effect was observed when the galvanometer and the magnetizing spiral were together introduced into the circuit. The interposition of the plate entirely neutralized the magnetizing power of the spiral, in reference to tempered steel, while the deflections of the galvanometer were unaffected.

48. In order to increase the number of facts belonging to this class, the last experiments were varied in several

ways; and first, instead of the hard steel needle, one of soft iron wire was placed in the spiral, with a small quantity of iron filings almost in contact with one of its ends. The plate being interposed, the small particles of iron were attracted by the end of the needle, indicating a feeble, temporary developement of magnetism. Hence the current which moves the needle, and is not neutralized by the interposed plate, also feebly magnetizes soft iron, but not hard steel.

49. Again, the arrangement of apparatus being as in paragraph 46, instead of a plate of zinc; one of cast iron, of about the same superficial dimensions, but nearly half an inch thick, was interposed; with this the magnetizing power of the spiral, in reference to tempered steel, was neutralized; and, also, the action of the galvanometer was much diminished.

50. Another result was obtained by placing in the circuit of the helix (fig. 1.), at the same time, the galvanometer, the spiral, and a drop of distilled water; with these the magnetizing power of the spiral was the same as without the water, but the deflection of the galvanometer was reduced from ten to about four degrees. In addition to these, the body was also introduced into the same circuit; the shocks were found very severe, the spiral magnetized needles strongly, but the galvanometer was still less moved than before. The current of low intensity, which deflects the needle of the galvanometer in these instances, was partially intercepted by the imperfect conduction of the water and the body.

51. To exhibit the results of these experiments with still more precision, an arrangement of apparatus was adopted similar to that used by Dr. Farraday, and described in the fourteenth series of his researches, namely, a double galvanometer was formed of two separate wires of equal length and thickness, wound together on the same frame; and, also, a double magnetizing spiral was prepared by winding two equal wires around the same piece of hollow straw. Coil No. 1*, connected with the battery, was supported perpendicularly on a table, and coils Nos. 3 and 4 were placed parallel to this, one on each side, to receive the induction, the ends of these being so joined with those of the galvanometer and the spiral that the induced current from the one coil would pass through the

* See Annals, fig. 9, plate viii., vol. iv,

two instruments, in an opposite direction to that of the
current from the other coil. The two outside coils were
then so adjusted, by moving them to and from the middle
coil, that the induced currents perfectly neutralized each
other in the two instruments, and the needle of the gal-
vanometer and that in the spiral were both unaffected when
the circuit of the battery was made and broken. With
this delicate arrangement the slightest difference in the
action of the two currents would be rendered perceptible;
but when a zinc plate was introduced, so as to screen one
of the coils, the needle of the galvanometer still remained
perfectly stationary, indicating not the least action of the
plate, while the needle in the spiral became powerfully
magnetic. When, however, a plate of iron was interposed
instead of the one of zinc, the needle of the galvanometer
was also affected.

52. From the foregoing results it would seem that the
secondary current, produced at the moment of suddenly
beginning or ending of a galvanic current, by making and
breaking contact with a cup of mercury, consists of two
parts, which possess different properties. One of these is
of low intensity, can be interrupted by a drop of water,
does not magnatize hardened steel needles, and is not
screened by the interposition of a plate of any metal,
except iron, between the conductors. The other part is
of considerable intensity, is not intercepted by a drop of
water, developes the magnetism of hardened steel, gives
shocks, and is screened or neutralized by a closed coil, or
a plate of any kind of metal. Also, the induced current
produced by moving a conductor towards or from a
battery current, and that produced by the movement up
and down of a battery in the acid, are of the nature of the
first-mentioned part, while the currents of the third,
fourth, and fifth orders partake almost exclusively of the
properties of the second part.

53. The principal facts and conclusions of this section
were announced to the Society in October 1339, and
again presented in the form in which they were detailed
in the Annals of Electricity, Magnetism and Chemistry, &c.,
vol. IV., p. 281. Since then, however, I have had leisure
to examine the subject more attentively, and after a careful
comparison of these results with those before given, I
have obtained the more definite views of the phenomena
which are given in the next section.

SECT. III.—*Theoretical Considerations relating to the Phænomena described in this and the preceding Communications. Read November* 20, 1840.

54. The experiments given in the last No. of my contributions were merely arranged under different heads, and only such inferences drawn from them as could be immediately deduced without reference to a general explanation. The addition, however, which I have since made to the number of facts, affords the means of a wider generalization; and after an attentive consideration of all the results given in this and the preceding papers, I have come to the conclusion that they can all be referred to the simple laws of the induction at the beginning and the ending of a galvanic current.

55. In the course of these investigations the limited hypothesis which I have adopted have been continually modified by the development of new facts, and therefore my present views, with the further extension of the subject, may also require important corrections. But I am induced to believe, from its exact accordance with all the facts, so far as they have been compared, that if the explanation I now venture to give be not absolutely true, it is so, at least, in approximation, and will therefore be of some importance in the way of suggesting new forms of experiment, or as a first step towards a more perfect generalization.

56. To render the laws of induction at the beginning and the ending of a galvanic current more readily applicable to the explanation of the phænomena, they may be stated as follows:—1. During the time a galvanic current is increasing in quantity in a conductor, it induces, or tends to induce, a current in an adjoining parallel conductor in an opposite direction to itself. 2. During the continuance of the primary current in full quantity, no inductive action is exerted. 3. But when the same current begins to decline in quantity, and during the whole time of its diminishing, an induced current is produced in an opposite direction to the induced current at the beginning of the primary current.

57. In addition to these laws, I must frequently refer to the fact, that *when the same quantity of electricity in a current of short duration is passed through a galvanometer, the deflecting force on the needle is the same, whatever be the intensity of the electricity.* By intensity is here un-

derstood the ratio of a given quantity of force to the time in which it is expended[*] ; and according to this view, the proposition stated is an evident inference from dynamic principles. But it does not rest alone on considerations of this kind, since it has been proved experimentally by Dr. Faraday, in the third series of his researches.[†]

58. In order to form a definite conception of the several conditions of the complex phænomena which we are about to investigate, I have adopted the method often employed in physical inquiries, of representing the varying elements of action by the different parts of a curve. This artifice has been of much assistance to me in studying the subject, and without the use of it at present, I could scarcely hope to present any views in an intelligible manner to the Society.

59. After making these preliminary statements, we will now proceed to consider the several phænomena ; and, first, let us take the case in which the induction is most obviously produced in accordance with the laws as above stated (56.), namely, by immersing a battery into the acid, and also by withdrawing it from the same. During the time of the descent of the battery into the liquid, the conductor connected with it is constantly receiving additional quantities of current electricity, and each of these additions produces an inductive action on the adjoining secondary conductor. The amount, therefore, of induced current produced during any moment of time will be just in proportion to the corresponding increase in the current of the battery during the same moment. Also, the amount of induction during any moment while the current of the battery is diminishing in quantity will be in proportion to the decrease during the same moment.

60. The several conditions of this experiment may be represented by the different parts of the curve, A, B, C, D, fig. 2, in which the distances, A a, A b, A c, represent the times during which the battery is descending to different depths into the acid; and the corresponding ordinates, $a g$, $b h$, c B, represent the amount of current electricity in the battery conductor corresponding to these times. The differences of the ordinates, namely, $a g$, $m h$, n B, express the increase in the quantity of the battery current during the corresponding moments of time repre-

* Or more correctly speaking, the ratio of two quantities of the same species representing the force and time.
† See Annals of Electricity, &c. vol. 1., p. 32, for our opinion of this inference.—EDIT.

sented by A a, ab, bc; and since the inductive actions
(59.) are just in proportion to these increases, the same
differences will also represent the amount of induced ac-
tion exerted on the secondary conductor during the same
moment of time.

Fig. 2.
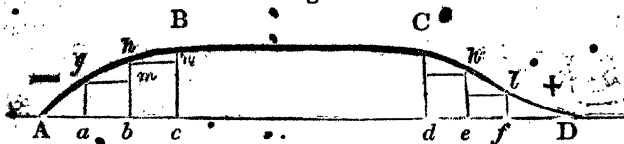
61. When the battery is fully-immersed in the acid, or
when the current in the conductor has reached its state of
maximum quantity, and during the time of its remaining
constant, no induction is exerted; and this condition is ex-
pressed by the constant ordinates of the part of the curve
B C, parallel to the axis. Also, the inductive action pro-
duced by each diminution of the battery current, while the
apparatus is in the progress of being drawn from the acid,
will be represented by the differences of the ordinates at
the other end, C D, of the curve.

62. The sum of the several increments of the battery
current, up to its full development, will be expressed by
the ordinate c B, and this will, therefore, also represent
the whole amount of inductive action exerted in one direc-
tion at the beginning of the primary current; and, for the
same reason, the equal ordinate, C d, will represent the
whole induction in the other direction at the ending of the
same current. Also, the whole time of continuance of the
inductive action at the beginning and ending will be re-
presented by A c and d D.

63. If we suppose the battery to be plunged into the
acid to the same depth, but more rapidly than before, then
the time represented by A c will be diminished, while the
whole amount of inductive force expended remains the
same; hence, since the same quantity of force is exerted
in a less time, a greater intensity of action will be pro-
duced (57.), and consequently a current of more intensity,
but of less duration, will be generated in the secondary
conductor. The intensity of the induced currents will,
therefore, evidently be expressed by the ratio of the or-
dinate c B to the abscissa A c. Or, in more general and
definite terms, the intensity of the inductive action at any

moment of time will be represented by the ratio of the rate of increase of the ordinate to that of the abscissa for that moment [*].

64. It is evident from the last paragraph, that the greater or less intensity of the inductive action will be immediately presented to the eye, by the greater or less obliquity of the several parts of the curve to the axis. Thus, if the battery be suddenly plunged into the acid for a short distance, and then gradually immersed through the remainder of the depth, the varying action will be exhibited at once by the form of A B, the first part of the curve, fig. 2. The steepness of the part A y will indicate an intense action for a short time A a, while the part g B denotes a more feeble induction during the time represented by a c. In the same way, by drawing up the battery suddenly at first, and afterwards slowly, we may produce an inductive action such as would be represented by the parts between C and D of the ending of the curve.

65. Having thus obtained representations of the different elements of action, we are now prepared to apply these to the phænomena. And, first, however varied may be the intensity of the induction expressed by the different parts of the two ends of the curve, we may immediately infer that a galvanometer, placed in the circuit of the secondary conductor, will be equally affected at the beginning and ending of the primary current; for, since the deflection of this instrument is due to the whole amount of a current, whatever may be its intensity (57.), and since the ordinates c B and C d are equal, which represent the quantity of induction in the two directions, and, consequently, the amount of the secondary current, therefore the deflection at the beginning and ending of the battery current will, in all cases, be equal. This inference is in strict accordance with the results of experiment; for however rapidly or slowly we may plunge the battery into the acid, and however irregular may be the rate at which it is drawn out, still if the whole effect be produced within the time of one swing of the needle, the galvanometer is deflected to an equal degree.

* According to the differential notation, the intensity will be expressed by $\frac{dx}{dx}$. In some cases the effect may be proportional to the intensity multiplied by the quantity, and this will be expressed by $\frac{dy^2}{dx}$, x and y representing, as usual, the variable abscissa and ordinary.

66. Again, the intensity of one part of the inductive action, for example that represented by A *g*. may be supposed so great as to produce a secondary current capable of penetrating the body, and of thus producing a shock,[*] while the other parts of the action, represented by *g* B and C D, are so feeble as to affect the galvanometer only. We would then have a result the same as one of those given in the last section (42.), and which was supposed to be produced by two kinds of induction; for if the shock were referred to as the test of the existence of an induced current, one would be found at the beginning only of the battery current, while, if the galvonometer were consulted, we would perceive the effects of a current as powerful at the ending as at the beginning.

67. The results mentioned in the last paragraph cannot be obtained by plunging a battery into the acid; the formation of the current in this way is not sufficiently rapid to produce a shock. The example was given to illustrate the manner in which the same effect is supposed to be produced, in the case of the more sudden formation of a current, by plunging one end of the conductor into a cup of mercury permanently attached to a battery already in the acid, and in full operation. The current, in this case, rapid as may be its development, cannot be supposed to assume *per saltum* its maximum state of quantity ; on the contrary, from the general law of continuity, we would infer that it passes through all the intermediate states of quantity, from that of no current, if the expression may be allowed, to one of full development ; there are, however, considerations of an experimental nature which would lead us to the same conclusion (18,90.), and also to the further inference that the *decline* of the current is not instantaneous. According to this view, therefore, the inductive actions at the beginning and ending of a primary current, of which the formation and interruption is effected by means of the contact with a cup of mercury, may also be represented by the several parts of the curve, fig. 2.

68. We have now to consider how the rate of increase or diminution of the current, in the case in question, can be altered by a change in the different parts of the apparatus ; and, first, let us take the example of a single battery and a short conductor, making only one or two

* The shock depends more on the intensity than on the quantity. See paragraph 13.

turns around the helix; with this arrangement a feeble
shock, as we have seen (11.), will be felt at the making,
and also at the breaking of the circuit. In this case it
would seem that almost the only impediment to the most
rapid development of the current would be the resistance
to conduction of the metal; and this we might suppose
would be more rapidly overcome by increasing the tension
of the electricity; and, accordingly, we find, that if the
number of the elements of the battery be increased, the
shock at making the circuit will also be increased, while at
breaking the circuit will remain nearly the same. To
explain, however, this effect more minutely, we must call to
mind the fact before referred to (17), that when the poles
of a compound battery are not connected, the apparatus
acquires an accumulation of electricity, which is discharged
at the first moment of contact, and which in this case would
more rapidly develope the full current, and hence produce
the more intense action on the helix at making the circuit.

 69. The shock, and also the deflection of the needle, at
breaking the circuit with a compound battery and a short
coil (9), appears nearly the same as with a battery of a
single element, because the accumulation just mentioned,
in the compound battery, is discharged almost instantly,
and, according to the theory (71.) of the galvanic current,
leaves the constant current in the conductor nearly in the
same state of quantity as that which would be produced
by a battery of a single element; and hence the conditions
of the ending of the current are the same in both cases.
Indeed, in reference to the ending induction, it may be
assumed as a fact which is in accordance with all the
experiments (9, 13, 73, 74, 75, 76, &c.), as well as with
theoretical considerations*, *that when the circuit is broken
by a cup of mercury, the rate of the diminution of the
current, within certain limits, remains the same, however
the intensity of the electricity or the length of the con-
ductor may be varied.*

Fig. 3.
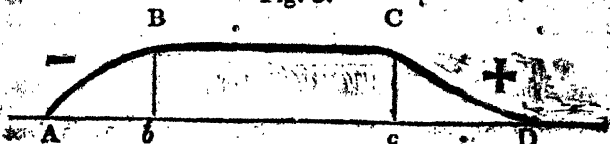
* See the theory of Ohm.—Taylor's Scientific Memoirs, vol. i. p. 312,
311, and vol. ii. p. 01.

70. The several conditions of the foregoing examples are exhibited by the parts of the curves, figs. 3 and 4. The gradual development of the current in the short conductor, with a single battery, and the gradual decline of the same, are represented by the gentle rise of A B and fall of C D, fig. 3 ; while, in the next fig. (4.), the sudden rise of A B indicates the intensity which produces the increased shock, after the number of elements of the battery has been increased. The accumulation of the electricity, which almost instantly subsides, is represented by the part B c e, fig. 4 ; and from this we see, at once, that although the shock is increased by using the compound battery, yet the needle of the galvanometer will be deflected only to the same number of degrees, since the parts B c and c e give inductive actions in contrary directions, and both within the time of a single swing of the needle, and, consequently, will neutralize each other. The resulting deflecting force will, therefore, be represented by e f, which is equal to C k, or to b B, in fig. 3.

Fig. 19.
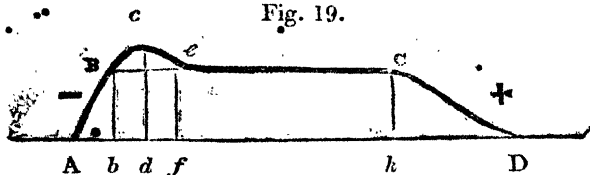
The intensity of the shock at the breaking is represented as being the same in the two figures, by the similarity of the rate of descent of the part C D of the curve in each.

71. We have said (69.) that the quantity of current electricity in a short conductor and a compound battery, after the first discharge, is nearly the same as with a single battery. The exact quantity, according to the theory of Ohm, in a unit of length of the conductor, is given by the formula.

$$\frac{n\,A}{r\,n + R.}$$

In this n represents the number of elements ; A, the electromotive force of one element ; r, the resistance to conduction of one element ; and R, the length of the conductor, or rather its resistance to conduction in terms of r. Now, when R is very small, in reference to r n, as

is the case with a very short metallic conductor, it may be
neglected, and then the expression becomes

$$\frac{n A}{r n} \text{ or } \frac{A}{r};$$

and since this expresses the quantity of current electricity
in a unit of the length of the circuit, with either a single
or a compound battery, therefore, with a short conductor,
the quantity of current electricity in the two cases is nearly
the same.

72. Let us next return to the experiment with a battery
of a single element (68.), and instead of increasing the
intensity of the apparatus, as in the last example, let the
length of the conductor be increased; then the intensity of
the shock at the beginning of the current, as we have
seen(14.), will be diminished, while that of the one at the
ending will be increased.   That the shock should be
lessened at the beginning, by increasing the length of the
conductor, is not surprising, since, as we might suppose,
the increased resistance to conduction would diminish the
rapidity of the development of the current.   But the
secondary current, which is produced in the conductor of
the primary current itself, as we have seen (19.), is the
principal cause which lessens the intensity of the shock,
and the effect of this, as will be shewn hereafter, may also
be inferred from the principles we have adopted.

73. The explanation of the increased shock at the
moment of breaking the circuit with the long conductor,
rests on the assumption before-mentioned (69.), that the
velocity of the diminution of a current is nearly the same
in the case of a long conductor as in that of a short one.
But, to understand the application of this principle more
minutely, we must refer to the change which takes place
in the quantity of the current in the conductor by varying
its length; and this will be given by another application
of the formula before stated (71.).   This, in the case of a
single battery, in which n equals unity, becomes

$$\frac{A}{r + R};$$

and since this, as will be recollected, represents the
quantity of current electricity in a unit of length; of the
conductor, we readily infer from it that, by increasing the
length of the conductor, or the value of R, the quantity

of current in a unit of the length is lessened. And if the resistance of a unit of the length of the conductor were very great in comparison with that of r (the resistance of one element of the battery), then the formula would become

$$\frac{A}{R},$$

or the quantity in a single unit of the conductor would be inversely as its entire length, and hence the amount of current electricity in the whole conductor would be a constant quantity, whatever might be its length. This, however, can never be the case in any of our experiments, since in no instance is the resistance of R very great in reference to r, and therefore, according to the formula (73.), the whole quantity of current electricity in a long conductor is always somewhat greater than in a short one.

74. Let us, however, in order to simplify the conditions of the induction at the ending of a current, suppose that the quantity in a unit of the conductor is inversely as its whole length, or in other words, that the quantity of current electricity is the same in a long conductor as in a short one; and let us also suppose, for an example, that the length of the spiral conductor, fig. 1, was increased from one spire to twenty spires; then, if the velocity of the diminution of the section of the current is the same (69.) in the long conductor as in the short one; the shock which would be received by submitting the helix to the action of one spire of the long coil would be nearly of the same intensity as that from one spire of the short conductor; the quantity of induction, however, as shown by the galvanometer, should be nearly twenty times less; and these inferences I have found in accordance with the results of experiments (75.). If, however, instead of placing the helix on one spire of the long conductor, it be submitted at once to the influence of all the twenty spires, then the intensity of the shock should be twenty times greater, since twenty times the quantity of current electricity collapses, if we may be allowed the expression, in the same time, and exerts at once all its influence on the helix. If, in addition to this, we add the consideration, that the whole quantity of current electricity in a long conductor is greater than that in a short one (73.), we shall have a further reason for the increase of the terminal shock, when we increase the length of the battery conductor.

75. The inference given in the last paragraph, relative to the change in the quantity of the induction, but not in the intensity of the shock from a single spire, by increasing the whole length of the conductor, is shown to be true by repeating the experiment described in paragraph 13. In this, as we have seen, the intensity of the shock remained the same, although the length of the circuit was increased by the addition of coil No. 2. When, however, the galvanometer was employed in the same arrangement, the whole quantity of induction, as indicated by the deflection of the needle, was diminished almost in proportion to the increased length of the circuit. I was led to make this addition to the experiment (13.) by my present views.
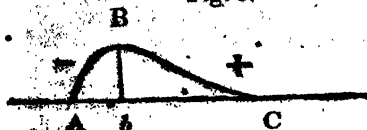
76. The explanation given in paragraph 74 also includes that of the peculiar action of a long conductor, either coiled or extended, in giving shocks and sparks from a battery of a single element, discovered by myself in 1831 (see Contrib, No. II.). The induction, in this case, takes place in the conductor of the primary current itself, and the secondary current which is produced is generated by the joint action of each unit of the length of the primary current. Let us suppose, for illustration, that the conductor was at first one foot long, and afterwards incseased to twenty feet. In the first case, because the short conductor would transmit a greater quantity of electricity, the secondary current produced by it would be one of considerable quantity or power to deflect a galvonometer; but it would be of feeble intensity, for although the primary current would collapse with its usual velocity (69.), yet, acting on only a foot of conducting matter, the effect (74) would be feeble. In the second case, each foot of the twenty feet of the primary current would severally produce an inductive action of the same intensity as that of the short conductor, the velocity of collapsion being the same; and as they are all at once exerted on the same conductor, a secondary current would result, of twenty times the intensity of the current in the former case.

77. To render this explanation more explicit, it may be proper to mention, that a current produced by an induction on one part of a long conductor of uniform diameter, must exist, of the same intensity, in every other part of the conductor; hence the action of the several units of length of the primary current must enforce each other, and produce the same effect on its own conductor that the same current would if it were in a coil, and acting on a helix.

I need scarcely add, that in this case, as in the given para-
graph 74, the whole amount of induction is greater with
the long conductor than with the short one, because the
quantity of current electricity is greater in the former than
in the latter.

78. We may next consider the character of the secondary
current, in reference to its action in producing a tertiary
current in a third conductor. The secondary current
consists, as we may suppose, in the disturbance, for
an instant, of the natural electricity of the metal, which,
subsiding, leaves the conductor again in its natural state;
and whether it is produced by the beginning or ending of
a primary current, its nature, as we have seen (22), is the
same. Although the time of continuance of the secondary
current is very short, still we must suppose it to have
some duration, and that it increases, by degrees, to a
state of maximum development, and then diminishes to the
normal condition of the metal of the conductor; the velo-
city of its development, like that of the primary current,
will depend on the intensity of the action by which it is
generated, and also, perhaps, in some degree, on the
resistance of the conductor; while, agreeably to hypo-
thesis we have assumed (69), the velocity of its diminution
is nearly a constant quantity, and is not affected by changes
in these conditions; hence, if we suppose the induction
which produces the secondary current to be sufficiently
intense, the velocity of its development will exceed that
of its diminution, as in the example of the primary current
from the intense source of the compound battery of many
elements. Now this is the case with the inductions which
produce currents of the different orders, capable of giving
shocks or of magnatizing steel needles: the secondary
currents from these are always of considerable intensity,
and hence their rate of development must be greater than
that of their diminution; and, consequently, they may be
represented by a curve of the form exhibited in fig. 5, in
which there is no constant part, and in which the steepness
of A B is greater than
that of B C. There
are, however, other con-
siderations, which will be
noticed hereafter (89.),
which may affect the
from of the part. B C.

Fig. 5.

of the curve fig. 20, rendering it still more gradual in its

descent, or, in other words, which tend to diminish the
intensity of the ending induction of the secondary current.

79. It will be seen at once, by an inspection of the
curve, that the effect produced, in a third conductor, and
which we have called a tertiary current, is not of the
same nature as that of a secondary current. Instead of
being a single development in one direction, it consists of
two instantaneous currents, one produced by the induction
of A B, and the other, by that of B C, in opposite direc-
tions, of equal quantities, but of different intensities.
The whole quantity of induction in the two directions
will each be represented by the ordinate B b, and hence
they will nearly neutralize each other, in reference to their
action on the galvanometer, in the circuit of the third
conductor. I say they will *nearly* neutralize each other,
because, although they are equal in quantity, they do not
both act in absolutely the same moment of time. The
needle will, therefore, be slightly affected: it will be im-
pelled in one direction, say to the right, by the induction
of A B, but before it can get fairly under way, it will be
arrested, and turned in the other direction, by the action
of B C. This inference is in strict accordance with
observation: the needle, as we have seen (24.), starts
from a state of rest, with a velocity which, apparently,
would send it through a large arc; but before it has
reached, perhaps, more than half a degree, it suddenly
stops, and turns in the other direction. As the needle is
first affected by the action of A B, it indicates a current
in the adverse direction to the secondary current.

80. Although the two inductions in the tertiary con-
ductor nearly neutralize each other, in reference in the in-
dications o the galvanometer, yet this is far from being the
case with regard to the shocks, and the magnetization of
steel nedles. These effects may be considered as the
results alone of the action of A B; the induction of B C
being too feeble in intensity to produce a tertiary current
of sufficient power to penetrate the body, or overcome the
coercive power of the hardened steel. Hence, in reference
to the shock and magnetization of the steel needle, we
may entirely neglect the action of B C, and consider the
tertiary excitement as a single current, produced by the
action of A B; and, because this is the beginning induction
(56.), the tertiary current must be in an opposite direction
to the secondary. For a similar reason, a current of the
third order should produce in effect a single current of the

fourth order, in a direction opposite to that of the current which produced it, and so on : we have here, therefore, a simple explanation of the extraordinary phænomenon of the alternation of the directions of the currents, of the different orders, as, given in this and the preceding paper. See paragraph 25.

81. The operation of the interposed plate (32, 47, 48, &c.), in neutralizing the shock, and not affecting the galvanometer, can also be readily referred to the same principles. It is certain that an induced current is produced in the plate (III. 65.), and that this must react on the secondary in the helix; but it should not alter the total amount of this current, since, for example, at the ending induction, the same quantity of current is added to the helix, while the current in the plate is decreasing, as is subtracted while the same current is increasing. To make this more clear, let the inductive actions of the interposed current be represented by the parts of the curve, fig. 5. The induction represented by A B will re-act on the current in the helix, and diminish its quantity, by an amount represented by the ordinate *b* B ; but the induction represented by B C will act, in the next moment, on the same current, and increase its quantiay by an equal amount, as represented by the same ordinate B *b*; and since both actions take place within a small part of the time of a single swing of the needle, the whole deflection will not be altered, and consequently, as far as the galvanometer is concerned, the interposition of the plate will have no perceptible effect.

82. But the action of the plate on the shock, and on the magnetization of tempered steel, should be very different; for although the quantity of induction in the helix may not be changed, yet its intensity may be so reduced, by the adverse action of the interposed current, as to fall below that degree which enables it to penetrate the body, or overcome the coercive force of the steel. To understand how this may be, let us again refer, for example, to the induction which takes place at the ending of a battery current : this will produce, in both the helix and the plate, a momentary current, in the direction of the primary current, which we have called *plus;* the current in the plate will react on the helix, and tend to produce it in two inductions, which, as before, may be represented by A B and B C of the curve, fig. 5 ; the first of these, A B, will be an intense action (78.), in the *minus* direction, and will there-

fore tend to neutralize the intense action of the primary current on the helix; the second (B C) will add to the helix an equal quantity of induced current, but of a much more feeble intensity, and hence the resulting current in the helix will not be able to penetrate the body; no shock will be perceived, or at least a very slight one, and the phænomena of screening will be exhibited.

*83. When the plate of metal is placed between the conductors of the second and third orders, or between those of the third and fourth, the action is somewhat different, although the general principle is the same. Let us suppose the plate interposed between the second and third conductors; then the helix, or third conductor, will be acted on by four inductions, two from the secondary current and two from the current in the plate. The direction and character of these will be as follows, on the supposition that the direction of the secondary current is itself *plus:*

The ending interposed . . . feeble and . . . *minus.*
The beginning secondary . . intense and . . *minus.*
The ending secondary . . . feeble and . . . *plus.*
The beginning interposed . . intense and . . *plus.*

Now if the action, on the third conductor, of the first and third of the above inductions be equal in intensity and quantity, they will neutralize each other; and the same will also take place with the action of the second and fourth if they be equal, and hence, in this case, neither shock nor motion of the needle of the galvanometer would be produced. If these inductions are not precisely equal, then only a partial neutralization will take place, and the shock will only be diminished in power; and, also, perhaps, the needle will be very slightly affected.

84. If, in the foregoing exposition, we throw out of consideration the actions of the feeble currents which cannot pass the body, and, consequently, are not concerned in producing the shock, then the same explanation will still apply which was given in the last paper (III. 94.); namely, in the above example, the helix is acted on by the minus influence of the secondary, and the plus influence of the interposed current.

85. We are now prepared to consider the effect on the helix (fig. 1.) of the induced currents produced in the conductor of the primary current itself. These are true secondary currents, and are almost precisely the same in

their action as those in the interposed plate. Let us first examine the induced current at the beginning of the primary, in the case of a long coil and battery of a single element; its action on the helix may be represented by the parts of the curve, fig. 5. The first part, A B, will produce an intense induction opposite to that of the primary current; and hence the action of the two will tend to neutralize each other, and no shock, or a very feeble one, will be produced. The ending action of the same induced current, which is represented by B D, restores to the helix the same quantity of current electricity (but in a feeble state) which was neutralized by A B, and hence the needle of the galvanometer will be as much affected as if this current did not exist. These inferences perfectly agree with the experiment given in paragraph 19. In this, when the ends of the interposed coil were joined so as to neutralize the induced current in the long conductor, the shock at the beginning of the primary current was nearly as powerful as with a short conductor, while the amount of deflection of the galvanometer was unaffected by joining the ends of the same coil.

86. At first sight it might appear that any change in the apparatus which might tend to increase the induction of the primary current (16) would also tend to increase, in the same degree, the adverse secondary in the same conductor; and that hence the neutralization mentioned in the last paragraph would take place in all cases; but we must recollect, that if a more full current be suddenly formed in a conductor of a given thickness, the adverse current will not have, as it were, as much space for its development, and, therefore, will have less power in neutralizing the induction of the primary than before. But there is another, and, perhaps a better reason, in the consideration, that in the case of the increase of the number of elements of the battery, although the rapidity of the development of the primary current is greater, yet the increased resistance which the secondary meets with, in its motion against the action of the several elements, will tend to diminish its effect. Also, by diminishing the length of the primary current, we must diminish (76.) the intensity of the secondary, so that it will meet with more resistance in passing the acid of the single battery, and thus its effects be diminished.

87. The action of the secondary current, in the long coil, at the *ending* of the primary current, should also, at

first sight, produce the same screening influence as the
current in the interposed plate ; but, on reflection, it will
be perceived that its action in this respect must be much
more feeble than that of the similar current at the begin-
ning ; the latter is produced at the moment of making
contact, and hence it is propagated in a continuous circuit
of conducting matter, while the other takes place at the
rupture of the circuit, and must therefore be rendered
comparatively feeble by being obliged to pass through a
small portion of heated air ; very little effect is therefore
produced on the helix by this induction (19). · The fact
that this current is capable of giving intense shocks, when
the ends of a long wire, which is transmitting a primary
current, are grasped at the time of breaking the circuit, is
readily explained, since, in this case, the body forms, with
the conductor, a closed circuit, which permits the com-
paratively free circulation of the induced current.

88. It will be seen that I have given a peculiar form
to the beginning and ending of the curves, figs. 2, 3, &c.
These are intended to represent the variations which may
be supposed to take place in the rate of increase and
decrease of the quantity of the current, even in the case
where the contact is made and broken with mercury. We
may suppose, from the existence of analogous phænomena
in magnetism, heat, &c., that the development of the
current would be more rapid at first than when it approx-
imates what may be called the state of current saturation,
or when the current has reached more nearly the limit of
capacity of conduction of the metal. · Also, the decline of
the current may be supposed to be more rapid at the first
moment than after it has lost somewhat of its intensity, or
sunk more nearly to its normal state. These variations
are indicated by the rapid rise of the curve, fig. 2, from
A to *y*, and the more gradual increase of the ordinates
from *h* to B ; and by the rapid diminution of the ordinates
between C and *l*, and the gradual decrease of those towards
the end of the curve. · · ·

89. These more · minute considerations, relative to the
form of he curve, will enable us to conceive how the time
of the ending of the secondary current, as we have sug-
gested (78.) may be prolonged beyond that of the natural
subsidence of the disturbance of the electricity of the
conducter on which this current depends. · If the develop-
ment of the primary current is produced by equal incre-
ments in equal times, as would be the case in plunging

the battery (59.) into the acid with a uniform velocity, then the part A B of the curve fig. 2 would be a straight line, and the resulting secondary current, after the first instant, would be one of constant quantity during nearly the whole time represented by A c ; but if the rate of the development of the primary current be supposed to vary in accordance with the views we have given in the last paragraph, then the quantity of the secondary current will begin to decline before the termination of the induction, or as soon as the increments of the primary begin to diminish ; and hence the whole time of the subsidence of the secondary will be prolonged, or the length of b C. fig. 5 will be increased, the decent of B C be more gradual, and the intensity of the ending induction of the secondary current be diminished (see last part of paragraph 78.)

90. Besides the considerations we have mentioned (88.), there are others of a more obvious character, which would also appear to affect the form of particular parts of the curve. And first we might perhaps make a slight correction in the drawing of fig. 2, 3, &c., at the point A, in consideration of the fact that the very first contact of the end of the conductor with the surface of the mercury is formed by a point of the metal, and hence the increment of development should be a little less rapid at the first moment than after the contact has become larger ; or in other words, the curve should perhaps start a little less abruptly from the axis at the point A. Also Dr. Page has stated* that he finds the shock increased by spreading a stratum of oil over the surface of the mercury ; in this case it is probable that the termination of the current is more sudden, on account of the prevention of the combustion of the metal by means of the oil, and the fact that the end of the conductor is drawn up into a non-conducting medium.

91. The time of the subsidence of the current, when the circuit is broken by means of a surface of mercury, is very small, and probably does not exceed the ten thousanth part of a second ; but even this is an appreciable duration, since I find that the spark at the ending presents the appearance of a band of light of considerable length, when viewed in a mirror revolving at the rate of six hundred times in a second ; and I think the variations in the time of ending of the current under different conditions may be detected by means of this instrument.

92. Before concluding this communication, I should state that I have made a number of attempts to verify the the suggestion given in my last paper (III 127.), that an inverse induction is produced by a galvanic current by a change in the distance of the conductors, but without success. These attempts were made before I had adopted the views given in this section, and since then I have found (80.) a more simple explanation of the alteration of the currents.

93. In this Number of my Contributions, the phaenomena exhibited by the galvanic apparatus have alone been discussed. I have, however, made a series of experiments on the induction from ordinary electricity, and the re-action of soft iron on currents, and I think that the results of these can also be referred to the simple principles adopted in this paper; but the require further examination before submitting to the public.

A Comparative Examination of the Sugar Cane grown at the Antilles and in France, followed by Considerations on the Manufacture of Sugar.— Second Memoir. By M. Osmin Hervy, Professor of Chemistry in the School of Pharmacy, Paris. *

This work had been remitted to us by M. Hervy some days previous to the frightful event which removed him from the ranks of science. We hasten to publish it, happy in having it still in our power to render this late homage to the memory of this excellent and unfortunate young man.

The richness of the sugar cane (arundo saccarifera) being at the present day well known, a colonial manufacture better understood ought to cause by an augmentation of its products in sugar, a great commercial movement. It is that we may hasten as much as lies in our power the realization of this laudable hope conceived by the planters, that we now add a few words to a work on the sugar cane published by us in the month of September last. The sugar canes of which we have made use in these new experiments are no longer those grown on the

wet and burning soil of the Antilles but those of France, the one under the protection of the greenhouse, the other in the open land.

Salts, and other organic matters foreign to sugar, exist in the French canes, in far more considerable proportions than in the canes of the colonies ; but it is then a fact of little importance, and only worthy of being disposed of in the same manner as their richness in sugar; it matters little in effect, to the creole planter, whether cane grown in a greenhouse be more or less rich in sugar than his own. That which to him is of the greatest importance is to know first,—That the molasses does not pre-exist in the cane during its vegetation ; that what we have found as a constituant part of the dry canes which we had received from the colonies, prove that they had undergone some alteration which must be due either to dessication, or to some effect produced by the voyage. Secondly, that molasses may be formed by the reaction of certain bodies on the crystallizable sugar.

The sugar canes which have been the objects of our researches had been cultivated either in the greenhouses of the royal domain of Neuilly,* or in the open air in the botanic garden of Bordeaux. M. Gachet, professor of botany, cheerfully offered to send them to us on our last visit to that place.

The sugar canes grown in open land were of small diameter, but of a yellow gold colour, stripped of their leaves to the summit, an evident proof of a perfect maturity : their savour was sweet, aromatic, and very agreeable.

The canes grown in the greenhouse, have a much greater diameter. Their knots were at very diversified distances, their colour green or opal yellow, some were stripped of their leaves, others still reserved them, and let us here remark that the age of these canes varied from a few months to three years. Each year of vegetation was marked by a protuberance, marking the point where vegetation had been renewed the year following. Their savour which is sweet, aromatic, and very agreeable, has been recognized by the creoles, altogether sugary and little different in flavour from canes grown in the Artilles. The juice of our canes was remarkable for its different

* We are under considerable obligations to M. Jaques, gardener in chief to the Royal Society of Agriculture on the Seine.

degrees of density, from 103 to 106 (5 to 12 Baumi), according as it was obtained from the foot of the cane or the upper part of it.

Sugar exists in its crystallizable state in the higher knots of the cane, that is to say, at the earliest period of vegetation, precisely as it does in the lower knots which are of several years growth. It appears very evident, in our opinion, that the crystallizable sugar is not a secondary product, but, in reality, a primitive product, from the secret elaboration, to the state of crystallizable sugar, to the moment of the formation of the vessels in which it is enclosed. It appears even certain—we shall attempt likewise to prove it—that it exists in the state of crystallizable sugar from the moment of the blossoming and fructification of the cane.*

The ideas which we have now detailed, on the absence of liquid sugar in the sugar cane, have for their basis the following experiments:

Pieces of sugar cane, cut into very thin slices, were dried, some in vacuo, and others in a dry air, at a temperature of 61° centigrades—in fifteen minutes a complete dessication took place without altering the sugar. These dried slices were then pulverized; the cane now remains, very little hygrometric. From the powder of cane, de-

* These facts seem to us to establish a very decided limit between the formation in vegetables of the sugar of cane (crystallizable) and the formation of the sugar of raisins; in fact, we never find sugar in fruits at their birth, and we see it produced during the act of maturation; there is then a very decided difference between the natural formation of the sugar of canes, and that of sugar of raisins; those parts of plants which contain the first have, from their infancy, as the cane and the beet, a sweet flavour evidently saccharine. The parts of plants which contain the second have in their birth or infancy, a harsh bitter flavour, with no traces of sugar.

The microscopic examination of the sugar cane, has shewn us that this beautiful cellular arrangement, undergoes no change in its organization during any period of its vegetation; the nascent cane like that more advanced in age, presents the same tubes, the same groups of ligneus fibre; and we may truly say, that the anatomical organization of the cane is as beautiful and as simple as it is possibly to conceive of. M. Fremy has observed on the contrary, that the disposition of the cellules change from day to day in the sacchariferous fruits, in proportion as they approach the period of maturation, that the partitions, at first thick and opaque, terminate by becoming much swelled, transparent, and at last burst, to set at liberty the acid liquid which they contain.

We believe then that we may safely say, *that the sugar of the cane is always a primitive production;* whilst the sugar of raisins is always a secondary product. These facts are amply sufficient to explain how it is that the sugar of raisins is artificially formed with such great facility, whilst we have not been able to the present day to make the sugar of cane of whole pieces.

rived from the lower extremity of the plant, after having been treated with cold alchol, at 95°, which dissolved it, was produced $\frac{3}{100}$ of a deliquescent matter, either isolated from the matter $\frac{5}{1000}$, the residue was a deliquiescent substance, soluble in water, neither sucreous nor saline, and which left no ashes after incineration. To this treatment by cold alcohol, repeated three times, succeeded several experiments with boiling alcohol. After its reaction on the powder of the cane, the alcohol, by a slow cooling, left deposited some crystals of sugar, perfectly white and transparent; the liquid on its evaporation afterwards gave no trace of crystallizable sugar.

The same experiments were repeated with powdered cane taken from the summit of the cane; treated with cold alcohol, it gave out no liquid sugar, but a dissolution of a quantity of deliquescent matter sensibly the same as that obtained by the treatment of lower part; with boiling

This experiment which we regard as decisive, because it enabled us to exhaust the cane of its sugar, and to obtain it afterwards in small white crystals, has been applied to more than ten different specimens, taken from all parts of the sugar cane. If these experiments which we have just described, on the dried sugar cane, have not given us in any instance the least trace of liquid sugar (molasses) it has not been the same when we have treated the fresh sugar cane or its juice with alcohol.

In 100 grammes of the juice of the cane of a density of 108 (12° Baumi) produced by expression, we have poured 50 grammes of alcohol at 95°, which gave out a floculent precipitate, which was separated by the aid of a very fine filter. The alcoholic sucreus liquid was placed under a large pan containing quick lime, at the end of 15 days all the water and a great part of the alcohol had disappeared, the sugar had disposed itself on the sides of the capsule, in whitish nipple like forms hard and brittle, it weighed 16gr. 8. The remaining alcohol decanted and evaporated, left 2gr. 2 of a sucreous, saline, deliquiescent matter; ether isolated from it a very small quantity of wax; the rest, very soluble in water, obstinately refused to crystallize, or to give out to any menstrum, the sugar which it retained. alcohol no other matter was obtained but crystallizable sugar. By the direct treatment of fresh canes with alcohol, the evaporation in vacuo, of the burnt juice of the cane have given us the same result: crystallization has never been

complete, there always remained a residual sugary saline *
matter, the weight of which corresponded with the quantity
of soluble salts contained in the canes which we analyzed.

We believe then that we may safely assert, first—That
there is no uncrystalizable sugar pre-existing in the sugar
cane. Secondly—That the salts react on the crystallizable
sugar, in such a manner as produce molasses, that their
action does not take place during vegetation; but depends
much upon the manipulations; it cannot be otherwise,
since in the vital act of vegetation, the different materials
are elaborated in a complete independence of each other.

But what are we to think of the non-sacreous deliquie-
scent matter which was obtained from the dried cane by
alchohol at 95°?

We are induced to believe that in the great work of
vegetation, it has power to perform an important part in
the production of the moloss, that is to say, that if it
does not combine with the sugar, it naturally tends to
augment the viscosity of the syrups, and to aglutinize the
crystals in the latter crystallizations in the parent stem.

This matter is colourless when it has been obtained by
evaporation in vacuo, or at a low temperature; it is
coloured by heat: its aqueous solution presents these two
distinct characters, of being precipitated by tanner's bark,
and of being absorbed by pure animal carbon in such a
manner that it cannot be lifted up again by cold water.
We have seen farther, that the burnt juice of the sugar
cane certainly proves the alcoholic fermentation; that it
passes, with much greater facility, the viscuous fermenta-
tion, whilst that the alcoholic fermentation is always deve-
loped in the juice of the cane passed through animal black,
which never follows the viscous fermentation. We have
found this matter small in quantity in canes in our pos-
session, but it cannot exist in a greater proportion. The
canes called *folles*, are improper to furnish crystallizible
sugar; such as the canes grown on new land, which take
a much greater development. For the remainder we have
no need to have recourse to hypothesis, in order to com-
prehend why it is that the juices of the cane are nearly
incrystallizable.

These canes have grown up on virgin lands, on those
lands of which the trees which covered them have been

* We have already indicated that the ci not which We u ed in our experi-
ments contained a much more considerable quantity of salts than the
colonial canes of which we have made the analysis.

destroyed by the action of fire; a part of the soluble
salts, in such great abundance in their fertile soils, have
been carried along into the cane, exert in a more matured
state of the cane, with a fatal influence, their chemical
action on the crystallizable sugar.*

The sugar juices always contain quantities more or less
great of salts, and we know that sea salt combined with
the sugar of the cane, gives birth to a delequiescent com-
position, which contains six parts of sugar for one part of
sea salt, and which remains in the parent juice in the
state of uncrystallizable sugar. But the chloride of
sodium is not the only salt which can combine with sugar,
and exercise a deleterious influence on the process of crys-
tallization; we have observed that a great number of salts
are often opposed to the crytallization of a baking syrup.
The haloid salts may be placed in the first rank, the car-
bonates of potash and of soda react even on sugar at the
baking temperature, and render the syrups high coloured
and almost uncrystallizable.

The parent juices of the sugar cane, and the sugar
beet, can yield molasses then, without these plants neces-
sarily containing them during vegetation.

Let us remark, however, that a liquid never crystallizes
without leaving a residue representing a solution at its
point of saturation, and we will explain how M. Avequin
obtained so much molasses, that is to say a crystallizable
sugary juice, whilst M. Plagne, by crystallizing the juice
four and five times, obtained an almost complete crystal-
lization, a circumstance which is equally inherent to the
mode of manipulation in the colonies.

A practical fact now comes to the support of what we
have advanced: they then employ in the preparation of
double-refined sugar the very finest sugar land; the syrup
which is taken to be baked in vacuo is perfectly colourless
and may be considered as a solution of sugar almost pure.
During the first crystallization there is not more than 50
per cent of crystallizable sugar, and it cannot be with-
drawn farther without candying *(earameliser)* a portion
of sugar; this depends, according to the observation of
M. Dubrunfaut, on the fact that a part of the water on
boiling can not dissolve more than five parts of sugar, that

* In the same manner as the sugar cane—The sugar beet grown in lands
over-manured, and rich in deliquiescent salts, furnish syrups which crys-
tallize with much greater difficulty, and furnish more molasses.

it abandons three only by cooling, whilst one part of cold water dissolves two of sugar, it is this dissolution which constitutes the parent syrup.

.The cooling of a saturated solution of sugar from heat leaves then a parent syrup *(can-mere)* which is sometimes designated by the name of molasses, and which is found to be formed in reality of two parts of sugar and one of water, but still we are not ignorant that these molasses are far from being liquid or uncrystallizable sugar ; we have even signalized this fact in order to explain why the Creoles who do not repeat the boiling, at least very rarely, of their running syrups, carry so high the figure of their molasses.   It is true that those amongst them who do reboil them, remove from them in general the bad products only.   The inferiority of these sugars of syrups holds so universally to this that in place of reboiling them each day, as in the sugar bakehouses, or refining house of sugar in France, the planters wait so long before reboiling them, that by this means the crystallizable sugar, which these syrups contain in such great abundance, are found partially altered by a slow fermentation, and by the reaction of saline matters.   A better understood treatment of this product, so rich in crystallizable sugar, would give, without contradiction, great benefits to the Creoles. In fact, on the place of manufacture, the rough sugar is worth from 50 to 60 centimes the kilogramme, whilst the molasses, whatever be their richness in crystallizable sugar, are never sold for more than 10 centimes the litre (7 or 8 centimes the kilogramme).   On a great number of plantations, the molasses are almost entirely for the nourishment of the flocks ; on others they are employed in the *guil diveries* in the extraction of rum ; in France likewise the molasses are utilized for the nourishment of animals, and for the extraction of alcohol, but these are the molasses marked from 44 to 50° of the areometer of Baumé, whilst the molasses of the colonies are ordinarily marked from 37 to 38°.   This difference in the density of syrups may vary from 6 to 50 per cent. the quantity of uncrystallizable matter in a running syrup.   The planters then are able to obtain from their molasses 35 or 40 per cent. of sugar.

Let us prove that the perfected equipages for the process of baking produce the finest sugars, from syrups the richest in crystallizable sugar ; but they will not augment the quantity of baked sugar obtained from the

first casting; let us also repeat, that it will always be necessary to have recourse again to the *rebaking of the syrups*, in order to obtain from them the maximum proportion of crystallizable sugar, as the practice of the manufacturer can teach.

These reflections, on the richness in crystallizable sugar, of a produce so precious and so badly utilised, have appeared to us to be worthy of being depended upon, at this time, when the planters are making all their efforts to progress in the art—whilst they avail themselves daily of the great improvements in their processes of manufacture—and whilst they acknowledge that they have too long been ignorant, or misunderstood the fertility of their soil, and the value of its products. New ideas are spreading each day in our islands; the Creole no longer has faith in the old modes of proceeding; the planters, with new kindled zeal, and animated with the love of good, like Messieurs de Jabrund and de Longchamps, adopt and encourage the most rational systems of manufacture; it is within the course of a few days that Havre saw depart from its shores, for Gondaloupe, a considerable number of baking equipages of the newest construction, amongst others, some constructed for M. de Jabrun, who is now returning to his plantations, after a deligation of six years; the same vessel likewise took out M. Hotessier, the author of an excellent pamphlet on improvements adapted to the manufacture of sugar from the cane. M. Hotessier has added to his pamphlet the description of some new baking apparatus, almost similar to those of M. de Jabrun, which apparatus present the most favourable conditions to a good fabrication.

During a period of some years numerous plantations have also availed themselves of improved apparatus; the impulse has been given, the Creoles will not leave, for a long time again, *thirty per cent. of sugar in their dry canes;* assuredly they will not, very soon, any more make forty per cent. of molasses, or even sixty, as is done in the isle of Bourbon. The metropolis, as well as the colonies, will soon feel the benefit of it, in the means of reconciling of from forty to fifty per cent. of crystal sugar.

The process of dessication of the cane, as at present applied on a grand scale on the island of Gondaloupe, gives rise to the brightest anticipations. Our works on the dried cane have proved, that dessication of them does not alter, in the least degree, the crystallizable sugar

that the exhausting of the cane, the extraction of sugar only becomes more easy and more simple. That day, wherein the system of dessication shall present itself in its great superiority for the perfect exhaustion of the cane—that day, the planter, in place of renouncing the fabrication of sugar—in place of conceding this advantage to the metropolis, will work on his plantations his dried canes, and will employ his improved baking equipage for the evaporation of the syrups enclosed in the dried canes; and this would not hinder our great manufacturers and merchants from importing dried canes, in order to extract from them, in the refineries of France, immense products of sugar.

The general good—the interests of our colonies, have equally a claim to an augmentation in the production of sugar. That product—primary necessity—which its present dearness puts out of the power of the working class to procure. Its consumption, which seems to diminish, in lieu of relatively augmenting by ready money, hinders its use, and always places this salutary nourishment at a price inaccessible to twelve millions of the French nation. That, then, will be a bright day for our colonies, wherein the planter, doubling his produce by his industry, shall augment the cargoes of his vessels sailing for the metropolis —and would thus also give life and vigour to our merchant fleets.

We cannot perhaps give a better idea of this question, and the important part it will perform in our industry, than in citing some words of a memoir by M. Millott on the question of sugars.

" Nature has even ordained that sugar occupies the first rank in the vegetable order of which it is one of those products the most precious for aliment, as well as in a commercial point of view."

" Sugar is so much an article of merchandise *par excellence*, that if the consumption of sugar is increased by a reduction of the price of this object of predeleotion, the cargoes of our merchantmen are increased by the carriage of the number of tons of sugar consumed."

ART. V. – *On the means of detecting Arsenic in the Animal Body, and of counteracting its Effects* ; by J. LAWRENCE SMITH, M. D., of Charleston, S. C.*

Messrs. Editors—This, I hope will receive a place upon the pages of your Journal, if it be only for the importance of the subject of which it treats, although it is not improbable, in stating what I am about to do concerning the more recent experiments upon arsenious acid, that your readers will be able to find something which may be of importance to them in future investigations upon this substance. But two months have elapsed since the whole of France was agitated by one of the most interesting criminal processes upon record—it was a case of poisoning by arsenic ; and the contradiction of the results of the medico-legal examinations, created an excitement which the decision of the jury augmented. Three chemical examinations were made upon different portions of the body, and at different times, to ascertain whether arsenic had been administered to the individual during life. The materials for the first were furnished immediately after death, and consisted of the fluid found in the stomach, the stomach itself, and a portion of the intestines ; but the first was lost by an accident which happened to it while being experimented upon, so that the stomach and intestines alone remained. The second and third were made upon portions of the body exhumed after eight months' burial ; they were the liver, heart, brain, and inner muscles of the thigh. The first and second examinations were made by several expert chemists of Tulle, without detecting the poison. The third M. Orfila was called upon to make, and he succeeded in exhibiting the metal, reduced by means of Marsh's apparatus ; his success was no doubt owing to the manner in which he carbonized the animal matter, which was by the aid of nitric acid.

One cannot be surprised at the excitement that a thing of this character must have produced, and it is with much interest and benefit that I, have followed up the chemical researches consequently arising, as well as the many interesting questions proposed for solution. and my object now is to mention the most important of them. Some of the questions are as follows :

1st. Does the hydrated peroxide of iron contain arsenic ?

2d. Does arsenic exist normally in the animal tissues ?

3d. Is not Marsh's apparatus subject to serious objections ?

4th. What are the best means not only of detecting but of ascertaining the quantity of arsenic when in combination with animal matter ?

5th. What are the best means of combating the poisonous effects of arsenic acid ?

* Silliman's Phil. Journal, for April, 1841.

To all these questions such answers will be given as have hitherto been furnished.

Does arsenic exist in the peroxide of iron ?

This question originated from the fact that this substance had been administered in the case spoken of; and there are those who suppose that the arsenic detected belonged originally to the peroxide of iron used as an antidote.

It is well known that arsenic exists in a state of combination in many of the sulphurets of iron, from which the sulphate is obtained, and it is the latter that furnishes the peroxide either by precipitation or heat. Both forms of this oxide have been subjected to minute examination by M. Orfila, who was particularly interested in this question, and the following are his experiments with their results :

" 1st. I boiled during four hours, in five capsules, four and a half ounces of hydrated peroxide of iron, taken from different apothecaries, with four ounces of distilled water, and by Marsh's apparatus no trace of arsenic could be obtained.

" 2d. I then added thirty grains of pure caustic potash to the hydrated peroxide of iron in each capsule, but no trace of arsenic could be obtained,

" 3d. But on treating by an ebullition of five hours an equal quantity of hydrated peroxide of iron in pure sulphuric acid, the liquid of three capsules out of the five gave arsenical taches.

" 4th. Four portions of four ounces each of colcothar of commerce, (the anhydrous peroxide of iron formed by heating the sulphate,) obtained from different merchants, by ebullition for four hours in distilled water, did not give indications of the presence of arsenic,

" 5th. This substance in the same quantity by ebullition during five hours with strong sulphuric acid, gave large arsenical stains with the aid of Marsh's apparatus.

" 6th. Thirty grains of colcothar boiled with sulphuric acid, gave arsenical stains.

" 7th. Fifteen grains of the same body, treated in the same way, gave no indications of arsenic.

" 8th. A solution of salphate of iron gave no arsenical stains with the apparatus."

M. Orfila next administered four ounces of colcothar to three dogs, tying the œsophagus to prevent vomiting. One of them was examined in thirty-four hours after, the second fifty, and the third sixty. The liver, spleen, heart, and kidneys of these animals, were submitted to investigation, but no trace of arsenic could be obtained. The liquid of the stomach and intestines of the first dog being separated from the colcothar, gave arsenical stains, though its urine did not indicate the presence of this metal. The

intestinal liquid of the second dog gave some stains, less apparent
and less numerous than that of the third, but on the contrary its
urine gave strong indications of arsenic.

The conclusions to be arrived at from these experiments are,
that the hydrated peroxide of iron, and the colcothar, the former
of which is administered as an antidote for arsenious acid, contain
arsenic in minute quantities, (though the former being no doubt
as often without as with it,) but that it requires the aid of a strong
acid to develope it, and also, that when these substances are
admistered, the arsenic that they contain is slowly absorbed, passes
by the organs, and is eliminated by the urine. The organs never
at any time retain sufficient arsenic to exhibit it when examined
for.

This question being answered in the affirmative, would appear
to throw a great obstacle in the way of pronouncing with certainty
whether the arsenic found in the intestinal liquid of an individual
supposed to have been poisoned, and to whom the hydrated
peroxide of iron had been administered as an antidote, was due to
arsenious acid or to the oxide of iron. This difficulty would not
arise except the quantity found be extremely small ; for the
peroxide of iron, from the manner in which it is prepared, can
contain but the smallest appreciable amount ; and, moreover, as it
has already been remarked, it is not always that we find even that.
The plan that the medico-jurist should adopt, in a case of this
character, would be to examine the peroxide of iron that the person
had taken, should there be any of it remaining, if not the sulphate
of iron from which it was made. Again, he should lay but little
stress upon the examination of the intestinal liquid, but direct his
attention particularly to the organs. This, together with circum-
stances peculiar to each case, will explain away any doubt that
might arise.

It ought to be perfectly understood, that the facts of the peroxide
of iron containing a small quantity of arsenic, should be considered
rather as a light to guide the chemist in his researches, than as a
stumbling-block that might cause him to fall into error.

Does arsenic exist normally in the animal tissues ?

This perhaps has been one of the most interesting questions
ever proposed to chemists, and the investigations that it has given
rise to, serve to show the almost perfection of their science, for
were it supposed that the whole animal frame contained but one
fiftieth of a grain of arsenic, the chemist would not despair not only
of being able to detect it, but also of fixing its locality.

As it regards the bones, it has been clearly demonstrated that
they contain arsenic in a minute quantity, but sufficient to place
the fact beyond the smallest doubt.

Whether it exists in the muscles or not, it is a question by not

means settled. It is true, that with the aid of Marsh's apparatus
there can be obtained from muscles digested a long while in nitric
acid, stains which are of different shades, such as brilliant white,
brilliant yellow, and rusty colour; they are volatile and not soluble
in nitric acid. Many have supposed their composition to be sul-
phur with an infinitely small quantity of arsenic. I think that
these stains can be more easily accounted for by sulphur and
phosphorus, both of which exist in the muscles, and I am sorry
that there is neither time nor opportunity to examine into the
truth of this supposition. Nevertheless, whether they contain
arsenic or not, the stains obtained have but one characteristic
belonging to that of arsenic, volatility.

The next part of this question is very important ; it is whether
the organs, such as the liver, spleen, heart, &c., contain normal
arsenic. The reason of its importance is, that it is upon them
that we should place considerable reliance, in the examination of
the body of a person supposed to have been poisoned by arsenic.
To this we answer, that not the smallest trace has been detected
in any of them ; and the answer is based, not upon the few experi-
ments of a single individual, but d awn from numerous careful
researches, made by skillful chemists. What is still more con-
vincing on this point, is, that even in some few cases, where an
animal has been poisoned by arsenic, its liver will not indicate its
presence.

To sum up the answer to this question in a few words—th e
bones do contain arsenic. No positive evidence has yet been
given to lead us to believe that the muscles contain the smallest
quantity of arsenic. We have the most positive evidence that the
organs do not contain the least trace of arsenic.

Is not Marsh's apparatus subject to many and serious objections?

This valuable instrument I think was discussed a year or two
since, by Dr. Mitchell, of Philadelphia, but as I have never seen
his article on the subject, I hope, that if this should meet his eye,
he will excuse such parts of it as may be a repetition of what he
then stated. Most that is about to be mentioned concerning this
apparatus, belongs to the investigation of those more intimately
connected with the subject than myself.

Marsh's apparatus, modified from its original and rather
complex form,* consists of a four or eight ounce phial, with a per-
forated cork and glass tube, bent at right angles, or straight, (the
former is considered preferable, though in both instances the ex-
tremity must be drawn out to a capillary opening,) and furnished
with a porcelain plate or saucer, and the materials for generating
hydrogen—zinc, sulphuric acid and water. These three last sub-
stances in effect constitute the instrument. The first question to
be decided is, whether any of them are subject to an impurity that

* See " Annals ;" Vol. 1.

might create an error.

As regards the zinc, that there are some instances of the zinc of commerce containing a small quantity of arsenic, is not to be denied; and that this will give rise to an impure hydrogen, when acted upon by pure sulphuric acid and water. But then, again, there is nothing more easy than to procure zinc of commerce which will generate hydrogen perfectly free from arsenic, nowithstanding there are some who say that purified zinc is not free from this metal; but it is evident that they must be mistaken, as any one may see by making the experiment, which, as it is a very simple one, it would be well to perform; and I feel confident in saying that little or no difficulty will be found in procuring ordinary zinc of the ... ty to be used in Marsh's apparatus.

... acid may contain arsenic, when manufactured with ... obtained from pyrites holding that substance in combina- ... out a simple distillation will serve to rid it of this impurity.

After placing the zinc, sulphuric acid and water in the apparatus, replace the cork with the glass tube inserted in it, and when the hydrogen has been allowed to generate a sufficient length of time to expel the air, inflame it as it issues from the extremity of the tube; if a porcelain plate be now applied to about the middle of the flame, and no stain or spot be obtained, we have the best evidence of the purity of our materials.

Another apparent objection to the apparatus, is, that the intro- duction of animal matter, either solid or liquid, causes the forma- tion of a large quantity of froth, which arrests the progress of the operation. This, however, is so easily remedied, that it need hardly be considered an objection. If the froth be not in too great quantity, it will suffice to introduce a little oil, which will serve to arrest its formation. Another method is to turn the liquid out of the phial into a funnel, with the finger placed upon the lower ex- tremity, the froth will at once rise to the surface, and by taking away the finger the liquid will pass out perfectly free from it. Again, if care be taken to carbonize the matter before using it, this obstacle will be removed. There is still another means, and I find it to succeed very well in most instances; it is to pour the sulphuric acid destined for the formation of the hydrogen first upon the animal matter, and then pour the two upon the zinc and water; it would appear that a partial carbonization takes place. No doubt most persons will now perceive that this objection possesses no weight, and vanishes altogether before the means proposed to encounter it.

The next part of this question to be examined, is, what sub- stances besides arsenic produce stains with this apparatus, and is there no danger of confounding them with that of arsenic? They are antimony, sulphur, phosphorus, and iron. Before speaking of

their distinguishing characteristics, it would be as well to say a few words concerning that produced from arsenic.

The arsenical stain is highly metallic, of a steel colour, with a slight reddish tinge, and borders of a dark rusty color ; but to have a proper idea of its appearance, as well as that of the others, one should see them. It is volatile by heat, and is dissolved by cold nitric acid, which solution gives to the nitrate of silver a brick-red precipitate, the arsenate of silver.

The antimonial stain is less metallic than the former in its appearance, also blacker, and when very dense even smutty. It can be volatilized, but with great difficulty, and not before it has been as it were chased about the surface of the porcelain. It is soluble in cold nitric acid, which solution gives no red precipitate with nitrate of silver.

The next stain to be spoken of, is the compound one, of arsenic and antimony ; at the same time mention will, be made of the method adopted by M. Orfila for detecting the one or the other of these metals in it. It partakes, as might be expected, of the characters of both the metals that enter into its composition, being partially volatile, soluble in nitric acid, from which the brick-red precipitate of arsenate of silver can be obtained. M. Orfila proposes a plan of separating the constituents of this stain, and of testing each by itself. He proceeds as follows : having collected a number of the compound stains upon a porcelain plate, he dissolves them in nitric acid, which solution being poured into a capsule, is evaporated to dryness, and a residue remains composed of antimonious acid and a mixture of arsenic and arsenious acids. Upon this residue a little water is poured, and the capsule slightly heated, which enables the water to dissolve more readily the two last mentioned acids. The antimonious acid being allowed to settle, the clear liquid is decanted, and a few drops of nitrate of silver being thrown upon it, the brick-red arsenate of silver is formed, which is sometimes mixed with a considerable quantity of a yellow precipitate, the arsenite of silver. This will, however, rarely happen, if a large quantity of nitric acid has been used ; for by so doing, only an extremely small quantity can remain in the state of arsenious acid, the oxidation being carried a degree higher. Nevertheless, if the entire precipitate produced by the nitrate of silver be yellow, it can have no effect in destroying the fact concerning the presence of arsenic, as it only indicates that it has met with arsenious and not arsenic acid. But to return to the substance left in the capsule :—A small quantity of muriatic acid, slightly diluted, is poured upon it, which immediately dissolves it : A current of sulphuretted hydrogen is now made to pass through this solution, when the orange-colored sulphuret of antimony is formed. Another process will be stated for arriving at the same

end, when mention is made of a method by which I propose to separate arsenic from organic substances. The importance of studying this double tache will be evident to every reflecting mind, for it may not unfrequently happen that the physician called upon to administer to a person supposed to be laboring under the effects of arsenic, may use tartar emetic to disembarrass the stomach of the supposed poison; death taking place, an examination is made of the liquid found in the stomach and intestines, of urine, &c., by means of Marsh's apparatus, and a stain is obtained which is not easily volatilized, and which has the appearance of antimony. What then is to be done? Why, we are to proceed in our experiments as just stated, and the two metals, if both be present, are to be separated.

The stain from sulphur has all the characteristics of that substance; colour yellow, volatile, with a suffocating smell, &c. There is not the least probability of confounding it with any thing else.

The stain from phosphorus possesses three different shades, brilliant white, brilliant yellow, and rust color. When the quantity of phosphorus is very small, either the first, or only the first and second are seen. It is volatile, reddens litmus paper, and is insoluble in cold nitric acid, so that there cannot be the least occasion for mistaking between this and arsenic.

The next substance that produces a stain when introduced into the apparatus in question, is iron, but it ought not to be classed with the others, for I am firmly convinced that it is not due to any iron that may be dissolved by the hydrogen; in other words that there is no ferruginous hydrogen. My reason for so believing is based upon the following facts:—If we desire to obtain this stain, a considerable quantity of iron, or some salt of iron, must be used, and the gas made to generate rapidly. Now observe what must take place. The action of the liquid being violent, a spray is formed, which consists of the dilute acid and whatever salts it may hold in solution, in this case iron as one; this spray passes along with the hydrogen through the jet; the hydrogen being now ignited, a porcelain surface is placed in contact with the flame, which, becoming heated, enables it to evaporate the water from the salt of iron, which deposits itself, and afterwards becomes decomposed by a continuation of the heat, the peroxide being left. If we still retain the iron in the apparatus, but make the action not very brisk, no stain will exibit itself upon a smooth porcelain surface; but if the broken surface of a piece of porcelain is placed in contact with the flame, a slight black deposit is formed, consisting, as in the former case, of peroxide of iron; the reason of this is, that it is a more convenient surface for retaining the particles of the solution of iron, thrown out in company with the hydrogen. Again, this stain is evidently an oxide, which it is not probable

would be the case, had the iron been chemically combined with the hydrogen. Another reason is, that if the gas be made to traverse water or chloride of calcium before ignition, no stain will be formed, for the iron mixed with the hydrogen is retained by either of these means. This stain has been perhaps more noticed than it deserves. It is not easily produced, and is distinguished by its not being volatile and its solution in any of the strong acids, giving a blue precipitate with ferrocyanuret of potassium.

There is yet one other stain to be spoken of. If the flame of the apparatus, containing only zinc, sulphuric acid of water, be prolonged for some time upon one spot on the porcelain, an opake white stain will be perceived, which I propose to explain in the same way as the last, the cause of it being the oxide of zinc instead of iron, this oxide arises from the decomposition of a small quantity of sulphate of zinc thrown out with the hydrogen, but still it is a thing hardly worthy of notice, for after it is formed it is difficult to see it.

What is the conclusion to be arrived at concerning Marsh's apparatus, after what has been said? Why, that it should be considered as the most valuable instrument that the medico-jurist possesses, to assist him in his experiments upon the poison in question; for with proper care all the objections to it can be easily remedied, and the character of each stain is so well marked that they need never be confounded, as will be seen by glancing the eye over what follows.

Arsenic—Steel colour, highly metallic, easily volatilized by heat, readily dissolved in nitric acid; the nitric acid solution gives with nitrate of silver a brick-red precipitate.

Antimony—Colour darker than steel, metallic, with difficulty volatilized by heat, readily dissolved in nitric acid; the nitric acid solution gives with nitrate of silver no precipitate.

Sulphur—Colour sulphur-yellow, easily volatilized by heat, not soluble in nitric acid, gives the well known smell of sulphur when burnt.

Phosphorus—Colour brilliant from white to red, easily volatilized by heat, not soluble in nitric acid, reddens litmus paper.

Iron—Colour black but slightly metallic, not volatilized by heat, soluble in nitric acid; the nitric solution strikes a blue colour with ferrocyanuret of potassium.

Examinations for arsenic in case of poisoning.

Under this head will be answered the fourth question, which is, What are the best means not only of detecting, but of ascertaining the quantity of arsenic in combination with animal matter?

Arsenious acid, it is well known, does not destroy life by a mere local action upon the stomach and intestines, as do many of the strong acids, but that its poisonous effects are exhibited after it

has been absorbed into the system. It is true that it inflames the mucous membrane of the intestinal canal, but that is comparativey of minor importance to its other effects. If it be absorbed, in what secretions and in what oagans is it to be found in the greatest abundance? The urine is the first secretion in which arsenious acid exhibits itself, and in that not long after administration. This fact, then, makes it important to preserve the urine of a person who we may suppose has been poisoned by this agent, for making the necessary medico-legal examination, and in cases where death does not occur it ought to be considered of more value than the matter vomited.

After the bladder, the liver and heart next demand our attention, for one may calculate with almost absolute certainty upon finding this substance in these organs, had it been employed? The brain and inner muscles of the thigh, in most cases of poisoning by arsenious acid, contain it in sufficient quantity to be exhibited by means of Marsh's apparatus. Other portions of the body frequently contain it in small quantities, but if we have the organs already mentioned, along with the stomach, intestines, and their contents, it will be all that it is important to experiment upon.

In commencing the experiments we should be furnished with the following materials, viz. nitric and sulphuric acids, nitrate of potash, zinc and water. Their purity should be fully established before they are employed.

The use of the nitric acid is to carbonize the animal matter, and in that way to develope any arsenic that it may contain This process is of vast importance as will be seen by the following example. Let the liver contain the largest quantity of arsenious acid that can reach it by the process of absorbtion, and it may be boiled for six hours, in distilled water. without giving up the smallest portion of the poison; whereas, carbonize it first by the aid of nitric acid, and then pour the water upon it, and results of an entirely different nature will be obtained, There are no doubt, two reasons for the cause of this; the first is, that the arsenious acid has undergone some chemical change, which renders it insoluble; the second is, that the liver is completely broken up by the nitric acid, and the arsenic, in whatever state it may have existed, is now converted into arsenic acid. The nitrate of potash is sometimes employed to destroy the carbon after the nitric acid has acted upon the animal matter. The sulphuric acid, zinc and water, are the elements of Marsh's apparatus.

The fluid of the stomach and intestines should be first experimented upon; and this may be introduced into the apparatus either in its crude state, or after having undergone carbonization by heat or nitric acid. If it be employed uncarbonized, we may expect a great quantity of froth, which may be obviated in some

measure by the means already mentioned. When we carbonize the matter by heat, it becomes necessary to introdude a small portion of pure caustic potash during its evaporation, which combines with arsenious acid, forming arsenite of potash, a substance not easily volatilized. If nitric acid be used, we first evaporate the liquid to dryness, then pour upon it two or three times its bulk of nitric acid, and again evaporate to dryness, when we may expect an almost complete destruction of the animal substances. The carbonized matter, formed either by heat or nitric acid, with whatever it may contain, is digested for a little while in pure water, which easily dissolves the arsenic, now in the states of arsenite of potash, and arsenic acid. Filter, introduce the liquid into the apparatus, when we may expect to exhibit the metal upon a porcelain surface. In experimenting upon the urine, the same steps are to be taken.

The examination of the liver is conducted as follows :—Two or three pounds of it are first dried by a gentle heat, and then digested with about three times as much nitric acid by weight, until the mass becomes perfectly dry ; water is now poured upon it, and heat applied for ten or fifteen minutes ; the liquid is now filtered, and tested by the apparatus. The heart muscles, brain, &c., if examined, must undergo the same process.

There is yet another advantage, that has not been mentioned, connected with the carbonization of animal matter by nitric acid ; it is, that if antimony be present, it becomes converted into antimonious acid, which is insoluble in water.

Mention has been made only of the manner of separating arsenic from animal matter, by the aid of Marsh's apparatus, and it may be well to give a brief account of one or two new methods adopted by Mr. Persoz to serve the same end, with this additional advantage, that it enables one to ascertain the exact amount present.

The suspected materials, after having sufficient reason to suppose that they do not contain a poison of organic origin, or mercurial or antimonial preparations, are subjected to the action of dilute nitric acid, in order to destroy those parts that are decomposed by this agent. Most of the organic substances having undergone this decomposition, the residue is diluted with water, and heated to the boiling point, and then left to cool ; the fatty and resinous substances rise to the surface, are taken off and washed, and the washings added to the original liquid, which is then evaporated to the consistency of syrup. The liquid now has a dark brown tint, an evidence that it still contains a quantity of organic matter. Nitric acid, therefore, is again added, and a new oxidation takes place. We recommence to evaporate, and continue to add nitric acid, until the liquid acquires a lively orange tint, when a careful evaporation is commenced, first over a naked fire,

and then by the means of vapour. An approximate value being made of the quantity of residue, twice and a half times its volume of pure nitrate of potash is added, for the purpose of completing the oxidation. Water is next poured upon these materials, and heat applied and continued until the water is evaporated and the residue is dry ; by this means an intimate mixture is brought about between the nitre and animal substances. In this part of the operation, care must be taken to extend the matter as much as possible over the surface of the capsule as soon as it begins to dry. The capsule is now heated almost to redness, when a deflagaation takes place, and propagates itself through all the matter submitted to analysis, destroying all remains of organic matter. Care must be taken that the nitre be in sufficient quantity, for if not, this part of the process must be gone over a second time.*

After the deflagration has taken place, it may be well to heat the residue a second time, in a capsule of platinum or silver, to redness. The residue consists generally of the following substances: the excess of nitrate of potash mixed with the nitrite of the same substance ; carbonate of potash ; the salts existing in the organic matter, as well as those formed during the process, such as the phosphates, sulphates, and chlorides, free oxides, and finally arsenic acid, free and in combination with potash. This compound mass being pulverized, is mixed with one and a half times its bulk of hydrochlorate of ammonia, introduced into a retort, and heated to a dull redness. By the action of the heat, the chlorine of the hydrochlorate of ammonia combines with the potassium, and the hydrogen of the ammonia reduces the arsenic acid to the state of arsenious acid, which sublimes with the excess of hydrochlorate of ammonia, and is condensed on the upper part and neck of the retort. Other chemical changes take place, but they do not modify the one just stated. The operation being finished, the retort is broken, and the substance sublimed dissolved in water strongly acidulated with hydrochloric acid, and through this solution is passed sulphuretted hydrogen, which enables us to obtain all the arsenic that was originally in combination with animal matter, in the state of a pure sulphuret. This process is somewhat complicated, but each step is so clear, that with proper care, the most satisfactory result might be obtained in almost all cases.

In fulfilling the promise as regards the stating of all important facts lately brought to light concerning this too universal poison, I will mention two other methods of separating and of ascertaining the quantity of arsenic in combination with organic substances. They are both modifications of Marsh's apparatus ; one is proposed by M. Lassaigne, and the other by myself.

M, Lassaigne, instead of igniting the arseuuretted hydrogen, and obtaining the arsenic upon a cold surface, passes it through a

solution of nitrate of silver, which it has the property of decomposing. The solution first becomes brown, and then a deposit of oxide of silver takes place. After the gas has ceased to pass, a quantity of muriatic acid is poured upon it, which decomposes what nitrate of silver remains, and converts the precipitate into chloride of silver. There remains now in solution arsenic and arsenious acids, and by filtering or evaporating to dryness, they are obtained.

I propose to pass the arsenuretted hydrogen through a tolerably strong solution of iodine in alcohol, in order to decompose it, which it does effectually, there being formed the iodine of arsenic, which remains in solution. All that is now necessary to be done, is to evaporate nearly to dryness, until red fumes make their appearance, and then pour twice or thrice as much nitric acid as there is residue in the capsule. Heat is again applied, and the evaporation continued to dryness, when there will be remaining arsenic and arsenious acids. The nitric acid in this case converts the iodine of the iodide of arsenic, and the free iodine into iodous and iodic acids, both of which are evaporated with the undecomposed nitric acid.

Iodine also decomposes the antimoniacal hydrogen, first forming iodide of antimony, which the water of the alcohol immediately decomposes into hydriodic acid and oxide of antimony, the latter of which is precipitated. This then becomes a convenient mode of separating the two substances, antimony and arsenic, for by passing the compound gas through the alcoholic solution of iodine, it becomes decomposed, and iodide of arsenic is formed, which remains in solution, and the oxide of antimony which is precipitated, can be separated by means of a filter. This, however, is not the plan that I would propose; it would be better to invert the precipitate as well as the liquid into a capsule, evaporate and treat with nitric acid as in the case of arsenic, when we shall have left the arsenic, arsenious and antimonious acids, the two former of which are soluble in water.

One may now imagine that there is nothing easier for the medico-jurist than to form a correct opinion, and one that cannot be doubted, concerning the poisoning by arsenic. Whether such is the fact or not, he will find, in some cases, that all his skill and care will be required, not only to convince the minds of others, but even his own. It may not unfrequently occur, that arsenious acid has been the poisoning agent, and still great difficulties present themselves, which are enumerated in almost all works on medical jurisprudence.

There is one very important fact to be kept in mind with reference to examinations of this character; it is the medical treatment that the individual has been subjected to before death. For

instance, the treatment by diuretics, which will be mentioned presently, may remove from one or more organs the poison previously contained in them, and still the impressions made upon them be too strong to be recovered from. How then is this difficulty to be be removed? By carefully preserving all the urine—an observation which is of such importance that it should not escape the memory of any physician.

What are the best means of treating the poisonous effects of arsenic?

A few words upon two new methods of treating the effects of arsenic, will conclude this article, already extended much farther than I had intended.

The remedies that we already possess, are, at the very best, but feeble agents to combat the effects of this poison. The one most to be relied on is the hydrated peroxide of iron, it being a veritable antidote to poisoning by arsenic; however, there are some objections, the principal of which is the slowness of its absorption, for it is only where it encounters the poison that its salutary effects are displayed, by forming with it an inert arsenite of iron.

A treatment proposed in Italy, is the administration of stimulant draughts every two or three hours, consisting of brandy one ounce, wine two ounces, bouillon (the liquid produced by boiling beef or other meat in water) four ounces. It is based upon the supposition that the effects of arsenic are atonic, the truth of which is far from being established. Instances are given where this treatment has proved efficacious, although I have witnessed experiments made with it, in comparison with simply tepid water, where the latter proved to be the most successful of the two.

The treatment by diuretics is one that deserves some consideration; it is advanced by M. Orfila, based upon numerous experiments. It has been more than once stated, that the urine exhibits a large portion of the arsenious acid absorbed into the system, and it seems very rational to suppose, that if this secretion could be augmented by any means, that the quantity of arsenic carried off would be also increased. It has been observed, that where equal quantities of arsenious acid have been given to two dogs of equal vigour, and if one died and the other survived its effects, we find that the latter had urinated largely. The diuretics merit some attention; they are not to be used until the stomach is emptied of its contents by some mild emetic and tepid water.

In a medico-legal examination for antimony, most of the steps that have been proposed in the case of arsenic can be followed; the principal modification is where either nitric acid or heat has been used to carbonize the animal matter; for in that case, muriatic acid slightly diluted is to be employed as the dissolving agent instead of water.

Paris. December 6, 1840

The employment of Iodine as a reagent for Hydrosulphuric Acid ;
by M. ALPHONSE DU PASQUIER.*

TO THE EDITORS OF THE AMERICAN JOURNAL OF SCIENCE.

Gentlemen,—THE original of this article was published in the March number of the Annales de Chimie et de Physique, and the importance of its being generally known to those who devote any of their time or attention to the investigation of our mineral waters, which are more or less impregnated with Hydrosulphuric acid and the alkaline hydrosulphates, has induced me to transmit to you, for publication, a translation of such parts as explain the method of employing the reagent in question, and the conclusions that M. Alphonse has arrived at by his varied experiments.

The sulphohydrometer that is described, is of easy application, and enables one to obtain very accurate results in a short space of time, particularly when use is made of a table that I have calculated and annexed.

As regards the strength of the tincture of iodine, that is altogether optional with the individual who employs it ; it being only requisite to have a knowledge of the amount of iodine contained in a measured portion of the liquid. I should propose, as most convenient, that each division on the sulphohydromer should answer to $\frac{1}{10}$ of a grain of iodine, and a subdivision to $\frac{1}{100}$.

Yours respectfully,   J. LAWRENCE SMITH, M. D.
Paris, Sept. 20, 1840.

" To determine the proportion of hydrosulphuric acid, either free or in combination in sulphureous waters, is an operation attended with considerable difficulty, and of which the results are far from being certain. All the methods employed to arrive at this end, comprising even the process of M. Grotthuz, (the employment of ammoniacal nitrate of silver,) adopted by M. Anglada, and the generality of chemists of the present day, present great difficulties of detail, and are, as has been demonstrated in my first memoir, subject to gross errors, particularly when we obtain a sulphuret more or less impure ; and moreover when the quantity of hydrosulphuric acid is very minute they cease to act.

" In my researches upon the waters of Allevard, the uncertainty of these methods, made me desire to discover some process more satisfactory, when, employing as a reagent the alcoholic tincture of iodine, (it not being among those ordinarily used ;) I found that the decomposition of the hydrosulphuric acid by this metalloid, was complete and instantaneous, and that one could determine, in a very easy manner, the precise point at which the decomposition of the '

* Silliman's Phil. Journal, for January, 1841.

hydrosulphuric acid is achieved, or when the iodine no longer enters into combination. I conclude, from this fact, that, with a tincture of which I know before hand the proportions, I shall be able to ascertain, by the quantity of iodine employed to saturate a litre of the sulphurous water, the precise amount of hydrosulphuric acid which it contains.

" Moreover, I am able to ascertain the quantity of iodine employed, without the use of a balance, by the means of an instrument which I call a sulphohydrometer. This instrument is a graduated tube, which allows the tincture of iodine to flow from an elongated extremity with a capillary opening, the other extremity being closed by a stopper.

" To employ the sulphohydrometer, we take a certain quantity of the sulphurous water which we may wish to analyze, and placing it in a porcelain capsule, add a few drops of a very clear solution of starch, and then allow the tincture of iodine to fall upon it, drop by drop, from the instrument, previously filled to the point marked 0°, and continue the addition so long as no change takes place in the colour of the water, favouring the reaction by agitation with a glass rod. So long as there remains the smallest trace of hydrosulphuric acid, the iodine disappears as fast as it is introduced; and the starch, upon which iodine in a state of combination does not act, gives rise to no colouration of the liquid until the hydrosulphuric acid is completely saturated, when the minutest addition of iodine at once strikes a blue colour with it. We then examine how many degrees of tincture have been employed, and knowing the strength of it, we are enabled to calculate the quantity of hydrosulphuric acid decomposed by it.

" This method of analysis, independent of its affording results of the most accurate character, has the additional advantage of being executed in so short a space of time, that one may make from fifteen to twenty experiments in less than one hour, and at the same time be perfectly sure of committing no error. It is also so easily put into practice, that any physician or intelligent person may apply it, and assure themselves daily of the variation in the strength of the sulphurous waters caused either bo atmospheric changes or an admixture with rain water.

"The conclusions that I have arrived at by my experiments are as follow :—

" 1st. That the best known reagents for hydrosulphuric acid are subject to great objections, since they do not indicate even notable quantities of the acid, free or combined ; a circumstance that explains why its presence has not been demonstrated in waters whose physical properties rank them as sulphureous.

" 2d. That an alcoholic solution of iodine, employed along with starch, is a very sensible reagent for hydrosulphuric acid, free or

in a state of combination.   It can detect, in an undoubted manner,
(by a comparative examination, with common water,) a drop of
concentrated solution of any of the alkaline hydrosulphates, dissemi-
nated one hectolitre[*] of water, although the known reagents lose
their action when the same quantity is disseminated in only ten
litres.[†]

   " 3d. That with the tincture of iodine and starch we can
recognize infallibly, in the weakest sulphureous waters, in those
where ordinary reagents are useless, not only the presence, but
also the quantity of hydrosulphuric acid, either free or in a state
of combination.

   " 4th. That the known processes for determining the proportion
of hydrosulphuric acid, free or combined, are so long and difficult
that their result is uncertain and incorrect, especially in regard to
waters possessing but little of the sulphureous principle."

*Table of the quantity of Hydrosulphuric Acid decomposed by
quantities of Iodine from $\frac{1}{100}$ to 10 grains.*

IODINE.	HYDROSULPHURIC ACID.		IODINE.	HYDROSULPHURIC ACID.	
Weight in grains.	Weight in grains.	Bulk in cubic inches.	Weight in grains.	Weight in grains.	Bulk in cubic inches.
.01	.001351	.003691	.60	.08106	.22146
.02	.002702	.007382	.70	.09457	.25837
.03	.004053	.011073	.80	.10808	.29528
.04	.005404	.014764	.90	.12159	.33219
.05	.006755	.018455	1.00	.13510	.36910
.06	.008106	.022146	2.00	.27020	.73820
.07	.009457	.025837	3.00	.40530	1.10730
.08	.010808	.029528	4.00	.54040	1.47640
.09	.012159	.033219	5.00	.67550	1.84550
.10	.013510	.036910	6.00	.81060	2,21460
.20	.027030	.073820	7.00	.94570	2.58370
.30	.040530	.110730	8.00	1.08080	2.95280
.40	.054040	.147640	9.00	1.21590	3.32190
.50	.067550	.184550	10.00	1.35100	3.69100

   * Hectolitre, about 26½ gallons.   † Ten litres, about 2½ gallons.

Description of a Double Exciting Cylinder Electrical Machine.
in a Letter to the Editor.

Sir,—In consequence of your invitation to the readers of your excellent Annals of Electricity, &c., to furnish you with any new or original plans of apparatus that may in any wise promote the interests of the science, I have taken the liberty of forwarding you the enclosed sketch (Fig. 6, Pl. I.) and description of one I contrived and made about ten years ago :—

When first made, it was upon *Nairne's* principle, with a parallel positive and negative conductor, both the same size, and each the length of the cylinder and two-thirds of its diameter, which was made out of the usual proportions for compactness, 12 inches long and 13 inches diameter ; there need be no fear about the fluid running off, if there is a sufficient number of clean and sharp points of brass wire running along the face of the cylinder from edge to edge, as in the sketch.

I thought it a great waste of time and labour to excite in one place only about 30 square inches of surface, out of nearly 450, upon each revolution of the cylinder, as is the case upon *Nairne's* plan ; and that there was nothing to prevent a greater amount of useful excitement, if the fluid generated could be effectually carried off to the prime conductor, which object I have attained, so as to double the power, or nearly so, of the machine, and give it a compact and better appearance, without adding much to the expense, which plan and advantages may be easily adapted to any old machine now in use.

The construction will be easily understood by reference to the sketch. The prime conductor is insulated by a stout glass pillar, cemented into a wooden socket that screws on to the top of the front pillar of the machine on the under-side of the conductor ; rising inwards are sockets, into one of which a hollow arm, about 1 inch diameter fits, that extends across the top of the cylinder, fitted with a row of collecting points, each about an inch long, and apart, so as to extend to the edges of the cylinder. From the end of the arm nearest the conductor proceeds a piece of No. 1 brass wire, bent into the size and curve of the edge of the cylinder, or rather larger ; reaching round to the bottom of the cylinder, to which is attached another similar arm, to carry the fluid from the second cushion ; the curved wire is bent at each end about 3 inches, so as to make the arms sit parallel to the cylinder, and they pass into a cork cemented into the ends of the arms, with a bit lipped out of the edge to receive the wire to keep them steady in their places : from the front end of the prime conductor proceeds another arm that also fits into a similar socket, at the bottom end of which is a wire with a ball to adjust to the proper height for charging jars, &c., as represented in the plan.

The frame and negative pillars are made of seasoned beechwood, baked

to complete dryness, and immediately worked up and coated with varnish, made of equal parts of gum mastic and shellac dissolved in alcohol, the pillars coated eight or ten times over as fast as they become dry and hard, which I find secures as perfect an insulation as glass when wiped clean and dry, with the advantage of strength and no-risk of breakage. The conductors are made of sheet tin and gilt over with Dutch metal leaf, having one coat of the varnish over it to prevent their being dis* coloured by the action of the atmosphere, it does not destroy their effi- ciency, and gives them a smart appearance at little cost. The axis that the cylinder is mounted upon runs completely through the cylinder, and is about half the diameter of the holes it passes through, which allows of its being so wedged as to make it the truest on the surface for excitement; it is then secured in its place by wooden caps covering the projecting necks cemented to them and the end of the cylinder.

If you should deem this worthy of your notice in your Annals, and enter- tain any desire for other kinds of apparatus not commonly in use, I have another or two contrivances for useful purposes that I shall have great pleasure in submitting to your notice.—I am, Sir, your most obedient servant,   RICHARD EATON.

Muchpark Street, Coventry.
     April 30, 1841.

Lightning Conductors.   By J. MURRAY, Esq., F.G.S., &c.

SIR,—As your columns seem ever open to communications which relate to the public well-being, or are connected with the question of individual benefit, I venture to trespass on your time, and to solicit a niche in your columns.   Favouritism, I fear, mingles often in Parlia- mentary Committees and Government Commissions of Inquiry, and hence a one-sided and partial view of the question terminates the case. I had no reason to be disappointed in the very imperfect and useless investigation on the subject of shipwreck, consigned to a Committee appointed by Parliament, when Mr. Dennett was not examined, and his rockets, every way superior to the invention of Manby, formed no item in the recommendations of the Committee; but I think I have a right to complain in the case of the favouritism shown to Mr. Harris, of Plymouth, in the recommendation for adopting his conductor in the British Navy. The opinions of Faraday, Daniell, and Wheatstone, the personal friends of Mr. Harris, seem to have been a *ne plus ultra*, and were made to bear entirely and exclusively on *his* lightning rod, as if no other had been in existence, or ever proposed. It may be also reasonably asked why Mr. Crosse, of Broomfield, was not examined on the occasion—one who is perhaps more practically conversant with atmospherical electricity than any other?

My lightning conductors have been long before the public, and their efficiency fully proved in many a thunder-storm during the last twelve years, so that there is nothing hypothetical or problematical in reference to my invention. More than thirty of these conductors have been erected in Yorkshire alone. It has been described in my work on the subject, as well as in the " Annals of Electricity." I am not aware that Harris's conductor has ever been erected, and Mr. Roberts and Mr. Sturgeon have controverted its efficacy. Mr. Harris sues for their *gradual* introduction into the British Navy; but, if there be no doubts as to their complete efficiency, or any " compunctious visitings of nature," why not entirely and at once ? ;

My lightning conductor comes before the public recommended for its *economy* and *simplicity*, while it stipulates *all the conditions* which should enter into the composition of a complete storm conductor, calculated on the sound principles of induction. Its perfect security has never been questioned, nor has any one ventured to doubt the absence of any of the elements of a good lightning rod. It may certainly be received as an axiom, that a bad or imperfect conductor is infinitely worse than no conductor at all.

The elements of a good conducting medium for lightning in the storm, are these :—A provision for its prompt and safe receipt—a continuous conducting medium for its subsequent transit, that being a material fraught with the best conducting character—and further, a safe escape to the subsoil, or to water. As lightning penetrates to a depth scarcely appreciable, the substance of good conductors, a considerable superficial extent is requisite.

I employ copper gas piping, screwed together in definite lengths, and terminating above in an elongated and pointed solid copper pyramid. Immediately beneath this terminus, the hollow stem is perforated, so that the lightning may also penetrate the interior. This interior surface may be preserved for ever free from oxidation. The continuous rod enters the earth, and terminates in a well, or the subsoil. In order to protect the external surface from oxidation, I apply the galvanic principle of strips of zinc, insulated by bands of leather, and connect the zinc with the copper stem by copper wire.

It is easy to see the application of this conductor to ships. The higher parts of the conductor may be made to slide into each other like the tubes of a telescope, to meet the contingencies of the topmast or topgallant mast being struck (lowered) in a storm. The lower end of the conductor passes through the keelston.—Yours, &c.,

May 18th, 1841?   *J. MURRAY.

P. S.—Apart from the question of " the lateral explosion," I consider a *smooth rounded surface* essential to the character of a good conducting

medium; a conclusion, of course, at variance with Harris's strips of
metal. This communication has been transmitted to, and its receipt
acknowledged by, the Earl of Minto.

LECTURES ON ELECTRICITY, &c.
LECTURE IX.

The electrical light which was so brilliantly exhibited in some of our
latest experiments in the last Lecture, is capable of being moulded into a
great variety of very beautiful experiments, some of which I will presently
show you. You have already seen that a spark taken from the prime
conductor, is capable of penetrating and making its way through a stra-
tum of air, of the usual density, more than twelve inches thick; and it
will traverse a much thicker plate of air that is attenuated, because, when
in the latter condition, the air is a much better conductor than when it is
dense: but in proportion as we attenuate the air through which a current
of electric sparks is passing, the light becomes more feeble, and that
brilliancy which the electric fluid exhibits in air of the common density, is
entirely lost in highly attenuated air.

There is an experiment, however, usually called the Falling Star Expe-
riment, and of which I shall speak more particularly in a future Lecture,
in which the light is very brilliant when the air is attenuated to a certain
degree; but in that case we transmit a greater quantity of the electric
fluid, in one spark, than can be accumulated on the surface of the
prime conductor, for which purpose we employ an instrument called the
Leyden jar.

A very simple piece of apparatus, for showing the electrical light in a
pleasing manner, is made by a few leaden shot, (duck-shot, for instance)
strung on to a thread of silk, keeping a space of about one-tenth of an
inch between every two. Twenty or thirty, or even more, of these small
globes, may be thus arranged, and the ends of the thread cut, or burnt,
close to the extreme globes of the series, so that no loose ragged end may
interfere with the experiment; then, by holding one of the extreme shot
between the finger and thumb, and letting the rest hang down to the ball
of the prime conductor, so that the lowest do not touch it, you will see a
beautiful series of sparks traversing the silken thread from one end to the
other.

In this experiment, it will be observed, the sparks appear between the
leaden globes only, and no light is observable whilst the fluid traverses
the balls; therefore, the lowest shot first receives the sparks from the
prime conductor, and delivers them to the second, which again delivers
them to the third, and so on, from one to the other, until they arrive
at the hand, where they are lost.

This instrument, it is true, is but a very rude substitute for some of those elegant pieces of apparatus which are usually employed for the same purpose, but, at the same time, it answers all the purposes of explanation quite as efficiently as they do. The apparatus generally employed are pieces of glass, either flat, or in the shape of tubes, partially spangled with discs of tin foil. They are generally constructed very tastefully, and the spangles of foil suitably arranged, so as to diversify the route of the sparks in a variety of ways, which give to their display a more pleasing and interesting effect than could otherwise be produced.

The glass tubes are capped at both extremities, with hollow spheres of brass, well polished and lacquered, and the spangles of tin foil are arranged round and round the tube, in the form of a spiral, from one end to the other, taking care that no two of them touch one another. The spangles are attached firmly to the glass, by means of strong gum-water, and the whole occasionally covered with a coating of lac varnish. The spangled tube, however, is sometimes placed inside of another glass tube, which, when capped by brass spheres at both ends, excludes all dirt and moisture in future. This method of fitting up these spangled tubes is, therefore, much preferable to that of using one tube with varnish only. When one end of this apparatus is held in the hand, and the other presented to the ball of the conductor, a series of sparks are seen traversing its surface between the spangles, and, consequently, in the same spiral line.

Another form of apparatus for this purpose, is that of a long strip of plate glass, on one side of which an arrangement of metal spangles, from one end to the other, is fastened by means of gum-water, and the line of electric light displayed by the sparks, will vary with every variation in the arrangement of the spangles. The opposite side of the glass is usually varnished in bands of different colours, which vary the colour of the sparks accordingly with the colour of the medium through which they are seen. When a spiral arrangement of metallic spangles is placed on coloured glass tubes, such as blue, purple, violet, &c., the sparks will assume different colours as they proceed from one end to the other. They are perfectly white whilst passing along that side of the tube next to the spectator, but will assume the colour of the tube at those parts of its route, which are behind it. The varieties of arrangements, or routes which the electric fluid may be made to assume, are almost endless in this class of experiments: but I shall pursue them no farther until I have made you acquainted with some other interesting facts.

There are some curious circumstances connected with the electro-con- duction of bodies, which I did not mention whilst classifying them in our second lecture. They are these. Those solid bodies which have been called non-conductors are considered to be such, only whilst in a state of solidity, or whilst their parts are in a certain state of aggregation. For

instance, glass whilst solid and perfectly dry, is a non-conductor; but melted glass is a very good conductor. Crystals of several salts also possess the same faculties; for although non-conductors whilst solid, they become conductors when in a state of fusion by heat. Green wood is a conductor of electricity, but baked wood is an insulator. If the baked wood be converted into charcoal, it is again an excellent conductor; but burn it to ashes and it loses the conducting faculty.

I will now show you a very satisfactory experiment on this point. You are already aware that a solid stick of sealing wax is a non-conductor; but if we warm the same stick, so as to make it quite soft, and then apply it to the cap of Bennet's Electroscope, whose leaves are already in a state of electro-divergency, you will soon find that the electric action disappears, and the gold leaves hang down close together. A tube of glass presents us with a similar fact, for although a non-conductor whilst cold, it becomes a conductor by being made to assume a high red heat; and better still when white hot.

By taking advantage of these facts, an illustrative experiment has been established, whose effect is called the Electro-spinning of sealing wax. I have a stout brass wire, which will just fit the hole in the end of the prime conductor of the machine. It is somewhat pointed at the other end, which is stuck into a short stick of soft sealing wax. When the wax has become hard and cold it is no conductor, but, as we have already seen, when melted, or even softened considerably, it becomes a conductor. I place this wire in the end of the prime conductor, and afterwards soften the wax by the flame of a spirit lamp; and now, whilst the machine is in operation, I hold a blank card in front of the softened sealing wax, and I find that a considerable quantity of the melted wax has been blown off from the wire, and is attached to the card in beautiful fine filaments, as soft and flexible as flocks of wool, or silk. By repeating this experiment with several cards, we have an opportunity of varying the arrangement of the fibrous wax in many ways. A great quantity may be collected upon the surface of one card, and placed in a cabinet, as a curious production of this species of electric action.

I will now introduce to your notice another piece of electrical apparatus, called the Leyden jar. It is represented by Fig. 10. It consists of a glass jar, sometimes perfectly cylindrical, and sometimes of the shape shown in the figure; but its shape is a mere matter of choice, for I shall have to show you very shortly, that a piece of flat window glass possesses the same powers as when in the shape of a jar, or of any vessel whatever; but the instrument is more frequently used in the shape of a jar, as in Fig. 10, than in any other.

Fig. 10.
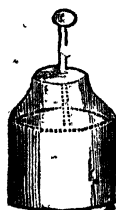
The Leyden jar is lined with tinfoil from the bottom to within about two inches and a half of the top. Some persons have three or even four inches of the upper part of the jar uncovered with the foil. Its outside is also covered with tinfoil to the same height as the lining. The foil may be stuck to the surface of the glass, either by means of gum water or by thin paste. The mouth of the jar is furnished with a wooden cover which fits into the inside, and rests on the jar by means of a narrow projecting rim, which forms a shoulder. A stout brass wire passes through the centre of the cover, having a brass chain hanging from its lower extremity to the bottom of the jar, which is also covered with tinfoil, and in contact with the other part of the metallic lining. The upper part of the wire projects two or three inches above the cover, and is surmounted with a brass ball.

If I present the ball of the jar to the ball of the prime conductor, having hold of the coated part of the jar with my hand, a series of sparks are seen to pass between the two balls, from the conductor to the inner surface of the glass jar; but their size and frequency very soon diminish, and eventually they entirely disappear. When this happens, the jar is incapable of receiving any more fluid from the conductor, and is in that condition which we call *charged*. If now, whilst the jar is charged, any person were to touch the outside coating with one hand, and approach the ball on the top of the wire with the other, he would immediately experience a smart blow, or shock, which would affect him more or less accordingly with the size of the jar. This effect is known by the name of *Electric Shock*, and is one method of discharging the jar.

If, instead of one person making the communication between the outside and inside of the jar, a number of persons were to take hold of hands, so as to form a chain from the person at one end in connexion with the outside of the jar, to the person at the other end, who, by his hand, touched the ball at the top, the whole chain of persons would simultaneously receive the shock; but, in this case, the shock experienced by any individual, would be much feebler than if the same person were to discharge the jars himself from one hand to the other. But if a chain of persons were to take hold of a long wire, which reached from the outer coating of a charged jar to the ball at its top, the glass would be more effectually discharged than by the chain of persons alone, and they would be entirely free from the shock, showing that the electric fluid prefers traversing the best conductors in the circuit, which, in this case, was the metallic wire; for, although a portion of the fluid passed through every person in the arrangement, the quantity was so small as scarcely to be productive of any sensible effects. Hence it is, that we can discharge a jar with impunity through a metallic rod, held in the hand, having one of

its extremities in connexion with the metallic coating, and bringing the other extremity close to the brass ball. Any metallic wire employed for that purpose, is called a *discharging rod.*

Prize Volumes of the Annals of Electricity, &c.

The Prizes offered for volume seven, will be awarded to the successful competitors on the following subjects :—

1st.—For a description, with drawings, of the most powerful electro-magnet, exceeding a power of two tons. The iron may be of any shape or size, but is to be in one piece, and the armature also in one piece. The power signified is the power of sustaining a load by the force of attraction, whilst the magnet and armature are in close contact with each other. It is expected that the battery employed will be described.

2nd.—For a description of the most powerful *compound* electro-magnet, above a power of two tons. Conditions as in the first.

3rd.—For a voltaic battery of ten pairs, of the greatest decomposing power of acidulated water, with the least cost, including *price of battery*, cost of *exciting liquid*, and *time* necessary to keep it in good action. In short, the most economical battery for decomposing water for one hour continuously, without change of exciting liquid. The metals to expose not less than eighty square inches to the exciting liquid, and to be distributed in any manner the experimenter thinks proper.

4th.—For the best code of theoretical laws for the explanation of the production of *secondary, tertiary*, &c. Electric currents, with examples of application.

5th.—For the best set of experiments on the subject of atmospheric electrical waves ; with a description of their effects during the exhibition of lightning. The experiments are at the option of the competitors, but are expected to be made with very elevated conductors, either by means of a kite, balloon, tall buildings, or otherwise.

6th. —For the best paper on any novel experimental results in any branch of electricity or magnetism.

The Prize for each of the above subjects will be volume seven of the " Annals of Electricity, Magnetism, Chemistry, &c.," bound and gold-lettered in the first rate style, with a suitable emblem and motto. To be presented to the successful candidates, or to their agents, (in London, if required,) on the first day of February, 1842. The communications to be made on or before the 10th of December, 1841.

are others, which I shall shortly have to notice, wherein the differ-
ence of the electrical condition of bodies is not so easily illustrated
independently of direct experiment. I shall presently have to
show you that two distinct metallic bodies in their *natural electri-
cal condition*, are very far from being in *one and the same electrical
condition :* and I will further observe, in this place, that not only
are the various substances constituting the body of the earth in
different *relative* electrical conditions : but that, different parts of
the *same body* are also *differently electrical.*

I have long entertained this view of the electrical condition of
bodies when surrounded by equable electrical pressures, or when
they are in, what Franklin called, their natural electric states : and
I will lead you through a series of experiments, as we proceed in
these lectures, that will prove to you that this view is a correct
one, as far, at least, as those bodies on which we operate, are capa-
ble of rendering us the necessary information.

We will now proceed to an experiment which will not produce
very satisfactory results, unless we avail oursevles of the advan-
tages afforded by the condenser. We will take two metallic discs,
one of which is copper and the other zinc ; they may be about six
inches diameter, and each furnished with a glass handle in manner
of the cover of the electrophorus ; and their opposite sides should
be perfectly flat, so that when placed together, as shown in fig. 8,
those surfaces may be in contact through   z c   Fig. 8
out. Let us now bring the moveable
plate S, of the condenser, into contact
with the fixed plate P, fig. 7, and after-
wards separate them a little, so that we
can just see between them, and no more :
which is best done by having a sheet of
white paper lying on the table on the
opposite side of the electroscope to that
on which you are standing. We have   z o
now an exceedingly thin plate of air between the two plates of the
condenser, which will afford great facility for their polarization by
a feeble electric force.
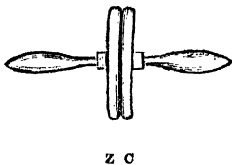
The electroscope with its condenser, and the zinc and copper
with their dry and warm handles being now ready we will
proceed to the experiment, which is to show that the zinc and
copper, though each in its natural electric condition, are not in
one and the same electric condition : but that the copper is
positively electrical with respect to the zinc, and that by their
simple contact alone, the copper will communicate to the zinc
a portion of its natural share of the electric fluid, so that the
zinc shall become positively electrical and the copper negatively
electrical ; not only with regard to each other, but with regard
to all surrounding bodies which are in their natural electric
state.

I now place the copper plate upon the palm of my left-hand with its smooth flat face upwards; and with my right I take up the zinc plate by its glass handle, and place it on the face of the copper one. I now separate the plates suddenly, and touch the cap of the electroscope with the zinc one, by which means I communicate a feeble electric force to the instrument. I bring the plate into contact with the copper one as before, and again separate them suddenly and communicate another portion of electric force to the electroscope; and by proceeding in this manner for about half a dozen times, I lay down the two discs and withdraw the plate S of the condensor and the gold leaves immediately diverge; in consequence of the electric fluid which was condensed in the plate P, whilst the plate S was close to it, being now nearly equally distributed over the whole of the metal in connexion with P: and as the gold leaves are portions of that metal, they receive their portion of the electric action and are repelled from each other accordingly.

Our next business is to ascertain what kind of electric action is possessed by the electroscope, and it is found to be positive, whether we test it by a negatively or by a positively electrized body. I am very anxious that the facts which this experiment develope should be very well understood : because I am well aware that many persons fail in producing any electric action whatever by these means, and others absolutely doubt the fact altogether. It was first shown by the celebrated M. Volta, and was the foundation of all that sound philosophical train of reasoning which led that eminent electrician to the invention and formation of the most formidable source of electric action that has hitherto been placed in the hands of philosophers, viz., the Voltaic battery, an implement of research which so justly bears his name, and by which the most important discoveries in this branch of science have been made.

We will now vary the experiment by placing the zinc disc on the hand, and with the other taking hold of the glass handle of the copper disc. But we must first adjust the plate S of the condensor to its proper distance from the plate P. Having now brought the face of the copper disc into contact with the face of the zinc one, I again separate them quickly and touch the cap of the electroscope with the copper disc, and after repeating this operation a few times as before, we shall find that the gold leaves diverge as soon as the plate S is withdrawn from the plate P, and by testing in the usual way, we find that the gold leaves are negatively electric ; proving all that I said, before we performed the experiment, respecting the development of electricity by the simple contact of metals : and that they are, naturally, in different electric states. It is in the investigation of these beautiful electrical niceties that we discover the superior penetrating genious of the genuine electrician, and distinguish

the philosopher from the mere iterant experimenter. The word VOLTA, will ever remain associated with the electrophorous, the condenser, and the famous battery which bears his illustrious name : and will be venerated by the electrician till this beautiful branch of science shall cease to be known.

When an electroscope is not furnished with a permanent condenser, the open hand held near the cap of the instrument will answer very well for many purposes. In this position the hand becomes a substitute for the uninsulated plate, and the flat horizontal disc which is insulated by the glass tube, in Singer's improved electroscope, forms the other. I will now show you an experiment which will convince you of the usefulness of the hand in this capacity.

I will take the cover of the electrophorous by its glass handle, and give the metallic disc a gentle rub on the table cloth or on the sleeve of my coat ; and whilst the other hand is held over the insulated disc of the electroscope I will touch that disc with the excited cover of the electrophorous, and immediately take it away again. The communicated electric action is very feeble, and the gold leaves will scarcely diverge : but now, that I take away my hand, you will observe that they separate to a considerable extent. This is precisely the result that we should obtain were we to use the metallic condensor; and in those delicate cases where the electric action is too feeble to deflect the gold leaves by one single contact of the excited body, we have only to repeat these feeble units of electric force, for a few times, as in the experiment with Volta's copper and zinc discs, and the sum of the feeble electric increments of force, collected whilst the hand is present, will be sufficient to diverge the gold leaves when it is withdrawn from the vicinity of the insulated disc of the electroscope. If I stand on a stool with glass legs, or be insulated by any other means, my hand has still the same kind of influence, though in a less degree than when uninsulated.

There is another method of showing the influence of vicinal uninsulated bodies, which I will now point out to you. Let us communicate, in the usual way, either kind of electric action to the electroscope whilst unfurnished with a condenser. The gold leaves diverge and remain divergent when the electrized body is withdrawn. I now bring my open hand over, and parallel to, the insulated disc of the electroscope. You will observe that the divergency lessens as my hand approaches the disc, and when it is sufficiently near the gold leaves collapse and hang together. But on withdrawing my hand gently, the divergency again commences, and gradually becomes greater till the hand is removed from the sphere of action, when the divergency is nearly the same as at first. Similar phenomena are produced by insulated plates of metal, though in a minor degree.

This is a beautiful experiment, and conveys a great deal of information which we ought to avail ourselves of in all those electroscopic inquiries in which the electric action is *to be tested*, and the electric action of the *testing body* are of different degrees of tension. Let me give you an experiment in illustration. I excite a feeble electric action in Volta's copper disc, by rubbing it against the sleeve of my coat, and I communicate a portion of this electric action to the electroscope, and the gold leaves are observed to diverge slightly. The electricity is negative. I now excite the tin plate of the electrophorus, to a considerable degree of power, by rubbing it on the fur side of a dry rabbit-skin ; and it will be observed, when I bring this tin disc over the cap of the electroscope, that the divergency of its gold leaves increases to a great extent. Being satisfied with this result, let us now reverse the process, by first communicating a powerful *electroscopic* action to the gold leaves, from the excited tin disc, and afterwards bringing the feebly excited copper plate over the face of the insulated disc of the electroscope. Now, although you are aware, by the former experiment, that both discs are in the same *electric* condition, (negative) with reference to the uninsulated group of things about the table, yet as they are positive and negative with regard to each other, the divergency of the gold leaves diminishes by the approach of the copper disc, which is the less formidable electrized body of the two. Hence experimenters unacquainted with this curious fact would be almost sure to be led into error from the indications afforded by the electroscope. And as this, I believe, is the first time that such a circumstance has appeared in print, it is highly probable that wrong conclusions may have been drawn by some of those who have studied these nice points of electric action. This interesting fact may be produced by one excited disc only,—the tin disc for instance. First, excite it on the fur to a considerable degree, and afterwards communicate a portion of its electric action to the electroscope. Again excite it very feebly, and then bring it over, and parallel to, the disc of the electroscope. The divergency of the gold leaves will diminish, and when the experiment is dexterously performed the divergency will be completely annihilated, until the feebly electrized plate be withdrawn from the vicinity of the other.